This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

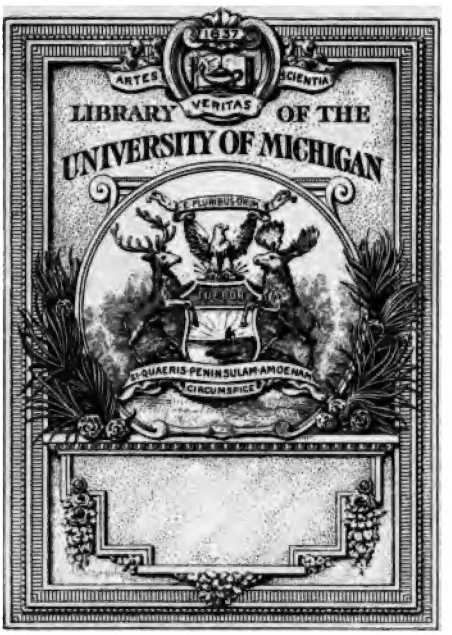
+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at http://books.google.com/

A 3 9015 00380 810 5
University of Michigan – BUHR


1837
ARTES
SCIENTIA
VERITAS
LIBRARY OF THE
UNIVERSITY OF MICHIGAN
E PLURIBUS UNUM
SI QUAERIS PENINSULAM AMOENAM
CIRCUMSPICE

THE

MEDICO-CHIRURGICAL

REVIEW,

AND

JOURNAL OF PRACTICAL MEDICINE.

VOLUME FIFTY-ONE.
APRIL 1, TO SEPTEMBER 30, 1847.

NEW-YORK:
REPUBLISHED BY R. AND G. S. WOOD,
NO. 261 PEARL STREET.
1847.

E. O. JENKINS, PRINTER,
114 Nassau Street.

CONTENTS

OF THE

MEDICO-CHIRURGICAL REVIEW,

JULY 1, 1847.

Reviews.

I.

I. Entwickelungsgeschichte des Hunde-Eies. Von. Th. L. W. Bischoff .
II. Zusatze zur Lehre vom Baue und von den Verrichtungen der Geschlechtsorgane. Von Ernst Heinrich Weber . . . . .
III. Ueber die Glandulæ Utriculares des Uterus des Menschen und ihren Antheil an der Bildung der Decidua. Von Th. Ludw. W. Bischoff . } 1
IV. Die Entwickelung des Menschen und des Huhnchens im Eie, &c. Von Dr. M. P. Erdi . . . . . .

1. Importance of Embryology 2
2. Component parts, &c. of the Ovum 3
3. Mode of Impregnation of the Ovum 6
4. Influence of the Semen 7
5. Time and Seat of Conception 8
6. Office of the Germinal Vesicles 9
7. Professor Weber on the Uterine Glands 11
8. Structure of the Placenta in the Dog 13
9. Dr. Weber on the Human Decidua 14
10. Professor Bischoff on the Human Decidua 15
11. Of the Decidua Reflexa 16
12. Professor Weber on the Vessels of the Placenta . . . 18
13. Circulation of the Placenta 21

II.

Traite Elementaire et Pratique de Pathologie Interne. Par A. Grisolle, M.D.P. 22

1. Fevers 24
   - A. Typhoid Fever 25
   - B. Typhus 28
   - C. Simple Intermittent Fever 31
   - D. Pernicious Intermittent Fevers 32
2. Inflammation—Hæmorrhages 33
3. Apoplexy—Poisons 35
4. Lesions of Nutrition 37
5. Accidental Productions foreign to the System . . . 40

III.

1. A Treatise on the Structure, Diseases, and Injuries of the Blood-Vessels. By Edwards S. Crisp, M.R.C.S. . . . . } 24
II. Observations on Aneurism and its Treatment by Compression. By O'Bryen Bellingham, M.D. . . . . }

CONTENTS.

X.

The Construction and Government of Lunatic Asylums and Hospitals for the Insane. By JOHN CONOLLY, M.D. . . . . .
Journal of Insanity. Vol. III. Utica, U.S. . . . . .
Third Annual Report of the Managers of the State Lunatic Asylum. By AMARIAH BRIGHAM, M.D. Albany, U.S. . . . . .
Twenty-second Report of the Officers of the Retreat for the Insane at Hartford, Conn., U.S. . . . . .
Report of the Medical Officers of the Lunatic Asylum for the County of Lancaster . . . . . .
1. Site and Construction of Asylums . . . . . .
2. Paucity of In-door Employments . . . . . .
3. Importance of Educated Attendants . . . . . .
4. Religious Services—Instruction . . . . . .

XI.

Treatise on Fractures in the Vicinity of Joints, and on certain Forms of Accidental and Congenital Dislocations. By ROBT. WM. SMITH, M.D.M.R.I.A. &c. . . . . .
1. Fracture of the Neck of the Femur . . . . . .
2. Fracture of the Lower end of the Radius . . . . . .
3. Fractures of the Humerus . . . . . .
4. Congenital Dislocation of the Shoulder . . . . . .
5. Dislocations of the Lower Jaw . . . . . .

XII.

ictures on the Comparative Anatomy of the Vertebrate Animals. By RICHARD OWEN, F.R.S. &c . . . . . .
1. Theory of the Cranial Vertebræ . . . . . .
2. Teleology of the Skeleton in Fishes . . . . . .

XIII.

ttres de Gui Patin. Par J. H. REVEILLE-PARISE, Docteur en Medecine, &c.

XIV.

Dictionary of Practical Medicine. By JAMES COPLAND, M.D. F.R.S. &c. Art. Hæmagastric Pestilence . . . . .
Report of a Special Committee of the House of Assembly of the State of New York, on the present Quarantine laws . . . . .
Report of the Fever at Boa Vista. By Dr. McWILLIAM . . .
Letter addressed by Sir William Pym to the Lords of the Council, relative to a Report on the Fever of Boa Vista, by Dr. McWilliam, R.N. .
1. The History of the Yellow Fever surrounded with difficulties .
2. Attempts to define it unsatisfactory . . . . . .
3. Its Resemblance to other Intertropical Fevers . . . . .
4. Nature and Source of the Black Vomit . . . . . .
5. Description of Malignant Remittent Fever . . . . . .
6. Malignant Remittent in Batavia . . . . . .
7. Dr. Copland's Views of the Ætiology of Yellow Fever . . .
8. Is Yellow Fever essentially Infectious? . . . . . .
9. Epidemic Visitations at Sierra Leone and Gibraltar . . . .
10. Influence of Sanitary Improvements at New York . . . .
11. Comparative immunity of Boston . . . . . .
12. The Fever on board the "Eclair" . . . . . .
13. The Yellow Fever as it appeared at Boa Vista . . . . .
14. Sir Wm. Pym's Observations on Dr. McWilliam's Report . . .
15. Quarantine and other preventive Measures . . . . .

XV.

ental Dynamics, or Groundwork of a Professional Education. By JOSEPH HENRY GREEN, F.R.S. &c. . . . . .

Recueil de Memoires de Medecine, de Chirurgie, et de Pharmacie Militaires. Vol. LXII. 49
1. Fragments from the Surgical Clinique of M. Sedillot . . 49
2. Several Cases of Congelation. By Dr. Shrimpton . . . 52
3 On the early Use of Injections in Gonorrhœa. By Dr. Poullain . 53
4. Frequency of Tænia in Algeria 57
5; Report of the Venereal Patients under the care of M. Marchal (de Calvi) at the Val de Grace. By M. Souhaut . . . 57

V.

Sullo Scorbuto Indagini, Osservazioni ed Esperienze di Carlo Novellis . 61
1. Apyretic or Chronic Scorbutus 63
2. Pathology of Scorbutus 63
3. Acute Scorbutus, or Scorbutic Synocha 64
4. Complications of Scorbutus 65
5. Curative Measures 67

VI.

Observations on the History and Treatment of Dysentery and its combinations. By William Harty, M.D. &c. 69
1. Simple Dysentery 72
2. Affinity between Dysentery and Rheumatism . . . 73
3. Combinations of Dysentery with Intermittent and Remittent Fevers 75
4. Combination of Dysentery with Typhus 79
5. Infectiousness of the Typhoid Form 85

VII.

On Wounds and Injuries of the Abdomen and Pelvis. By G. J. Guthrie, F.R.S. &c. 88
1. Ventral Hernia 90
2. Employment of Sutures 91
3. Management of the Protruded Parts 91
4. Wounds of the Intestine 92
5. Effusion of Blood into the Abdomen 95
6. Artificial Anus 95
7. Wounds of the other Viscera 95
8. Treatment of Injuries of the Abdomen 97

VIII.

Thoughts on the Nature and Treatment of several severe Diseases of the Human Body. By Edw. J. Seymour, M.D. F.R.S. &c. 97
1. Pain in the Stomach—Causes and Treatment . . . 98
2. Vomiting, its Causes and Mode of Treatment . . . 100
3. Gout, its Causes and Treatment 102
4. Psoriasis and Pompholyx in Gouty Habits 105
5. On the Treatment of Sciatica 106
6. Utility of Opiates in some Forms of Insanity . . . 109
7. Connexion between Melancholia and Phthisis . . . 113

IX.

Transactions of the Provincial Medical and Surgical Association. Vol. XV. 114
1. An Experimental Inquiry into the Effects of Hydrocyanic Acid, produced upon Animal Life. By Thomas Nunneley, Esq., F.R.C.S.E. 114
2. Observations on the Operation of Ovariotomy. By George Southam, Esq. 121
3. Observations on the Pathology of Abscess of the Heart. By T. H. Stallard, Esq. 122
4. An Essay, Literary and Practical, on Inversio-Uteri. By John Green Crosse, Esq. 124

X.

I. The Construction and Government of Lunatic Asylums and Hospitals for the Insane. By JOHN CONOLLY, M.D.
II. Journal of Insanity. Vol. III. Utica, U.S.
III. Third Annual Report of the Managers of the State Lunatic Asylum. By AMARIAH BRIGHAM, M.D. Albany, U.S. 128
IV. Twenty-second Report of the Officers of the Retreat for the Insane at Hartford, Conn., U.S.
V. Report of the Medical Officers of the Lunatic Asylum for the County of Lancaster
1. Site and Construction of Asylums . 129
2. Paucity of In-door Employments . 131
3. Importance of Educated Attendants . 132
4. Religious Services—Instruction . 134

XI.

A Treatise on Fractures in the Vicinity of Joints, and on certain Forms of Accidental and Congenital Dislocations. By ROBT. WM. SMITH, M.D.M.R.I.A. &c. . 137
1. Fracture of the Neck of the Femur . 137
2. Fracture of the Lower end of the Radius . 143
3. Fractures of the Humerus . 147
4. Congenital Dislocation of the Shoulder . 149
5. Dislocations of the Lower Jaw . 150

XII.

Lectures on the Comparative Anatomy of the Vertebrate Animals. By RICHARD OWEN, F.R.S. &c . 152
1. Theory of the Cranial Vertebræ . 153
2. Teleology of the Skeleton in Fishes . 166

XIII.

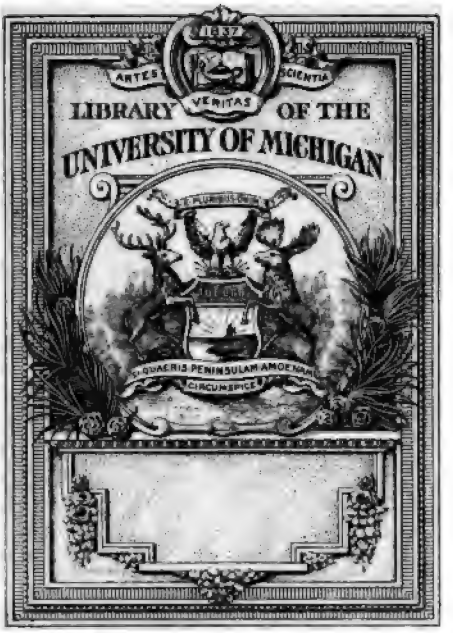
Lettres de Gui Patin. Par J. H. REVEILLE-PARISE, Docteur en Medecine, &c. 169

XIV.

I. Dictionary of Practical Medicine. By JAMES COPLAND, M.D. F.R.S. &c. Art. Hæmagastric Pestilence
II. Report of a Special Committee of the House of Assembly of the State of New York, on the present Quarantine laws 183
III. Report of the Fever at Boa Vista. By Dr. MCWILLIAM
IV. Letter addressed by Sir William Pym to the Lords of the Council, relative to a Report on the Fever of Boa Vista, by Dr. McWilliam, R.N.
1. The History of the Yellow Fever surrounded with difficulties . 185
2. Attempts to define it unsatisfactory . 187
3. Its Resemblance to other Intertropical Fevers . 189
4. Nature and Source of the Black Vomit . 192
5. Description of Malignant Remittent Fever . 194
6. Malignant Remittent in Batavia . 197
7. Dr. Copland's Views of the Ætiology of Yellow Fever . 202
8. Is Yellow Fever essentially Infectious? . 205
9. Epidemic Visitations at Sierra Leone and Gibraltar . 209
10. Influence of Sanitary Improvements at New York . 212
11. Comparative immunity of Boston . 215
12. The Fever on board the "Eclair" . 217
13. The Yellow Fever as it appeared at Boa Vista . 219
14. Sir Wm. Pym's Observations on Dr. McWilliam's Report . 230
15. Quarantine and other preventive Measures . 233

XV.

Mental Dynamics, or Groundwork of a Professional Education. By JOSEPH HENRY GREEN, F.R.S. &c. . 235


ARTES
SCIENTIA
VERITAS
LIBRARY OF THE
UNIVERSITY OF MICHIGAN
SI QUAERIS PENINSULAM AMOENAM
CIRCUMSPICE

at the expense of the yelk, which body furnishes the materials for the formation of the earliest and most fundamental parts of the germinal structures. It is remarkable that when Baer, repeating more carefully and skilfully the observations of Prevost and Dumas, first discovered the minute ovum of mammalia, he thought that this object corresponded to the germinal or Purkinjean vesicle of other animals, an error into which he could not have fallen, if the true office of the yelk, namely that of nourishing the germ in the first stages of its existence, had, at that time, been comprehended; so important is it, if it be wished to obtain a clear view of the whole process, scrupulously to investigate the very earliest changes that occur.

The constituent parts just noticed are then met with in the ovum of all animals, whilst it is still contained in the ovarium; and, as far as the germinal vesicle and its spot are concerned, there is tolerable uniformity of character throughout, the principal difference in the several classes depending on the relative size of the vitellus. Those who wish to satisfy themselves of this identity, may compare the small, greyish, and incipient eggs seen in the ovarium of birds with those of mammalia. Up to this point all is certain and undisputed; but in most animals, and especially in the ovipara, the ovum, after it has quitted the ovarium, receives, in its transit through the oviduct, and before it enters the homologous part to the uterus, certain additions, which are most apparent in birds. In this class it is well known that besides the chalazæ, which are evidently a mechanism peculiar to incubating animals, the upper part of the oviduct secretes the albumen or white, and the lower part, called isthmus, placed just above the uterus, the membrana testæ or chorion. We shall not pause to show that in many other oviparous classes both of these superadditions are made to the ovarian ovum, but proceed to consider what takes place in the case of mammals. Professor Owen, in his account of certain uterine ova which he examined in some pregnant ornithorhynchi, varying in diameter from $2\frac{1}{2}$ to $3\frac{1}{2}$ lines, states that there were two distinct membranes; the outer one is called the "chorion," and resembles the cortical membrane of the ovum of the salamander, except that it is of a more delicate texture; the inner membrane is regarded by Mr. Owen to be the membrana vitelli. Lying immediately beneath the latter, there was a thicker granular membrane, which is analogous to the blasto-derma or germinative membrane; careful examination showed that there was no decidua. It is also important to mention that, besides the yelk, which was of a yellow colour and quite distinct, a fluid matter was found between the cortical and vitelline membranes, thus occupying precisely the same space as the albumen in the egg of the fowl. This distinguished zoologist further remarks that the cortical membrane was certainly not the decidua, but that "it is certainly analogous to the outer tunic of the uterine ovum of the rabbit and bitch, which in them is gradually separated from the vitelline membrane by the imbibition of an albuminous fluid."

It is our own conviction that what is here proved of one of the lowest of the mammalia applies to the whole class. It is no slight indication that the Fallopian tube is a real secreting organ, and not a mere passage of transit, that its mucous membrane in all the mammalia in which we have examined it, including the human female, is highly elaborated, forming in the outer two-thirds of its extent, a multitude of projecting valves; it is

further found that this canal presents, in its uterine portion, a peculiarity of structure as marked as that seen in the isthmus of the fowl, when contrasted with the upper part of the oviduct. Developments and modifications like these must be associated with action and modification of function; and justify the inference that, in all mammals, the ovarian ovum receives, in its passage through the tube, in the first place an albuminous matter or white, and subsequently a distinct envelope, the chorion. In considering this question, it must not be overlooked that, amidst various modifications and much confusion, the best observers, from De Graaf to Baer and Martin Barry, have distinctly found the egg, previous to its attachment to the uterus, enclosed in two membranes, of which the outer is affirmed to be the chorion and the inner the yelk-membrane, or as it is called in mammals, the zona pellucida. The conclusion then at which we have arrived is, that in all essential particulars, the ovum of mammalia is identical with that of oviparous animals; an inference which is not only interesting in itself, but more especially because it gives an assurance that, in the subsequent development, a similar kind of identity will be preserved.

Having stated what we believe to be the view that is most in harmony with the general constitution and economy of the ovum, it is necessary to notice Dr. Bischoff's researches relative to this point. In his former work on the process of development in the rabbit, to which a prize was awarded by the Berlin Academy of Sciences, this excellent observer states that the ovum, in its passage through the Fallopian tube, receives a distinct albumen, but he contends that there is no cortical membrane or chorion surrounding it. In the work now before us, the Professor says, "the egg of the bitch, according to my observations, acquires no white either in the oviduct or in the uterus; this well-ascertained fact distinguishes the ovum of this animal very remarkably from that of the rabbit, which, as I have shown, receives a very distinct layer of white in the oviduct, and possesses it also, in the beginning, in the uterus. In the rabbit this matter soon disappears, since it becomes blended with the zona pellucida and with this represents the outer membrane of the egg, which, in the dog, is formed by the zona alone. It is therefore clear that no particular importance should be attached to this production of the albumen; and, therefore, that this last can have no essential influence upon the formation of the ovum and its coverings."—(*Entwickelungsgeschichte des Hunde-eies*, p. 69.) As regards the formation of the chorion, Dr. Bischoff says, in another place, it results from the junction or blending together of the yelk-membrane with the peripheral part of the animal layer of the germinal membrane.—(*L. c.*, p. 122.) We can only repeat our dissent from the correctness of this account, and especially that which attributes the chorion to the zona pellucida, which seems to be, like its homologous part in the bird's egg, the vitelline membrane, namely, merely a retentive bag, and not an active structure.

In proceeding to notice more particularly the work of Dr. Bischoff, we would in the first instance observe, that it is a treatise to which, at this particular time, great value must be attached; for, although much of the matter it contains is not novel, there are many facts recorded relating both to the earliest parts of the process, and to the subsequent changes taking

place in the uterus, which bear upon some of the most obscure but essential phenomena connected with the history of development, and which, at this time, engage a large share of attention of physiologists. In the second chapter the author enters upon one of these difficult questions, the mode, namely, in which the semen operates on the egg. He justly remarks, what a reference to the writings of the older physiologists will amply confirm, that until lately the greatest doubt still prevailed upon the most important questions relating to the impregnation of mammals. In the following passage this uncertainty is well stated:—"It was not known whether the formation and escape of the ova was or was not dependent upon coitus; whether the semen of the male played a material, and what part; whether this fluid penetrated to the ovary, and in that place fertilized the egg, or whether both first met in the uterus or in the oviduct; whether conception took place at the instant of connexion or subsequently; and what changes the ovum thereon experienced; all these questions were variously answered, and for the sufficient reason that there were no certain facts upon which to rest the reply."—(*L. c.* p. 13.) At the present moment most of these doubts have received their solution; and it is not useless to add that this might long since have been accomplished, if physiologists would have been content to receive information from the marked and ample phenomena displayed in the animal kingdom.

We need say nothing on the first question raised; because it is now ascertained that the maturation and discharge of the ova, are processes altogether independent of any influence communicated by the male; on this subject the researches of Raciborski, Negrier, Girdwood, Dr. Lee and others leave scarcely any room for doubt. With respect to the influence of the semen, the majority of our readers will recall the vague and frequently absurd theories, which some few years ago were put forth in works and lectures. And yet many accurate experimentalists, among whom Spallanzani, J. Hunter, Haighton, &c. may be mentioned, had distinctly proved that the act of impregnation depended on the material contact of the semen with the ova; a fact equally substantiated by the mode in which the egg is fertilized in fishes and batrachia by the direct application of the seminal fluid. But all these and many other exact observations were insufficient to subvert the old doctrines of the aura seminalis, general sympathy, and other equally vague notions; "it was in fact," to borrow the language of our author, "impossible to clear up all these involved questions, so long as the egg itself contained in the ovarium was not known; and since it has been discovered, no one has devoted a sufficiently careful and successful investigation to the subject."—(*L. c.* p. 13.) It is well known that Dr. M. Barry advanced the theory, that when an ovum was fitted for impregnation, it was carried up to the part of the Graafian follicle next to the outer surface of the ovary; that the vitelline membrane or the zona pellucida then in turn became attenuated, and that at length a fissure was formed in it, through which a spermatozoon penetrated to the germinal vesicle. Dr. Barry subsequently obtained the impregnated ova of a rabbit from the Fallopian tube, in which he supposed, as well as several other physiologists to whom the specimens were shown, that spermatozoa could be detected actually within the interior of the zona pellucida.

This view of the subject, which is supposed to be in harmony with the

known method of impregnation in the plant, where it is said the granules of pollen are conveyed down the style into the ovulum, has been to some extent received in this country; but according to Dr. Bischoff it is founded on a deception. He says, "I have stated in my work on the Development of the Rabbit, and have so depicted it, that the egg of that animal whilst in the Fallopian tube is covered with spermatozoa I have even often seen them moving actively." "I am now certain that Dr. Barry has mistaken spermatozoa situated *upon* the ovum, for the same bodies *within* it." Those who have themselves made observations upon ova taken from the tube, are aware of the great difficulty of deciding whether the spermatic animalculæ, which are, unquestionably, *in immediate contact with the egg*, are without or within the zona pellucida, and will therefore be inclined, for the present, to suspend their judgment upon this deeply interesting point of embryology. It is, however, most important to know that, whatever may be the solution of this question, it is well ascertained that the seminal fluid, as indicated by the presence of its animalcules, penetrates as far as to the ovarium, upon which it has been seen by Martin Barry and Bischoff in the rabbit. In the work of the latter, now before us, the following conclusions are given, after numerous observations, with respect to the dog:

"1. The semen penetrates at the instant of copulation through the os uteri into its cavity, and even as far as the furthest point of its horn.

"2. The semen in the subsequent hours penetrates also into the Fallopian tube, and can thus reach the surface of the ovary.

"The semen comes, in every instance, into material contact with the ova, and impregnation is effected by means of a material reciprocal action between the semen and the egg.

"Impregnation never occurs at the moment of copulation, but at a later time, to be hereafter specified."—*L. c.* p. 16.

Although there are no sufficient data at present, to enable the physiologist to ascertain the precise influence which the semen exerts on the ovum, it will still be useful briefly to consider this question, which however viewed is full of mystery. That the effect produced is most marked, is more particularly shown by the history of hybrids, and in a less striking degree by those physical peculiarities or resemblances which are impressed on the new being by the male parent: but on these points we do not propose to touch. There is no doubt that the semen operates upon the contents of the germinal vesicle, and whatever may be the subsequent (morphological) changes therein induced, the earliest ones seem to be of a chemical character; an inference which receives some support from the fact that, whereas the ovule of the plant prior to impregnation contains only starchy matter, it presents subsequently to that phenomenon another organic principle forming the walls of the newly-produced cells. This view corresponds with that advanced by Professor Bischoff, to the effect that the operation of the semen upon the egg is in the first instance of a chemical nature; and he further believes, with Valisneri, Bory St. Vincent, Valentin and others, that the object of the spermatozoa is to maintain by their motion the easily-changed composition of the semen.

As to the time and place of impregnation they are both subject to variation. The author concludes that, in the bitch, the ovum is capable of being

impregnated, during the whole of the interval occupied in its transmission from the ovarium as far as to the end of the Fallopian tube, that is to say, from six to eight days; but when the egg has reached the uterus, it is no longer susceptible of the influence of the semen, inasmuch as the process of development commences, after conception, in the lower part of the tube. In support of this position, it is an interesting fact that in all animals subject to periodical venereal excitement or *heat*, this entirely ceases when the ova have arrived at the uterus. With respect to the place of impregnation, the principal point to determine is whether it is possible for ova to be impregnated whilst in the ovarium. If the records of extra-uterine conception in the human subject are to be relied on, this may take place; for cases are referred to, among others, by J. Hunter, of ovarian pregnancy.* It must, however, be confessed that the excellent researches of Prevost and Dumas, joined to the now ascertained fact that the actual contact of the semen is required, have created a rather general belief that the ovum escapes from the ovarium in consequence of the act of copulation, and that the semen coming in contact with it, either in the tube or in the uterus, impregnation takes place in one or other of these organs, and nowhere else. This opinion has been more positively affirmed in a late work published by M. Pouchet (Theorie Positive de la Fécondation des Mammiferes, Paris, 1842). This writer says that physical obstacles are opposed in mammalia to the contact of the semen with ova still contained in the Graafian follicles; that assuredly ovarian pregnancies, properly so called, do not exist; and that, as fecundation in mammals, takes place normally in the uterus, abdominal and tubular pregnancies do not indicate that impregnation is effected normally in the ovary.

The observations of Dr. Bischoff on this question are so important and exact, that no apology is required for laying them before our readers in the author's own words. After stating that he has repeatedly found, in the rabbit and dog, spermatozoa in different parts of the Fallopina tube, between the fimbriæ, and upon the ovarium, this distinguished physiologist remarks:

"All theoretical objections are entirely disproved by these direct observations. I have also further shown that there is not the least impediment to the penetration of the spermatozoa into and entirely through the Fallopian tube, the movements of those bodies and the contraction of the tube itself being quite competent to effect such a result. Again, as to the assumed difficulty of the impregnation of an ovum whilst still in the ovarium, they do not exist. I have expressed my conviction that the fluid portion of the semen is the fructifying part, and that the smallest amount of it is sufficient to produce the effect. There is nothing therefore in the way to prevent the semen penetrating through the coverings of the ovary and of the Graafian follicle, till it reaches the ovum; especially if it be recollected that all these envelopes, at the moment when the

* Hunter admits three forms of extra-uterine conception, according to the situation of the fœtus, namely, ovarian, tubular, and abdominal; it is evident, however, that the latter is owing to the escape of the ovum by rupture or absorption from one or other of the former situations. Hunter justly remarks on the obscurity attending these cases. They are, he says, "extraordinary and seldom happen, and when they do occur, are often attended with so many hindrances to critical investigation, as hardly to allow of thorough or satisfactory information."

ovum is on the point of escaping, are thinned to the uttermost. It is, therefore, certain that eggs still in the ovarium can be impregnated; but this does not altogether prove the possibility of their becoming developed in the ovary, or of ovarian pregnancy, a phenomenon which seems to rest on incorrect and inaccurate observations."

He then adds that ovarian conception is, however, only the exception; for as the ova quit the ovarium, not in consequence of the influence of the semen, but of their being fully developed, and as in general this happens before the seminal fluid has had time to reach the ovary, the rule is that "*the ova and the semen meet together in the Fallopian tube, and that here impregnation takes place.*"—*L. c.* p. 30.

In this extract we have no doubt the phenomenon relating to the seat of impregnation are correctly set forth, excepting that it is with us a question, whether, in ovarian conception, the semen does not come into direct contact with the ovum, the Graafian follicle, whilst grasped by the fimbriæ of the tube, being as usual ruptured, but the egg, subsequently to the act of fertilization, being by some unknown cause retained within the ovi sac, and there becoming developed.

Although, as we have said, it is not our object to trace the process of development, it will be desirable to notice some of those parts of it concerning which at present there prevails much uncertainty. One of these relates to the first changes that ensue in the ovum consequent upon its fecundation; and this should be a subject neither for surprise nor discouragement when the minute size of the objects to be examined are recalled to mind, the ovum of the bitch having, when it quits the Graafian follicle, a diameter not exceeding the $\frac{1}{10}$ or $\frac{1}{13}$ of a Paris line, whilst that of the germinal vesicle is pretty constantly in the dog $\frac{1}{10}$, and of its macula $\frac{1}{210}$ of a line. Those of our readers who have kept up with the progress of modern embryology, will recollect that all investigators are agreed in attributing to the minute vesicula germinativa, and even to its spot or nucleus, some predominating influence in the first acts of development: it is even thought and apparently proved, that the basis of the embryo is produced by the direct operation of this fundamental element of the ovum. But, although there is this accord with reference to the supreme importance of the Purkinjean vesicle, there is the greatest discrepancy as to its mode of action. These conflicting views may, however, be referred to two hypotheses: according to one, and it is that which, resting on the deeply interesting researches of Dr. M. Barry, is rather generally adopted in this country, the vesicle receiving the direct influence of the semen (according to Barry a spermatozoon penetrates into it), takes on a complex series of cell-formations, determined especially by the reproductive powers of the macula germinativa, and which ends at length in forming the first rudiments of the germ, so that, in this theory, the vesicle is not only the determining cause of the primary acts of development, but also the seat of them, the yelk thus performing the merely subordinate part of furnishing by endosmose the crude nutritious matter.

According to the second, more generally received, and, as we believe, the true hypothesis, the vesicula either is ruptured or dissolved as the immediate and necessary result of a fruitful copulation; it thus disappears as a vesicle, though the macula or nucleus germinativus may remain intact,

and play a very active part in the subsequent evolutive process. This is the account of the matter to which Professor Bischoff inclines in this and his earlier works; he having always contended, sometimes we think with unjust asperity, against Dr. Barry's peculiar views. The series of actions consequent upon this rupture of the germinal vesicle has been variously regarded; but there is much concurrent testimony, and we allude herein more particularly to the earlier researches of Prevost and Dumas, to the subsequent ones of Rusconi, Baer, Rathke, and Siebold, and to the still later ones of Kölliker, Bagge, Vogt and others on the remarkable process called *the cleaving of the yelk*; there is we say much concurrent evidence to show that the way in which the contents of the vesicle conduce to development is by determining the subdivision of the vitellus into minute segments or globules which are ultimately converted into nucleated cells, and that these united together constitute the germinal membrane or true rudiment of the embryo. Thus it is found that, after impregnation, the yelk has in its centre a single cell (the first embryonic cell according to Kölliker): this cell next divides into two secondary cells, and at the same time, and apparently as a consequence, the yelk also breaks up into two halves; and this process of subdivision goes on in arithmetical progression, till at last a mulberry-shaped mass is produced lying of course within the zona pellucida or vitelline membrane. In the bitch, according to the author, the ovum at the end of the Fallopian tube has between 16 and 32 of these segments; and in the centre of each he found a very delicate vesicle, powerfully refracting the light.—(*L. c.* p. 44.) There has been much difference of opinion as to the exact relation which the segments of the yelk and the central bright vesicle have to the cell-formation; but, without dwelling upon this involved point of minute anatomy, it will suffice to state the general conclusions of Professor Bischoff on this subject. He says "these yelk-globules are not cells, but an agglomeration of yelk-granules without an envelope; each contains in its interior a transparent vesicle, similar to a fat vesicle, but without a nucleus. As to what determines this process of cleaving, and what is the source of the transparent vesicle, nothing is still certainly known; but it appears that the cleaving of the yelk and of its segments, is dependent upon these vesicles, and that these take their origin either from the germinal vesicle or from its nucleus." "In the uterus, the ovum has at first still the same appearance as in the oviduct, and the subdivision of the yelk still proceeds. But now the yelk-globules, continually diminishing in size, transform themselves into cells by becoming surrounded with a delicate membrane. The nuclei of these cells are the transparent vesicles placed in the centre of the globules. These cells arising out of the yelk-globules rapidly join together, and, their number still increasing, represent, by becoming flattened and blended together, a very delicate membrane lying close beneath the zona pellucida, which thus appears as a vesicle, called by me the *vesicula germinativa*."—*L. c.* p. 119, 120.

Such, then, are the earliest changes in the fecundated ovum, by means of which the first rudiment of the embryo is laid down in the form of a delicate cell tissue, commonly called the germinal membrane, but as we have just seen named by the author, germinal vesicle. We shall not pursue the subsequent changes of development, but proceed to notice what

has more interest for the medical practitioner—the mode of attachment of the ovum to the uterus, and the formation of the placenta.

There can be no hesitation in affirming that of all the branches of embryology, by far the most difficult in its investigation and the most involved in its description, is that which concerns the connexion existing between the uterus and the fœtus; and, notwithstanding the numerous and valuable contributions made of late years to this important subject, if we may judge from some of the publications that have appeared in this country, and from conversation with many of our professional brethren, very vague notions still prevail even among professed accoucheurs. The time, however, is arrived when the obscurity and contradictions in which the description of the placenta has been involved may be all but completely removed; and it will be our object, with the aid of the recent researches of Professors E. H. Weber and Bischoff, combined with those of other observers, to give a clear and succinct account of this matter. To do this it will, as in all similar cases, be necessary to consider the earliest changes in the uterus consequent upon conception, and the first relations established between that organ and the ovum. It is well known that, as the result of impregnation, there is formed in the uterus the production called *decidua;* and that, under this name two substances are comprised, one lining the inner surface of the uterus being called the decidua vera s. uteri, and another investing the ovum, call the decidua reflexa. Now, until the researches to which we shall immediately refer, these two productions were generally supposed to be both formed by coagulated lymph secreted by the mucous coat of the uterus; but it is a point of primary importance to know that this is not the fact. The two tunicæ deciduæ are perfectly distinct structures; one, the decidua vera, is nothing else than the *developed and thickened mucous membrane of the uterus;* whilst the other is unquestionably a *true secretion*, derived in all probability from the uterine glands. For the first positive step in the right direction, and by which a beginning was made for the reversal of the generally received Hunterian doctrine on the production of the decidua, we are certainly indebted to Professor E. H. Weber, who, in his edition of Hildebrant's Anatomy, minutely and accurately described in ruminants the glands of the mucous membrane of the uterus which he had already seen in the uterus of a woman, impregnated seven days previous to death, and which, on account of their form, he named "*glandulæ utriculares.*" He also especially pointed out the manner in which the cells of the chorion take up the yellowish fluid poured out by these flask-shaped glands.

The existence of these most important glands seems to be general among mammalia: thus they have been discovered by Eschricht in the porpoise and cat; in the rabbit by Weber; in the bitch by Dr. Sharpey; and in the human uterus by Professor Weber, and independently by Dr. Sharpey, to whose admirable description in Dr. Baly's translation of Müller's Physiology we have much pleasure in referring our readers. On the present occasion we shall principally avail ourselves of the recent observations of Professors Weber and Bischoff, which appeared in Müller's Archives for 1846.

Professor Weber states, after conception, the mucous membrane of the human uterus becomes soft, and, by degrees, attains a thickness of

two or three lines: *it is this structure which forms the tunica decidua uteri.* This change depends upon the increase partly of the vascular and partly of the non-vascular layer of this membrane or its epithelium; that is to say, the blood-vessels of the mucous membrane of the uterine glands become enlarged, and, at the same time, an abundant formation of white epithelial cells, partly nucleated, takes place. The utricular glands thus enlarged are two or three lines in length; they run like the gastric glands towards the inner or free surface of the mucous membrane, and there terminate by orifices, which have long been known as existing in the tunica decidua, to which they give a cribriform appearance.

In the dog and cat the uterine glands, which in these animals are of two kinds, simple and large or branched glands, enlarge themselves in like manner at the place where the placenta subsequently appears; the smaller ones in their whole length, but the larger branched ones only towards their orifices, where they become dilated into a number of sacks or pouches, around the walls of which the uterine blood-vessels are dispersed, and which, as they enlarge, push as it were against the lining membrane, carry this before them in a complex manner, and thus become enveloped by it in the same way as the large intestine is covered by the peritoneum.

The fact that the decidua vera is composed actually of the developed uterine glands, and not, as the Hunterian doctrine taught, of an exuded fibrine, must now be regarded as firmly established; and this, which is the clue to the whole formation of the placenta, requires us in the next place to consider what is the nature of the connexion established between the outer membrane of the ovum, the chorion, and the so-called deciduous membrane. In the ruminantia there is apparently afforded the type of what, under different modifications, takes place in all mammalia: in these animals it is found that the tufts and villi of the chorion are received into the glandular tubular canals of the maternal cotyledons just as the fingers are introduced into a glove. The connexion consists of intimate contact, not of union, and so the tufts of the fœtal cotyledons can be easily drawn out—in the gravid uterus of the cow, for example—from the sheath of the maternal ones, when it is seen that they were bathed in a milky kind of liquid. A similar relation, in principle, has been detected by Drs. Sharpey and Weber in the bitch; the latter thus describes it. "The tufts of the chorion, which carry the embryonic capillary vascular plexus of the umbilical vessels, grow, as it were, into the expanded openings of the uterine glands; fill out the expanded part of these glandular flasks; and, insinuating themselves into all their folds and tufts, grow with them, and thus form together a particular membrane, which alone possesses embryonic vessels. In this process of development, the part of the membrane derived from the walls of the uterine glands becomes probably thinned by absorption." This important and elaborate process is confined to the dilated pouches of the uterine glands, as the tufts of the chorion do not seem to penetrate into their tubular portion, nor into their cæcal extremities.

The exact structure of the zonular placenta of the bitch produced in the way just described, is thus explained:—the whole placenta is penetrated or traversed throughout by a coarse network of tortuous capillary vessels, carrying the maternal blood, and having the very large diameter of

from about $\frac{1}{12}$ to $\frac{1}{23}$ Paris line; these vessels are each of them overlaid or covered by a membrane, which gives transmission to a very rich network of extremely minute embryonic vessels, the diameter of which being $\frac{1}{173}$ to $\frac{1}{234}$ of a Paris line, is three times smaller (giving an area nine times smaller) than that of the maternal blood-vessels which are drawn over them. Thus it happens that the embryonic blood contained in rich vascular retia, flows over the surface of the large canals carrying the maternal blood, but evidently in such a manner that the two kinds of vessels and their contents do not communicate together; they therefore only come into indirect but most extensive relation with each other, the disposition being similar to that of the small branches of the air tubes with the rich networks of the pulmonary capillaries displayed over them.—*Müller's Archiv.*

These investigations into the structure of the placenta in the dog are so important in themselves, and supply to many of the voids at present existing in the human formation, that we are tempted to extract the very clear account given by Professor Bischoff as the result of his researches.

"Until the ovum has acquired the size of from 2 to 2½ lines, and when it has still no villi on its surface, it lies quite free in the uterus, and no other change is seen in the mucous membrane of the latter organ, except that it is more vascular, turgescent and villous than usual. But when the ovum has attained the above size, the mucous membrane, at the place where the placenta is subsequently formed, rapidly develops itself, and soon forms a distinct elevation on its inner surface. If the free surface of this be now carefully oberved, there will be seen, even with the naked eye, a great number of small apertures, and soon when the ovum with its tuft and the above elevation have increased in size, we can perceive that the tufts of the chorion project into these apertures: (this disposition is beautifully illustrated in the accompanying plates, fig. 48, A.) In the beginning, the tufts of the chorion can, after a slight maceration, be readily drawn out of the apertures; in a short time, however, this cannot be so easily effected, but" (and to this point we would direct the particular attention of our readers) "we can now much more readily accomplish a separation of the whole swollen part of the mucous membrane of the uterus, which remains attached to the ovum; the separation of this layer takes place so much the easier as the egg is more developed. Thus is formed the placenta of the dog. If a section of the mucous membrane be made at the zonular swollen place above described, it will be found that the swelling consists partly of a succulent infiltration of the whole tissue, but more especially of the above described uterine glands greatly developed: the small apertures noticed above as being seen on the free surface are the orifices of these glands, into which sink the tufts of the chorion."

Although the subsequent changes are difficult to decipher with accuracy there is no room to doubt that they consist essentially of the continued progress of the same disposition; that is to say, the uterine glands increase more and more, and with them the tufts of the chorion, which, sending off numerous lateral branches and offsets, project into them as into sheaths. The umbilical blood vessels of the fœtus ramify in the tufts, the arteries passing over into the veins in loops; and in a similar manner the blood-vessels of the mother expand themselves between the uterine glands, a network of capillaries forming the connexion between the uterine arteries and veins. The whole forms the zonular placenta, in which therefore can be distinguished a maternal part, formed especially of the greatly developed uterine glands and uterine vessels; and of a fœtal part composed of the tufts of the chorion and the umbilical vessels ramifying

within them. The maternal and fœtal blood pass by or over each other in capillary streams, but without actually intermingling; the tufts of the chorion, it is to be observed, are not sunk into the sinuses of the uterine veins, but into the tortuous and greatly enlarged uterine glands. Both parts of the placenta are subsequently so intimately blended together, without, however, any departure from the fundamental arrangement, that they cannot be separated; but the maternal part may be detached by laceration of the connecting vessels and fibres of the developed part of the uterine glands; in fact, the uterus in this place is stripped of its membrane, which, with its glands, is afterwards regenerated.—(*Entwickelungsgeschichte*, &c. pp. 114, 115).

In this discription, we have directed attention to the important circumstance that, after development has advanced to a certain extent, the decidua uteri readily tears off. This explains what happens in the case of human gestation, and especially shows how the decidua uteri may be raised in dissection from the uterus, as if it were a layer distinct from the mucous membrane.

In proceeding to the investigation of the human placenta, there is doubtless much more difficulty in analyzing its minute structure, though the main facts are fully determined. There are some important points in which it strongly contrasts with the placenta of mammalia; and we are ourselves of opinion that, taking these differences into consideration, it may be predicted that ultimately the mode of formation and constitution will be found to be essentially the same in man and animals. The principal differences to which we allude are these: that in the human female there is an additional element concerned, the decidua reflexa, which has no certain existence in animals; and secondly, that in man, the fœtus is larger than in any animal having a single placenta, that has hitherto been investigated, a circumstance which must modify the character of the placenta, though it may not alter its essential constitution. In the valuable contribution to which we have alluded, Professor Weber mentions several circumstances, from which he seems to doubt whether the human placenta is formed according to the same principles as that of the cat and dog; the most important of these circumstances we shall now quote, both because they bear upon the point in question and also because they convey several important facts connected with the structure of the placenta. The peculiar enlargement of the ducts of the uterine glands into the wide and folded pouches already described, has not yet been seen by Dr. Weber in the human female; nor in the gravid uterus of the tenth week could he discover that the branched tufts of the chorion had pushed into the openings of the utricular glands; and moreover the simple figure of the utriculi of the human uterine glands did not correspond to the infinitely ramified and divided tufts of the chorion. It cannot, therefore, be assumed as proved, that in man the tufts of the chorion grow into the utriculi of the uterine glands, as they certainly do in the dog. Again, as man is distinguished by exclusively having a decidua reflexa, so a difference may exist as to the nature and formation of the placenta. Several other subordinate distinctions are also pointed out; such as, that the placenta uterina of the human female is distinguished in this—that the coarse vascular rete carrying the maternal blood, and which penetrates through the whole placenta, consists

of canals having a diameter fifteen times larger than the capillaries that bring the maternal blood into the placenta of the dog, whilst the walls are much thinner: that the other element of the placenta, the tufts of the chorion, carrying a very rich rete of minute embryonic capillary vessels, form in the dog membranes and folds, in man, on the contrary, small trees with cylindrical branches and twigs; that in the human placenta there are, in the usual sense of the word, no capillary vessels, but only vessels of $\frac{1}{4}$ to $\frac{3}{4}$ of a line in diameter (colossal capillaries or veins), and that the arterial branches $\frac{1}{4}$ to $\frac{3}{4}$ line in diameter, which conduct the maternal blood out of the uterus, into the placenta, do not divide themselves repeatedly into branches, but form, in their passage into the placenta, a coil (*glomus arteriosus*), which consists of a single artery twisted here and there, and which at length prolongs itself directly into the network of those colossal capillary vessels or veins which penetrate through the whole placenta: lastly, that while it happens both in man and in the dog that the vessels carrying the maternal blood come into intimate contact with those carrying the embryonic blood, yet, in the latter, it happens that the vessels carrying the maternal blood into the placenta are individually developed between the membranes and folds of the tufts of the chorion, and are covered by them; in the former, on the contrary, the branches and fibres of the tufts of the chorion are covered by and developed in the walls of the very wide and thin-walled maternal blood-vessels, which, filling up the intervals between them, insinuate themselves on and so surround the tufts. Professor Weber finishes this summary by remarking that, if it should be shown hereafter that the tufts of the chorion in man, as in the dog, grow into the utriculi of the uterine glands, and thus fill them up, it would thence follow that the branches and terminal fibres of these tufts would acquire a thin and adherent covering, derived from the walls of the uterine glands.—*Müller's Archives*, 1846, p. 424.

In opposition to the objections set forth by Weber, but evidently without any certain conviction of their truth, we would again refer our readers to the account of Dr. Sharpey, and especially to the figures illustrative of the uterine glands of the bitch, and of the same organs in the human subject. We shall also now append the most recent views of Professor Bischoff on this important point. In the work before alluded to (*Müller's Archives* for 1846, p. 111), a very instructive case of early pregnancy is related by this admirable observer.

From the facts stated, there is no doubt that impregnation had occurred within 14 or 21 days before death. The case was that of a married woman, aged 31, who had borne eight children, and who destroyed herself by drowning.

"The inner surface of the cavity of the uterus had an appearance quite different from the usual one, and which became very distinct when the organ was placed in water; it had, namely, a very delicate and apparently tufted condition, and this was particularly distinct upon the cut borders. The surface itself, if viewed from above, appeared as if finely perforated; or rather, as if closely covered or beset with small white points, which, upon the section, were perceptible as the free ends of the apparently white tufts. These little tufts, however, were not such in reality; for, firstly, they were all connected together by means of a semi-transparent mass; and further, it was easy for a person expe-

rienced in these investigations to perceive, by employing magnifying powers, that these apparent tufts were small glandular canals or pouches (*drüsenschlauche*) 1½ to 2 Paris lines long. The analogous formations of the dog, the cow, the sow, &c. being known, I could not remain in any doubt, their nature was apparent at the first glance—they were the same uterine glandular canals which I had sought for in vain in the unimpregnated condition (of the human uterus), but which here showed themselves quite clearly and distinctly, even to the naked eye." "Now, as I have already entirely convinced myself that in the dog (herein agreeing with Sharpey) these glandular canals, expanded into flask-shaped pouches, take up the tufts of the chorion, to form together with these in the rapid progress of development, the so-called decidua in the bitch's ovum, so I hold it to be no longer doubtful that, in the human female, a precisely similar disposition takes place. These glands seem to be very small and indistinct in the unimpregnated state; but after conception they grow rapidly, and an exudation also occurring from the surface of the uterus, they in a manner grow into this exudation, and the two together then form the decidua vera; and, at that part where, owing to the application of the allantois, the tufts of the chorion become further developed, the placenta. The decidua, therefore, is in fact, if not exactly the membrana, at least the stratum uteri internum evolutum; and, as such, a formation is present, partly as a product of development and partly as a new formation. At birth, a real casting off of the inner (mucous) layer of the uterus takes place; but it is to be presumed that the fundamental part of the glandular canals, namely, their cæcal extremities, which project against the fibrous coat, are left behind."—*L. c.*

In this account there is one circumstance to which attention should be particularly directed, as it will explain one of the many sources of error by which the formation of the decidua vera has become so involved and obscured. We allude to the circumstance, that when the uterine glands become enlarged, they give to the free surface of the mucous membrane of the uterus, what would not have been expected *à priori*, a peculiar *tufted appearance*, and which has consequently been often mistaken for true villi. For the rest, it is very satisfactory to find the important researches of Weber and Sharpey concerning the uterine glands, thus confirmed by an anatomist, who probably, owing to his extended and successful researches, was the best qualified in Europe to offer an opinion.

There is still some uncertainty concerning the nature of the decidua reflexa; this is of the less consequence, as it is altogether a secondary part, and is absent in animals. We have already said that it has an origin totally distinct from that of the decidua vera. Dr. Sharpey suggests, without wishing to affirm anything positively, that it may consist of exuded lymph, which covers the minute ovum on its entrance into the uterus, either entirely or on that part of its surface which does not adhere to the uterus. Mr. Goodsir, who has satisfied himself that the decidua reflexa is a distinct formation, offers an explanation of its origin in accordance with his views of the process of nutritive absorption in the alimentary canal; namely, that this covering, which he proposes therefore to call the *cellular* decidua, is formed of cells passing off from the uterine glands, and which surround the ovum when it enters the cavity of the uterus. Connected with this view of the origin of the d. reflexa, he has a theory that this structure, by the powers of its active cells, nourishes the ovum after this has exhausted the supply provided in the ovary, and that it thus performs the same part in the gestation of the mammal, which, in the egg of the oviparous animal, is effected by the albumen.

In the plates illustrative of development published by Dr. Erdl, there is a scheme representing the formation of the decidua reflexa (*Erster Band, Zweiter Theil*, Tab. 2. fig. 8), which is altogether too mechanical; and in the brief description of the plates, the author gives what is certainly an erroneous account of the production of the above layer, inasmuch as he states it to be nothing else than the prolongation over the ovum of that part of the decidua uteri which he affirms, but again erroneously, extended, prior to the descent of the ovum, over the orifice of the Fallopian tube. Upon this point Dr. Bischoff properly remarks, that as we are speaking of a most minute body, of the 1-12th of a line in diameter, all mechanical explanations of the way in which the ovulum becomes covered by the decidua reflexa, must fail of necessity; he himself considers the question to be undecided, and to require further research.

We have been led further into detail than we had designed; but as no true or clear idea of the structure of the placenta can be formed, without the relations of the ovum and uterus are definitely made out, it is hoped this consideration will prove a sufficient apology for the extent of the foregoing observations. Those who have carefully followed the account now given, will find a cloud of doubts and difficulties removed; and by comparing especially the critical observations of Weber upon the reciprocal relations of the various maternal and embryonic vessels and membranes contained in the placenta, with the accounts given by those writers, who have discovered many isolated facts connected with this body, often, however, mixed up with error, much that before was confused, apparently even contradictory, and therefore most unsatisfactory, will become clear and comprehensive. The account of J. Hunter is certainly the most important of all the older histories of the placenta; and but for one pervading error, would probably long since have conducted his successors to the true anatomy. The error to which we allude is that Hunter, led away by his great principle concerning the effusion and organization of fibrin, regarded the decidua (both the vera and reflexa) as formed by effusion of coagulable lymph, and which, like the effused fibrin poured out into any of the cavities of the body consequent upon the introduction of an extraneous living part, a comparison made by Hunter, became subsequently organized and vascular by the prolongation of vessels from the inner surface of the uterus.* Another error also occurs in this account, namely, that the ma-

* It is interesting to compare the account given by Hunter of the first changes induced in the uterus as the result of conception, with the late researches which we have noticed above. In a case where impregnation had occurred within one month before death, the inner surface was found covered with a pulpy substance in its thickest part forming a layer $\frac{1}{2}$ line thick, and "evidently formed by coagulated blood;" this substance was continued across the cervix, thus closing the cavity of the uterus, and also extended some way into the Fallopian tubes. "When the inner surface of the cavity of the uterus was examined with a magnifying power, it was found extremely vascular and dotted with innumerable whitish spots, too small to be seen by the naked eye." There is no doubt that these "whitish spots" were the orifices of the enlarged uterine glands filled with an abundant epithelium. We have quoted the authority and description of John Hunter, without at all alluding to the dispute between him and Dr. W. Hunter as to priority of discovery.

ternal blood carried into the placenta by the curling arteries was there deposited in cells, from which it was said to be returned by channels ultimately leading into the uterine sinuses. This view was finally adopted by Mr. Owen, after he had for a time received as correct Dr. R. Lee's account. He found, on examining a gravid uterus of the ninth month, that the uterine veins were continuous with wide channels, passing obliquely through the decidua into the placenta, which "decidual canals," as they are here called, became diffused through the fine spongy and cellular substance which every where surrounds and supports the fœtal capillaries, and at length penetrated to the surface of the placenta. This distinguished physiologist concluded that "the placental intercommunication between the fœtus and the mother of the human subject and the quadrumana, is carried on by by the contact of the fœtal capillaries with maternal extravasated blood; while in ruminants, the mare and the sow, it takes place by the apposition of capillaries to capillaries, and the two parts of the placenta, namely, the fœtal and maternal, can be separated. In the feræ and rodentia there appears to be an intermediate structure."

The first correct view of the vascular arrangement of the placenta was taken by E. H. Weber, so long ago as 1832; and as the account given by that admirable anatomist is in itself essentially true, and has laid the foundation of all later researches, we are desirous of briefly submitting it to the notice of our readers. He first accurately describes the entrance of the uterine arteries and veins into the placenta, by passing obliquely through the decidua vera, thus confirming the fundamental fact ascertained by Hunter, but which has at various times been called in question. He notices the fact that, the coats of these maternal vessels become, on entering, extremely thin, and are therefore easily lacerated. The true relations existing between the maternal and the fœtal portions of the placenta are then, for the first time thus accurately determined:—"The fœtal part of the placenta consists, in man, of the multitudinous arborescent tufts of the chorion, which project into the canals of the widely-dilated veins filled with the maternal blood; these latter vessels pass from the inner surface of the uterus through the decidua vera, and penetrate throughout between the tufts of the fœtal portion of the placenta, and thus form a large network." After pointing out the appearances that doubtless deceived Hunter and his successors, as to the existence of *cells*, namely, that the uterine veins, where lodged between the tufts of the chorion, losing their cylindrical form, owing partly to the extreme tenuity of their walls and partly to their disposition, have the aspect rather of interstices and passages than of veins. Professor Weber thus explains the true structure: "it occasionally happens, however, that we are so fortunate as to find a place where it can be seen in what manner the veins are related to the tufts. At the borders, namely, of the placenta and sometimes in its substance, entering uterine veins are found, into the interior or cavities of which a small tuft of the fœtal portion of the placenta here and there projects, whilst the vein still has, in other respects, entirely the characters of a definitely limited canal with smooth internal parietes. We can in such places convince ourselves, that these tufts of the placenta fœtalis, thus projecting into the interior of the veins, do not pass through an actual perforation in the vein, but that the inner extremely thin venous membrane at the place where the tuft pushes

itself in, is carried before it into the cavity of the vein, and that this projected part (of the lining membrane) is carried over the tuft and each of its individual fringes; or, in other words, that each tuft projecting into the vein fills up the pushed-forward, inner venous membrane, and that in this manner this latter is carried over the fringes of the tufts."

After again insisting upon these relations of the maternal and fœtal portions of the placenta, and precisely defining the whole structure, Weber gives the following summary of the *modus operandi* of this complex organization.

"The physiological actions of the human placenta appear then to consist in this—that the large stream of the embryonic blood is so conducted over the still larger current of the maternal blood, as that every blood-corpuscle of the fœtus, whilst it circulates through the placenta, comes into a very intimate but indirect contact with the blood of the mother for some considerable time. This is effected in consequence of the blood-stream of the embryo being so divided by traversing an infinite number of minute canals, that only a single series of corpuscles can pass along, whilst the maternal stream flows in very wide thin-walled passages, into which the tufts of the fœtal placenta project, and are bathed by the maternal blood flowing over them. Now, since the blood of the fœtus flows through the fine, hair-like extremities of these tufts, it may be presumed that it can attract through the extremely thin and moist parietes of the fine and elongated capillary vessels, certain substances contained in the maternal blood; and, on the other hand, that the maternal current of blood can doubtless also effect, through the attenuated parietes of its containing canals, an attraction or absorption of certain substances existing in the fœtal blood.—(*E. H. Weber's edition of Hildebrant's Hand. der Anat. Band,* IV. p. 496-499.)

The researches we have thus curiously explained, illustrate so perfectly and minutely the intimate texture of the placenta, that, combined with the equally important and original observations of the same author upon the uterine glands and the arrangement of the capillaries of the umbilical blood-vessels, they have left little more to be determined. They entirely explode the Hunterian doctrine of the maternal blood being extravasated or deposited by the curling arteries in the so-called "cells" of the placenta; they reveal the exact disposition of the chorion-tufts and of their contained blood-vessels; they do away with all the vague ideas of vessels with open mouths; they disprove the agency of lymphatics; and, finally, they explain the physiological phenomena connected with the introduction of nutritious matter into the system of the embryo. The term "cells of the placenta," indicating thereby extra-vascular cavities, has been so frequently used in descriptions of this organ, and in this country even of late years by writers who are looked up to as high authorities in this branch of medical science, that it is desirable distinctly to state that no such "cells" exist. It may be affirmed as a fixed law of the circulation, that the blood never quits its containing channels *en masse;* that is to say, it is never extravasated. The principal cause of the errors which have so constantly prevailed upon this point among writers unacquainted with microscopic and structural anatomy, are firstly, that where blood-vessels, and especially the veins, are either removed from external pressure, as in the case of the bones, or are connected with functions demanding a ready passage to and from the contained blood, their parietes become extremely attenuated, and are therefore often overlooked; and, secondly, that when such vessels are

artificially injected, especially when this is rudely done, as is usually the case, their delicate membranes are torn through, and a deceptive appearance of cells is thus produced.

The later inquiries of Dr. Reid and Mr. Goodsir have confirmed the account of Weber, and have thrown additional light on the minute relations of the maternal and fœtal vessels and membranes. The observations of the latter gentleman are particularly interesting; and although several of his physiological inferences are to a great extent speculative, his paper should be carefully considered by all who desire to become thoroughly acquainted with the subject to which it relates.—*See Med. Chir. Rev.*, July, 1845.

As additional evidence of the correctness of Professor Weber's views is still desirable, we may take this opportunity of stating that, we have lately had an opportunity of examining two gravid uteri of about the fifth and sixth month respectively, and in which the placentæ having been previously minutely injected both from the maternal and fœtal vessels, were favourably displayed. We were thus enabled distinctly to follow the uterine veins into the placenta, the passage taking place opposite to the interlobular spaces. Where quitting the fibrous coat of the uterus just at the point of entrance into the placenta, the walls of these enlarged veins become extremely thin and most liable to laceration, a circumstance which is doubtless a principal cause of the uncertainty that has prevailed upon this disposition. Just where the uterine veins had got to the placenta, tufts of the chorion were seen projecting into their cavities, as described by Weber and Reid; but no tufts were in either case seen pushing into the veins of the walls of the uterus, a disposition noticed by the latter observer. An injection of size having been employed, the curling uterine arteries could not be traced very satisfactorily into the placenta; but it was yet evident that at several points this transit took place. The maternal vessels having been filled in one of the specimens with red and the fœtal vessels with a yellow injection, the two orders of tubes could be distinctly followed; and in this way it was found, what other observers have noticed, that the uterine vessels penetrated completely to the fœtal surface of the placenta, and the umbilical vessels, as above stated, as far as to where the placenta came in contact with the fibrous coat of the uterus. A clean vertical section through the fundus uteri, displayed very beautifully the remarkable mechanism of the uterine veins for preventing hæmorrhage on the separation of the placenta; the free inter-communications of these vessels being provided with valve-like processes of the fibrous substance of the uterus, produce the same effect as if each of the uterine veins had been divided into a numerous series of chambers provided with muscular, contractile valves, and thus capable of being completely closed. This disposition becomes more and more developed, as the veins approach the inner surface of the uterus, or the place where, in parturition, they are torn across; and it thus happens, as Professor Owen has so well described, that on the forcible contraction of the organ, subsequent to the separation of the placenta, the whole of this complex valvular apparatus comes into play, and, by compressing the enormously dilated and now ruptured veins, effectively guards against the fearful hæmorrhage which must otherwise have taken place. It is further evident, that although this interesting arrangement is sufficient to prevent the loss of blood when the

whole of it is called into action on the normal detachment of the placenta, that if a partial separation of this organ takes place in the course of gestation without delivery following, the uterus not contracting, profuse hæmorrhage must occur.

All this shows that the contraction of the uterus is the essential means employed by Nature to prevent the loss of blood on the separation of the placenta, so far as the uterine veins are concerned. The mode in which arterial hæmorrhage is guarded against, is not so well understood. The forcible contraction of the muscular uterine walls is doubtless one cause; and probably the remarkable coils described by the uterine arteries at the place of their entrance into the placenta, by inducing an elastic retraction after laceration, may by aiding the other known means adopted by nature in the spontaneous suppression of hæmorrhage, be another influential circumstance, especially as regards vessels of the moderate magnitude of the "curling arteries."

The exact mode in which these last-named arteries end in the commencing branches of the uterine veins implanted in the placenta, is not known; but it cannot be doubted there is a direct continuity between the two sets of vessels. It is also certain that the arrangement and character both of the uterine and umbilical blood-vessels are such as to retard, in a remarkable degree, the velocity of the circulation; in the former vessels, owing to the great magnitude of the veins or sinuses as they are called; and in the latter, in consequence of the very interesting disposition of the capillaries lodged within the tufts of the chorion, and so admirably described and defined by Professor E. H. Weber, and, independently, by Mr. Dalrymple. The principal peculiarity of these umbilical capillaries is their great number, in proportion to the stems with which they are connected; a disposition which has an influence in diminishing the rapidity of the blood's current, both by enlarging the aggregate space through which it flows in passing from the arteries into the capillaries, and also by increasing the amount of friction owing to the minute size of the individual channels. It will not be superfluous to state, that the mechanical arrangement of the uterine arteries penetrating the convex surface of the placenta, and of the umbilical arteries entering by the cancave surface of that organ, both being very much coiled, especially the former, is such as must further concur in retarding the placental circulation.

The general result of all these elaborate arrangements is to allow time for that important reciprocal interchange of materials between the maternal and fœtal blood, which has already been alluded to, and on which the nourishment of the fœtus depends.

The large divisions of the uterine sinuses lodged between the lobes of the placenta communicate together, as can be proved by tracing them and also by inflation; there is therefore a disposition which, independently of any other circumstances, would tend to cause hæmorrhage from the substance of the placenta in cases of partial separation of that organ.

We have inserted the title of Dr. Erdl's work at the head of this article, not with an intention of noticing it on the present occasion, but for the purpose of calling the attention of our readers to the fact of its publication. It consists of a numerous series of plates illustrative of the development of the chick and human embryo, with brief explanations; and although

we think that some of the drawings might have been more happily designed and executed, and that the descriptions should have been given in Latin as well as German, yet, upon the whole, it is a very useful work, and we therefore recommend it to our readers, and especially to medical societies, as a desirable addition to their libraries. It is, however, necessary to state that at present the first volume only, consisting of two parts, has been published; the whole work is to consist of two volumes, and will, there is no doubt, be speedily completed.

The objects of Dr. Erdl's publication will be gathered from the following extract. "Under this title (Die Entwickelung des Menschen, &c.), I present to students, to teachers, and to those who are devoted to scientific pursuits, a work which is designed to supply a defect of some moment in physiological literature. My object is to give, as accurately as possible, figures of the various stages of development of the human embryo and of its individual organs and tissues, accompanied by an explanatory text. But as in the human embryo many of the earliest processes cannot be detected, I have endeavoured to supply the deficiency by means of observations made on the hen's egg."—*Prospectus.*

We shall probably, on a future occasion, have an opportunity of briefly noticing the work of Dr. Erdl, in connexion with the subjects which it more especially is designed to illustrate, the development, namely, of the body, organs, and the tissues of the embryo.

Traite Elementaire et Pratique de Pathologie Interne. Par *A. Grisolle*, M.D.P., Médecin de l'Hôpital St. Antoine agrégé à la Faculté de Médecine. Deuxieme Edition, revue et augmentée.

An Elementary and Practical Treatise on Internal Pathology. By *A. Grisolle*, M.D.P. Second Edition. In Two Vols. Octavo, pp. 1679.

A systematic work on Internal Pathology, or, as we should call it in this country, on the practice of medicine, from the pen of a distinguished physician of one of the Parisian hospitals, and which is already before the public in a second edition, cannot fail to claim the attention of the British practitioner and medical student. But its importance becomes more apparent when it is known that the views which the author sets forth have been principally taken in the wards of La Charité, and from the practice of Professor Chomel, who may be justly regarded as one of the most eminent, eclectic, or Hippocratic physicians of the present day. In this respect the author has trodden in the footsteps of Professor Louis, and may be esteemed of the same medical school with him and Professor Andral.

It is the design of the work to exhibit the state of medicine at the present day, as it exists not in France only, but in the other countries of Europe, and in the United States. But, although the author has shown a better acquaintance with English medical writers than is often to be found

amongst French physicians, it is evident that even he is imperfectly informed regarding the state of medicine in England.

In a systematic work like that of Dr. Grisolle the arrangement of the subject is a matter of no small importance. The author has adopted that which he calls a philosophical or nosological system, by which it is to be understood that his object is to bring together those diseases which are allied by the character and nature of the symptoms which they present, and which may therefore be supposed to require a parity of reasoning for their explanation, and analogous treatment for their cure. He prefers this to a pathological system, which might be founded either upon the classification of tissues, in which the particular derangements characteristic of different diseases occur, or upon the systems of organs subservient to the different functions of the body. There are advantages presented by each of these methods, but in the opinion of the author they are not equal to those of a nosological arrangement. The alphabetical order he rejects at once, condemning it as the worst that could be selected. In this opinion, if it is to be restricted to a comprehensive work for the use of the medical student, there is no one acquainted with the subject who will not readily concur. But in the application of the alphabetical method, as practically employed, it must be remembered that it is almost entirely restricted to large works of reference, such as the various Encyclopædias of medicine and surgery which are adapted to the use of those who are engaged in practice, or who may be already far advanced in the study of their profession. Each article in such works constitutes a monograph, in which the attention of the writer, as well as that of the reader, is concentrated upon the particular subject without his being necessarily obliged to consider what may be advanced in other parts of the extended work. In such a publication, the superiority of an alphabetical arrangement cannot be disputed.

The author observes with Requin, that Nosography must, in the present day at least, be neither exclusively organic, ætiological, nor symptomatic, but must assume a triple character. A mixed system must therefore be adopted to avoid mutilating the science, or leading it astray into hazardous hypotheses.

On this principle the following division into ten classes is employed :—

I. Fevers.
II. Diseases consisting in a fault in the proportion of blood.
III. Inflammations.
IV. Hæmorrhages.
V. Morbid secretions.
VI. The results of poisons.
VII. Diseases of nutrition.
VIII. Structural changes and accidental productions.
IX. Nervous diseases.
X. Diseases peculiar to certain organs or tissues.

We shall not attempt to criticise this classification, which the author does not pretend to be perfect. The number of authors who have already tried their hands on the construction of nosological systems sufficiently attest the difficulty of the task. These systems, which are necessarily more or less artificial, serve as the frame-work or skeleton to the body of

facts which are accumulated, and provided the principle be adhered to by the author, and comprehended by the reader, the principal object will be attained. It is of more consequence, as Dr. Grisolle remarks, to be accurate in the special details, and to these he has carefully, and we believe successfully, applied himself. As he informs us in the conclusion of his preface, he has treated of special Pathology without attempting those views which belong to general Pathology and Semeiology, for which he refers to the classical work of Professor Chomel, which he regards as unquestionably the most remarkable introduction to the study of medicine which has yet been produced.

FEVERS.

In making a distinct class of fevers, the author necessarily rejects the views of those who have endeavoured to reduce all these diseases to the class of inflammations, and says—"I believe there is no one, in the present day, who would dare to defend such a doctrine. On the contrary, every one admits the existence of diseases of which fever forms the predominant character, and which are not connected with any local derangement; or in which, if the solids are effected, their lesion is subsequent to the fever, and incapable of explaining it, being, like the fever, the effect and consequence of a more general cause."

He proceeds to enumerate the general phenomena of fever, and speaks of it as an element to be considered in forming a diagnosis. He repeats the remark of Andral, that in fevers not attributable to any local derangement, there is everywhere a tendency to inflammatory action, which, if the fever continue, will give rise to various phlegmasia, according to individual predisposition, or to the varied susceptibility of particular organs. Piorry believes that, in these cases, there exists a primitive inflammation of the blood, to which he gives the name of *hémite*. The author divides fevers into—

1. Continued fever—including ephemeral, inflammatory fever—typhoid fever, (the typhus of Europe)—typhus fever of the English—the bilious fevers of warm climates—yellow fever, and the typhus of the East.
2. Eruptive fevers, such as small-pox, chicken-pox, measles, scarlatina.
3. Intermittent, containing the mild, pernicious, and anomalous.
4. Remittent and pseudo-continued fevers, which are rather to be regarded as a sub-genus of the intermittent.
5. Hectic, lingering or chronic fevers.

Under the head of "typhoid fever" the author treats of that important febrile affection which constitutes the greater number of severe cases of fever which occur in our temperate zone, and which has received a variety of appellations, of which the following are enumerated at the head of the article:—Phrenitis, of the Greeks and Latins; Pestilential, Malignant, Putrid, Bilious, Mucous Fevers, of most authors; the low nervous Fever of Willis and Huxham; the adynamic and ataxic fevers of Pinel; the entro-mesenteric of Petit and Serres; the dothin-enteritis of Brettonneau; the Gastro-enteritis of Broussais, the Typhoid affection of Louis, Chomel,

and Andral; Follicular Enteritis of Cruveilhier and Forgèt; the Typhoid entro-mesenteric of Bouillaud; the Enterite septicémique of Piorry.

It would be difficult to adduce a more signal instance of the transitory character of individual popularity than that which has struck us in the perusal of this excellent article in the work before us, written and published in Paris, where the facts and views which it records have been collected. It does not contain a single mention of the name of Broussais except as having given one of the synonyms prefixed to the chapter. Broussais, who but a very few years ago was in the mouth of almost every medical man in Paris, from the professor to the student, applauded by many, and combated or condemned by a few: Broussais, who told the crowds of his admiring pupils, that they might disregard all preceding authors from the time of Hippocrates to the present day, and throw them aside as mere ontologists, whilst the only necessary key to medical theory and practice was to be found in the physiological doctrine which he propounded to them—Bronissais is not even once quoted. We looked in vain for the name of Broussais in the bibliography of the disease, in the exposition of its symptoms, and in the general and special instructions which are given for its treatment. Not a word of his exaggerated dread of aperient medicine—of his toleration of a loaded intestinal canal, that the fæces might serve as poultices to the ulcerated mucous membrane—of the myriads of leeches which he employed, or of the strict regimen which he prescribed. His merits and his defects are passed over in silence, and we are the more forcibly reminded of him by the very fact that so great and recent an idol is so completely thrown aside.

Nature of the Malady.

Typhoid Fever is anatomically characterized by lesion of an inflammatory nature situated in the follicular glands of the intestines and mesenteric ganglion. Louis regards this lesion as constant, whilst, according to Chomel, Andral, and Dalmas, it may in some cases be absent. Chomel having seen subjects sink when only one or two patches or only a part of a single patch of aggregate glands was diseased, was led to believe the possibility of the total absence of any lesion of the kind, and he was confirmed in this opinion by facts collected by Louis and Andral, in regard to individuals who had died after having presented many of the symptoms appertaining to Typoid Fever, without any of the intestinal lesions which characterize it having been discovered on inspection.

"I stated," observes Dr. Grisolle, "in the first edition of this work, that I have myself seen two similar cases, but made the objection that the phenomena noticed were not precisely those which occur in Typhoid fever, and that they ought in consequence to be regarded as belonging to another disease, to an affection not as yet defined. I am still of the same opinion. I find amongst my notes a case, which on other grounds is important to the solution of the question before us; it is that a man 22 years of age, who in 1835 died in the Hôtel Dieu, in the ward of M. Caillard, on the 27th day of continued fever, who had exhibited all the symptoms of severe Typhoid Fever, viz. intense headache (unaccompanied by epistaxis), vertigo, prostration of strength, absence of sleep, rambling, deafness, delirium, dryness of tongue, sordes of the mouth, diarrhœa, tympanitic distension of the abdomen, sibillant rattle, numerous sudamina (miliary eruption), rose-coloured spots, gangrene of the sacrum, penis, and scrotum. On dissection, however, no characteristic lesion, either of the intestinal

follicles or mesenteric ganglia was detected, but the spleen was enlarged and of a difluent consistence. This single fact would lead me to believe with Chomel that the intestinal affection is not an indispensable characteristic of Typhus Fever, since, in some excessively rare cases, it may be absent. Nevertheless, if the follicular affection of the intestines is not constant, in the rigorous sense of the word, we repeat, again employing the expression of M. Chomel, that it is extremely rare for it to be altogether absent, and that there is not a single authentic case on record in which this lesion has existed, unattended by the symptoms of typhoid fever. One circumstance which has greatly contributed to excite doubts as to the importance of the derangement of the Peyerian glands is the assertion of medical men in London, Edingburgh, and Dublin, who aver that derangement of the intestinal glands is frequently absent amongst their patients, who, during life, had exhibited symptoms of typhoid fever. But it is now proved by the clinical cases collected in London by our friend Dr. Shattuck of Boston, and analyzed by M. Valleix, as well as by the labours of Drs. Gerhard and Penwick of Philadelphia, that there exist in the United States and in England two febrile disorders, hitherto confounded together under the name of Typhus fever, but which are really distinct, and only resemble each other in their general symptoms. The one affects young subjects, and is the Typhoid Fever, such as we see it here (in Paris), the other common to all ages, with the exception of infancy, is Typhus Fever. It is a disease distinct from typhoid fever; it is the typhus fever which we shall describe by-and-bye.

"It is a question whether the lesion of the intestinal follicles is a primitive affection, as in the case of most inflammations, or consecutive to a general state, like the eruption of small-pox, to which it has been compared. The first supposition has the greatest appearance of probability, when it is remembered that in most cases the abdominal symptoms, diarrhœa and cholic, commence with the disease, which is not generally the case in other febrile affections. We cannot, however, speak with certainty as to the point.

"Does the intestinal affection constitute the sum of the disease? This does not seem to be probable, when it is known that there is frequently no proportion between the severity of the symptoms and the extent of the intestinal lesion. We have seen with M. Chomel death occur, although there was but a single patch of glands diseased, while, on the other hand, we often see in persons who have died from accidental causes very extensive lesions, although the symptoms during life had been of moderate severity. There are moreover, in the course of the disease, a number of morbid phenomena which are not explicable on the supposition of the existence of a general cause as yet unknown as to its nature and seat, which is placed by some in the nervous system, but which is more generally supposed to consist in an alteration in the blood, which has not yet been made out. Typhoid fever has sometimes been compared to eruptive fevers, and at others to simple inflammations, but if it have some points of resemblance with these, it has also many of dissimilitude. It is therefore expedient to consider typhoid fever a special disease, separated from all other affections by several fundamental characteristics."

The author then gives both sides of the question in the words of Professor Louis, who sums up the parallel, and the contrast between the eruptive fevers and the typhoid affection, and arrives at the conclusion adopted by our author.

Dr. Grisolle is one of those who admit the contagious nature of typhoid fever, although this character is disputed by many of his countrymen: but he agrees with Louis and others that it may be brought into existence by other causes, independently of contagion; and with Gendrin, that those who have been once affected acquire thereby a protection against future attacks.

Of the treatment he speaks under different heads. Under that of the Antiphlogistic he especially treats of the abstraction of blood. He admits its utility in certain cases, especially where there is a marked inflammatory character, but he condemns its general and copious employment, as adopted by M. Forget, and such as we remember to have been in vogue in the North of this island. He equally condemns the often reiterated bleedings of Professor Bouillard. He gives the preference to leeches applied to the iliac region, if there be acute pain there, but he objects to their application to the anus, which he regards as not merely useless, but liable to become a source of fresh irritation or even of gangrene. He likewise recommends the occasional application of leeches to the head, behind the mastoid process, but gives a salutary recommendation against allowing the bites to bleed long in children. He recommends the abundant use of drinks containing gum, or barley, as well as those which are flavoured with sugar and juicy fruits, some of which, we think, it would be better to omit, or give with great moderation. He enumerates emollient glysters, fomentations and cataplasms to the abdomen, and tepid baths, from which last, he observes, that great benefit is derived when the fever is violent, and the skin very hot and dry. He might have insisted more strongly on the efficacy of this treatment, and pointed out the circumstances which should direct its application, had he been acquainted with the classical work of Dr. Currie of Liverpool, and with that of Dr. Jackson.

He notices the contra-stimulant treatment merely to observe that he is unable to judge of the mode of employing tartar-emetic as recommended by Rasin, and that the use of sulphate of quinine in large doses and as a contra-stimulant is of questionable utility, although quinine, he admits, is decidedly useful in those cases in which regular paroxysms are established, when much smaller doses are employed.

Under the head of antiputrid or antiseptic treatment, he notices the employment of bark, camphor, musk, aromatics, wine, alcohol, mineral acids, internally and externally. He says of this treatment, that it was generally followed in France, whilst the opinions of Pinel (in the Nosographie Philosophique) were in vogue—that it has still many proselytes in England and Italy, and especially in Germany, but that it deserves no sort of confidence —that Andral has shown that of 40 patients so treated 26 died, and that three only of the 14 who recovered appear to have been benefited by the treatment, and that the others recovered as they would have done if left to themselves. Nevertheless, he sanctions the use of wine in extreme cases, though on the more reasonable ground of combating prostration of strength rather than the tendency to putridity.

In treating of the evacuant plan the author observes, that medical men have alternately adopted and rejected the employment of evacuants in the treatment of continued fevers, according to the theory which they adopted respecting the nature of the disease. They were at one time generally abandoned, and Brettonneau and Lerminier, who were almost alone in adhering to the older doctrines, were unable to inspire their brethren with the confidence that purgatives might be brought into contact with ulcerated intestines without danger. More recently, M. Delarroque, physician to the Hospital Necker, has demonstrated how groundless were these fears, and proved, by a numerous series of well-observed facts, the advantages

of evacuants in the treatment of severe fevers. This physician administers aperients in all the forms and periods of the disorder through its whole course up to complete convalescence. He generally commences by an *emetico-cathartic*, which Dr. Grisolle does not consider as useful, except there be symptoms of gastric oppression. The patients take every day a bottle of sedleitz water, or thirty grammes of castor oil, or two grammes of calomel, or a dose of cream of tartar. Pains in the belly, cholic, diarrhœa, and materorismas are, in his opinion, far from contra-indicating the use of purgatives, but seem, on the contrary, to call for their employment. If, in extraordinary cases, they increase the pain or produce hyper-catharsis, they are to be suspended for twenty-four hours. Delarroque conjoins with these means sweetened drinks and poultices on the belly, and gives tonics as soon as the fever is turned. In adopting this treatment, Delarroque has only lost one patient in ten. This plan has been tried by Honoré, Guenneau de Mussy, Bricheteau, Beau, Piédagnel, Jadioux, Andral, Louis, and many other physicians in Paris, who have all acknowledged the good effects of the evacuant treatment. Louis, after analyzing the different methods of treatment adopted in typhoid fever, gives the first place to the evacuant plan. It not only diminishes mortality, but shortens the duration of the disease. Grisolle adds, that by employing purgatives we are not favouring the development of any complication, and that the two most dreaded accidents, hæmorrhage and intestinal perforation, are much more rare than when the other modes of treatment are employed. He states that, in France, castor oil or sedleitz water is generally prescribed. In Germany and some parts of Switzerland calomel is most in favour, and this medicine is also preferred by Lombard and Fauconnet, who declare that they have only lost nine patients in a hundred so treated. He notices the assertion of Barthez, Rilliet and others, that the purgative plan is inapplicable to infants, though useful in adults; but he regards the assumption as doubtful, and worthy of further inquiry.

It is remarkable that the author makes no allusion to the employment of purgatives in this country, where, since the publication of the important work of Dr. Hamilton, they have been so generally employed, that the prescriptions, and books, and records, of almost every medical man in the country during the last thirty years might be appealed to in proof of the general adoption of the very plan to which the author has given such decided preference. In fact, the use of purgatives has not unfrequently been a subject of reproach against us on the part of our continental brethren, and it has been one of the happy results of the eminently eclectic character of British medicine, and of our readiness to profit by the observation and experience of our contemporaries, that the immoderate employment of purgatives has been checked, without our falling into the opposite error of rejecting them.

Typhus.—It has been attempted to constitute a disease distinct from typhoid fever under the name of "Typhus, or Plague of Europe." The fever so designated has been seen in prisons, hospitals, and camps, and has been common in besieged cities, wherever numerous individuals are brought together in close situations or under other unfavourable circumstances. It is eminently contagious, which has been regarded as one of the grounds of

distinction by those who doubt the contagiousness of the typhoid affection. Many of the symptoms are admitted to resemble those of the typhoid affection. It has been attempted to show that the derangement of the alimentary canal is slight or altogether absent in many cases of typhus. That petechiæ are much more common in typhus and miliary eruption than in typhoid fever—that the red lenticular spots appear some days earlier in the former—that hæmorrhage and intestinal perforation are also more uncommon, and that the spleen is not essentially in a morbid condition. Notwithstanding these attempts at distinction, the author regards it as proved by the pathological researches of Chomel, of M. Gaultier de Claubry, and by the narratives furnished by M. Landouzy himself (who adopts the opinion that they are different), that these two affections are essentially the same, and that the supposed differences merely belong to the accidental varieties of different epidemics.

The case is different with the typhus or continued fever in England and Ireland. The medical men of Great Britain, and even French physicians who have had the opportunity of visiting British hospitals, have noticed cases of fever in which the lesion of the Peyerian gland was absent. At first the French physicians of the school of Louis were disposed to doubt the accuracy of such narratives, and to presume that the inspections had not been made with sufficient care. But the concurrent testimony of several able and experienced observers, and more especially of Drs. Gerhard and Shattuck, American physicians, who had studied in Paris, removed all doubt on the point, and have led to the conclusion adopted by Professor Louis, as well as by the present author and many others, that there is a really distinct malady not uncommon in Britain, and occasionally occurring as an epidemic in the United States, which, notwithstanding many symptoms common to it and the typhoid, it is essential to distinguish from that form of fever. Whilst the most important anatomical difference is observed in the absence of lesion of the aggregate glands, a difference is also to be noticed in the cutaneous eruption occurring to the two diseases. In typhus fever the eruption consists in numerous, irregularly grouped spots, varying in size from that of the head of a pin to that of a pea, of a violet or deep red colour, and not disappearing under pressure. It appears on the sixth or the eighth day, and rarely disappears before the twentieth, but sometimes persisting to the twenty-eighth, and even thirty-first day. Miliary eruption is said to be uncommon in the English typhus fever. Blood drawn from the veins has no buff, and the clot is soft and again becomes fluid. Epistaxis is much less common than in typhoid affection.

If, with Dr. Grisolle, we have made the distinction between the typhoid affection as characterized by the specific derangement of the aggregate glands and the cases of fever noticed in England and America, which we think must be conceded, various considerations not altogether unimportant necessarily suggest themselves. If, in these days of careful anatomical research, the distinction has been with difficulty established, how are we to receive the narratives of the best older authors who have written on fevers, and who, with the same varieties of disease under observation and treatment, were incompetent to draw the distinction? If the derangement of the aggregate glands characterizes a disease as distinct and specific as scarlet fever or measles, and which is wont in its pro-

gress to be accompanied with many symptoms in common with typhus fever, are we prepared to admit that the cases in which the derangement of these glands has been absent, as in the fevers noticed in Britain and the United States, as really constituting a second specific form of fever, and are there not, rather, sufficient reasons for concluding that the conditions and symptoms, regarded as common to typhus and typhoid fever, may present themselves in several febrile diseases in common, constituting, as the older authors led us to believe, the tokens of a particular stage in the morbid process? Such is the idea which we are disposed to adopt, and if it will not remove the whole difficulty, it will at least prepare us to appreciate the importance of those accurate pathological researches which alone can enable us to distinguish different forms of fever tending to some common results.

The other forms of continued fever which are noticed are the bilious fevers of warm climates—yellow fever and plague, which we pass over. The eruptive fevers constitute the next group. In speaking of variola, he notices Piorry's plan of softening the skin by baths and moist compresses, and then breaking the pustules to prevent the formation of scars; and also the plan of cauterization advocated by Brettonneau, Seres and Velpeau; but these plans are not commended by the author. He has a better opinion of the application of mercurial ointment.

In speaking of Vaccinia, he admits the deterioration of the vaccine virus since its introduction by Jenner, and recommends frequent recurrence to the original matter from the cow, which produces a much finer pustule, attended with more constitutional affection than is the case with the older virus. He says that, by inoculating the cow with old virus from the human subject, we do not obtain any advantage; but he says nothing of the means of re-producing efficient vaccine virus, as taught by the remarkable researches of our countryman Mr. Ceely, or rather he merely alludes to them to express a doubt of their accuracy.

In treating of modified Small-pox and Chicken-pox, he alludes to the opinion held by the late Dr. Thomson and Mons. Rayèr, that modified small-pox and chicken-pox are only varieties of the same affection, being both produced by the contagion of small-pox. He does not consider the question as fully decided, though he inclines to the opinion of their being distinct affections. The fact of chicken-pox being seen in those who have had neither small-pox nor cow-pox concurs with the characteristic appearance of the eruption to place chicken-pox as a disease wholly distinct from small-pox. In his article on Measles he does not agree with Willan, that this disease was known to the Greeks and Romans, but thinks that it was probably derived from Asia, the apparent source of so many of our diseases, at a period not known, since Rhazes, the first author who has given an exact description of it, does not describe it as a new disease. He notes as varieties—measles without catarrh—and measles without eruption,—and the black measles of our countryman Dr. Willan, to whom he frequently refers in the course of his work. He mentions the formidable complications of measles; namely, Pneumonia, which in infants is almost always of the lobular form—Enterocolitis—Gangrene of the lips and lungs—Inflammation of the membranes of the brain, with Delirium, Coma and Convulsions, and Whooping

Cough and Croup, which often succeed to it. He does not omit to mention the curious fact pointed out by Hunter, that the contagion of small-pox and measles may simultaneously exist in the patient at the same time, and produce their characteristic eruptions, but that measles is suspended whilst small-pox proceeds with its development, and then resumes its ordinary course. The measles greatly favour the production of consecutive tubercles. And, measles attacking those already affected with phthisis, the progress of this disease is accelerated. Cold, he thinks, produces in those who are convalescent from measles, anasarca much more frequently than after scarlatina.

Measles, on the other hand, have sometimes been seen to produce a favourable effect on pre-existing maladies. Thus Rayèr gives an example of chronic Exema of the face, and Alibert one of Empetiginous exema of the scalp, which were quickly cured after the recurrence of measles. He notices the inoculation for the disease by means of blood taken from the spots, as practised by Home in Edinburgh, and Speranza at Milan, which caused the appearance of the disease after six days of inoculation, and also inoculation with tears or saliva, which does not appear to be quite so effectual.

We pass over the other eruptive fevers, to notice the important division of INTERMITTENT FEVERS, which he distinguishes as simple, mild, or pernicious, manifest or masked, essential or symptomatic, regular or irregular.

SIMPLE INTERMITTENT FEVER,

Having no local seat, furnishes no pathological appearance after death which can be regarded as the starting-point for the phenomena noticed during life. The enlargement of the spleen, which is almost never absent, being rather to be regarded as a concomitant affection. The softening of this organ is only to be met with in those intermittents which are pernicious.

The types of intermittent fever, distinguished by the returns of the paroxysms, are the quotidian, tertian and quartan, with their varieties, double quotidian, double tertian, and double quartan, but these last, with the exception of the double tertian, may be regarded as either exceptional or altogether doubtful; and the same may be said of triple tertian, triple quartan, and also of the quintan, septan, octan, monthly and annual, which have been admitted by some nosologists.

It is needless to quote descriptions of the different stages, which, though good and concise, offer nothing remarkable in relation to facts which are sufficiently known. A comparison of the frequency of different types is interesting. From a comparison of more than 160,000 cases observed in different countries, it appears that quotidians are more common than tertians, in the proportion of 9 to 1, and that quartans are so rare that there are scarcely two or three in a thousand. The author does not agree with Boudon that there is any connexion between the type of the fever and the intensity with which the malaria is disengaged.

The preparations of bark hold, of course, the first place among the curative means, and the sulphate of quinine is preferred to all the others. Besides the ordinary mode of giving it by the mouth, Dr. Grisolle notices

its administration as an enema, which is less certain than the endermic employment, as tried by Chomel. The sulphate of quinine applied to the skin only two hours before an expected paroxysm, succeeded in setting it aside, but a much longer time was required when the remedy was given by mouth. The application to the skin had the serious inconvenience of producing sloughs and ulcers, both painful and slow of cure. He notices, besides quinine, a list of other articles, such as salicine, chesnut bark, opium, tonquin bean, orange-peel, digitalis, armica, iron, mercury, alum, salt, phosphorus, all of which he rejects as entitled to no confidence. He has some reliance on arsenic, but he appears to regard it as so dangerous as to be only admissible in the smallest doses in those cases which resist the influence of quinine.

Our cautious author rejects the attempts to localize the affection by placing it either in the intestinal canal where cadaveric changes have been mistaken, or in the spleen, where Piorry and Audouard would fix its seat.

"It would be idle," he says, "to enter into a discussion to prove that intermittent fever is not an inflammation. Shall we, with Giannini, call it a *nevrodsthenie*, or, with Brachet and Rayèr, a *neurosis*, a cerebro-spinal irritation, with Maillot, or with Worms, an affection of the ganglionic system? It seems reasonable to connect the principal symptoms of the disorder with nervous derangement. We are not, however, in possession of any positive knowledge on this subject, and it is much better to confess our ignorance than to attempt to conceal it under ill-defined pretensions. It has not merely been attempted to localize the derangement in intermittent fever, but also give an explanation of its periodicity. The opinions which have been advanced in relation to this question are so extravagant that we think it best wholly to pass them by.

"In fine, we must admit that we know nothing of the composition of miasmata, of the organ upon which they act, or of the manner in which quinine operates to neutralize their influence."

Pernicious Intermittent Fevers.

The term pernicious is applied to those intermittent fevers which, on account of their great severity, and rapid progress, may terminate fatally in a few paroxysms. There are several varieties of pernicious fevers. In some there are many severe symptoms without the predominance of any one. The countenance is remarkably changed—the strength is prostrated—the pulse is small and irregular—the tongue dry, and the intellectual faculties blunted. More often, there is some symptom which arrests particular attention. Sometimes the cold stage is so intense that the countenance appears cadaverous;—the shivering is extreme, the breast cold, the voice gone, the thirst great, the pulse small and irregular. Death may take place in the first paroxysm, but if this be survived the warmth is slowly and imperfectly restored, and without prompt assistance the second attack is generally fatal. In the next variety death occurs in the sweating stage. This is a more insidious form, the case at first appearing mild, but in successive paroxysms the sweating becomes increasingly profuse, and soaks every part of the bed; the patients become cold, their strength is exhausted, their pulse extremely small, but the intellect is not affected.

Class 2.

In the second class, or that which comprises those diseases with con-

sist in a disturbance of the due proportion of blood, we have first the cases of excess of blood constituting plethora and local congestions, which are either active or passive. He properly distinguishes this state from that of inflammation, though he regards it as one of the modes in which inflammation may be set up.

The opposite state furnishes the character of the second division of this class, in which are placed Anæmia, Chlorosis, and the Anæmia of particular organs. With respect to general Anæmia, he refers to the researches of Andral and Gavarret, Bouillaud, Marshall Hall, and Piorry. He mentions the views of Drs. Hope and Ward, respecting the peculiar sound termed *bruit de diable*, which is heard in patients labouring under anæmia, but he does not agree with them in the opinion that it is produced in the larger veins, having been shaken from this opinion, which he had once adopted, by the observations of Mons. Beau, who maintains the old opinion that the sound is arterial.

Anæmia not being solely the consequence of blood, but originating in many instances in serious visceral disease, the treatment must of course be various, as well as the probability of success. He gives the first place to ferruginous preparations, as not only contributing to improve the blood, but also as the most effectual remedy to the neuralgic pains which are apt to occur in the course of the disease.

Chlorosis is regarded as essentially a state of anæmia, and not necessarily distinguished as a special affection, yet the class of subjects to whom the affection is peculiar seems to render the division not altogether unsuitable.

The cases of partial anæmia are merely alluded to, the author observing "that, in the present state of our science, we know absolutely nothing about them."

Class 3.

The third class, or that of Inflammations, in which are included not only inflammations of the viscera, but also various forms of skin-disease, occupies a considerable part of the first volume.

The several special affections are arranged in the following groups:—

1. Inflammations of the digestive organs.
2. Inflammations of organs accessory to digestion.
3. Inflammations of the organs of respiration.
4. Inflammations of the organs of circulation, including angioleucitis, or inflammation of the lymphatic vessels.
5. Inflammations of the nervous system.
6. Inflammations of the organs of the senses.
7. Inflammations of the urinary organs.
8. Inflammations of the genital organs.
9. Inflammations of the cellular membrane.
10. Inflammations of the skin.

Class 4.

In speaking of Hæmorrhages he distinguishes the spontaneous from the tromatic, which fall to the care of the surgeon. The spontaneous are either symptomatic or essential; the former being referrible to some anterior affection, of which it may be considered as forming a part, but, in the latter, the hæmorrhage itself constitutes the disease. These hæmor-

rhages are distinguished by authors as active, passive, constitutional, accidental, succedaneous, critical, internal, and external. They are also either insterstitial or on the free surfaces.

There is not always any manifest erosion by which the blood has escaped from its vessels, and in such cases it seems necessary to admit an alteration both in the fluids and solids, but it is impossible to state the intimate cause of such a phenomenon. When death has been caused by profuse hæmorrhages, the sanguiferous system is found almost empty—the blood which it contains is pale and serous—all the organs are reduced in colour, and the heart is contracted on itself. M. Beau has lately advanced an opposite assertion (*see Archives de Medicine*, 1845), viz. that, in those who die after having suffered from repeated hæmorrhages, the heart is hypertrophied and dilated. The fact is far from being established on positive proofs, and at present it has scarcely any support except from a few experiments performed on inferior animals.

We may pass over most of the forms of hæmorrhage, which are arranged very much in the order adopted for the corresponding inflammations. A single page is allotted to hæmatidrosis or bloody sweat, of which it is observed, "there exists a certain number of well-authenticated observations to prove that blood has sometimes been exhaled from the skin, probably by the same course as the perspiration. Such hæmorrhages are rarely general—they are partial, and are seen on those parts on which the skin is white, fine, and often bathed with sweat"—"or they may take place on the surface of an old cicatrix."

The cases alluded to are such as having occurred in young and middle-aged women, were connected with amenorrhœa or deficient menstruation; but no mention is made of another form of hæmatidrosis, which has in some rare cases occurred in conjunction with great disturbance of the circulation, arising from moral or morbid causes affecting the heart. A careful commentary on such instances is to be found in the recent and remarkable work of a learned and accurate English physician, Dr. Stroud, who, in treating on the Physical Cause of the Death of Christ, has had his attention directed to this symptom related by Luke, as accompanying the agony of our Lord in the Garden of Gethsemane.

Of the interstitial hæmorrhages to which the name of apoplexies has been given, cerebral apoplexy is the most important. After noticing the well-known fact that, the paralysis consequent on cerebral apoplexy occurs on the side of the body opposite to that on which the effusion of blood on the brain has taken place, the author says. "There exist, however, some authentic observations of paralysis taking place on the same side as the effusion, but these cases are too decidedly exceptional to prevent our stating, in a case of hemiplegia, that the seat of the effusion is in the opposite hemisphere. Is it possible to localize the malady more precisely? Can we for example, assert that the effusion is in the middle or the anterior lobe, in the corpus striatum or the thalamus opticus, or in the cornu ammonis, or in the convulsions from indications afforded by the seat and limits of the paralysis. I regard this localization as impossible in the present state of science." Though he founds his opinion on the numerous observations of Andral and Finck, who arrived at this conclusion, we must admit that we still attach great confidence and importance to the views of Dr. Foville,

who, though not named, was with his friend Delaye, the author of the localization in question. Our own experience has shown its accuracy in many instances. Dr. Bright and the late Dr. Sims have furnished examples which tend to confirm it, and lastly the very cases which are adduced by its opponents, when sufficiently detailed, offer the explanation of the apparent exception. As a general remark, it may be observed, that this localization cannot be made or relied upon in many recent cases, since the parts in the vicinity of the clot may suffer to a degree which interferes with the function, although the absolute lesion may not extend to them. In the process of recovery there is a marked difference in the returning power of the paralyzed members corresponding with these uninjured portions of the brain.

The *fifth class*, or that comprising the morbid secretions, relates first, to the serous effusions of Dropsies. The introduction of Asiatic Cholera into this division appears to be of questionable propriety. Secondly, to the mucous secretion of which bronchorrhœa, gastrorrhœa, catarrhal diarrhœa, leucorrhœa, and vesical catarrh are particularized. Thirdly, the proper secretions of special organs, such as the saliva, the bile, the urine, milk and semen; and fifthly, morbid secretions, all the examples of which group consist in the preternatural development or accumulation of air or gas.

In the *sixth class*, which comprises the effects of poisons, the author adopts the classification of Vicat, modified by Orfila, and makes four divisions according to their mode of action upon the system: viz.—1, Irritating Poisons; 2, Narcotics; 3, Narcotico-Acrid; and 4, the Septic or Putrescent. Though the most convenient classification, it has its defects and difficulties; and the author refers to the remark of Mead, that the same poison will act differently according to the dose and mode of application, and cites in illustration preparations of lead, which, though ranked with irritating poisons, exert a widely different influence after their absorption. "Many poisons are taken into the alimentary canal; others act when they have been applied to other mucous membranes, or to the skin, more especially when denuded; or they may have been applied to a wound or introduced into the cellular membrane."

Whatever may be the mode of introduction into the economy, poisons may be limited to a local action, irritating or disorganizing the tissues—such are the concentrated acids, potash, &c. Others producing no effect on the surfaces with which they are brought into contact, act on more or less distant organs after they have been absorbed. This is particularly the case with opium. Some poisons have a complex action. They first inflame the structure to which they are applied, and then, a portion being absorbed, occasion similar lesions in other organs. Such, for example, are cantharides, which, being swallowed, inflame some parts of the urinary passages. Others exert their force on the nervous system, either causing excitement or insensibility, without producing any material change of tissue. Such is the action of nux vomica and tobacco. Lastly, there are some poisons which, being absorbed, act primitively upon the blood and change its composition. Many poisons, but especially the several kinds of morbid virus, are of this description.

We see, then, that a great number of poisons are absorbed. This absorption, which but a few years since was unknown in the case of some

substances, and rather admitted on physiological inference in the case of others, has been demonstrated by experiment by Orfila, who has discovered most mineral poisons in the tissues of several organs, in the blood, and in the urine. A new path has thus been opened for legal medicine.

Most poisons act immediately, but others produce their specific effects after the lapse of days, weeks, or months, requiring what is termed a period of incubation, as in the case of poisoning by different kinds of morbid virus. The symptoms which are induced by poisons have generally an acute or hyper-acute character, and some persons have even denied the existence of slow or chronic poisoning, yet we have undoubted instances of this kind in the influences of lead or mercury. The old notions of a slow poison by which death may be insured at a fixed distant period, was an idea produced by ignorance and maintained by popular prejudice, and may be regarded as contrary to the laws of nature. Chronic visceral disease, either produced spontaneously or the sequela of an acute affection induced by poison, may have been frequently mistaken for cases of slow poisoning.

Symptoms dependent on disease of the abdominal viscera and derangements of the cerebro-spinal system may bear some resemblance to the effects of poison, as in indigestion, iliac passion, cholera, peritonitis, hepatic and nephritic cholics, certain nervous affections, and apoplexy of the brain and spinal cord. In general, inspection will remove the doubts which may exist; but when the investigation of the symptoms, and the inspection, aided by the microscope, are insufficient, recourse should be had to a chemical analysis of the excretions, and likewise of the organs themselves.

We pass over the author's remarks on individual poisons of the acrid narcotic, and narcotico-acrid classes, and give his enumeration of the putrid or septic poisons. We may, however, just observe that, in treating of poisoning by arsenious acid, he confirms the recommendation of the hydrated oxyde of iron. If this be not at hand, the *aperient saffron of Mars* may be substituted for it, but in larger doses. The antidote is to be given after the mechanical irritation of the fauces have been employed to produce the rejection of the poison by vomiting. We have employed the hydrated oxyde of iron in a case in which about two drachms of white oxyde of arsenic had been taken. Success in this instance was complete, the patient being perfectly well in the course of a few hours. But in this case repeated powerful evacuation of the stomach had previously been produced, by forcing the patient to swallow several raw eggs, then dosing him with a solution of the sulphate of zinc, followed at intervals by copious draughts of warm water. The energetic and copious vomiting brought away considerable quantities of arsenic in substance. We have known the white oxyde of arsenic to be retained, after an emetic of sulphate of zinc had been employed in the usual mode; which is not surprising, when it is considered that one of the effects of the arsenic, taken in substance, is to excite the effusion of lymph, by which the arsenic is entangled, and retained in contact with the stomach. This will account for one of the most remarkable cases on record, in which a young woman survived the swallowing and retention of a large dose of white arsenic, which, some months after, was discovered shut up in a membranous cyst attached to the lining of the stomach.

The effects of alcohol are mentioned among those of vegetable poisons,

and the general or partial combustibility of the body is recognized as one of its rare but undoubted effects. When general, this combustion may be rapid, and completed in two or three hours, or even in a single hour. This form is mostly met with in aged females, whilst the partial combustion has been more often noticed in men. The simple affusion of cold water seems to accelerate, rather than stop, the combustion, which, however, may be subdued by prolonged immersion in very cold water.

Under the head of Septic Poisons, the following are included:—Putrid exhalations; putrid articles of diet; putrid matters applied to wounds; ergot; maize; gas from cess-pools and drains; animal poisons, as of the viper, the rattle-snake, the scorpion, the wasp, &c.; the several kinds of morbid virus, as of syphilis, yaws, hydrophobia, malignant pustule, and farcy or glanders.

7th Class—Lesions of Nutrition.

The subdivisions under which the author arranges the affections comprised in this class are,

1st. Hypertrophy, specifying that of the brain, spinal cord, heart, liver, spleen, thymus and thyroid glands, and Arabian elephantiasis. Cirrhosis is introduced as hypertrophy of the liver, but constituting a separate form.

2ndly. Atrophy—viz. of the brain, spinal cord, heart, liver, and biliary ducts.

3rdly. Induration—of the brain, spinal cord, heart, and liver.

4thly. Softening—of the brain, spinal cord, heart, stomach, intestinal mucous membrane—of the liver, spleen, uterus and bones.

5thly. Gangrene—specially noticing that of the lungs, the mouth, pharynx and vulva.

6thly. Ulceration.

7thly. Those lesions which produce the contraction, obliteration, dilatation, perforation and rupture of hollow organs. Under this head are noticed the openings of the heart, contraction and obliteration of arteries, veins, and sinuses; contraction of the air-passages of the œsophagus and intestinal canal; dilatations of the bronchi and pulmonary cells; dilatations of the heart and of the lymphatics, aneurism, dilatations of the pharynx, œsophagus and stomach—Ruptures of the œsophagus, stomach, intestine, and parietes of the heart; Ruptures of the tendinous cords of the heart; Preternatural communications between the cavities of the heart; Ruptures of the aorta; rupture of the lungs and pleura, producing pneumo-thorax; rupture of the diaphragm; rupture of the spleen, liver, kidneys, and gall bladder; ruptures of the urinary bladder and of the uterus.

Class 8.

In this class, which comprises structural changes and accidental productions, there are two principal divisions—the first consisting of those diseases which are characterized by the transformation of tissues, or by the development of new tissues analogous to or identical with those which are natural to the body;—the second are marked by the existence of new products, altogether abnormal.

The first of these divisions comprises fatty, serous, corneous, cartila-

ginous, and osseous formations, and some forms of polypy. In the second class are included inorganic concretions, entozoa, cancer, tubercle, and melanosis.

In speaking of cysts, which he regards as the accidental production of a serous tissue, he notices their occurring sometimes solitary and sometimes in great numbers.

"We sometimes," he observes, "find so many in the same individual, that it has been supposed that their production depends on a particular diathesis. Such is the case of a woman related by Morgagni, in whom he found 800 serous cysts, disseminated in most of the viscera.

"The mode of formation of cysts has been a subject of much discussion. Some cousider, with Louis, Haller, and Morgagni, that the effused material is pre-existent to the cyst, whilst, according to Bichat and Cruveilhier, the opposite takes place. The latter opinion is probably more generally correct. It is, however, indisputable, that in many cases the formation of a cyst is consecutive to the presence of a foreign body. Such, for example, we have seen to be the case with collections of pus and blood of the brain. It is also seen when bullets are odged in the substance of tissues."

Cruveilhier's arrangement of cysts, which is very good and practical, recognizes these and various other forms of cysts.

In speaking of particular forms of cysts he notices specially those of the brain, those of the liver and spleen, those of the kidney, the dilatation of the pelvis of the kidney, caused by some obstruction in the passage of the urine to the bladder, to which the name of Hydronephrosis has been given by Rayèr, who has well described the affection, Ovarian Cysts, which he divides into the hairy, the serous, and the acephalocystic.

Ovarian cysts, containing hair, are often accompanied by teeth, generally those of the first dentition, portions of bone, apparently belonging to different parts of the skeleton, skin, muscular tissue, and a greyish-white matter.

"The origin of these productions has been much disputed, but we think, with Haller, Cruveilhier, and Velpeau, that they apertain to fœtal debris. It must be admitted that in the cases of which we are speaking, ovarian pregnancy had taken place; that the fœtus, having perished at a more or less early period from conception, had been almost entirely dissolved, while certain parts had resisted destruction, or even continued to grow. Such are the hair and even the teeth, which sometimes acquire considerable development. The interest of these tumours is almost wholly of an anatomico-pathological character, as they generally remain stationary during life, producing little distention or derangement of health. They may however be accompanied by inflammation, and attended with symptoms observed in other cases of extra-uterine pregnancy."

We do not admit the author's explanation of the origin of these cysts as appplicable to all cases

Of the serous form of ovarian cysts he speaks at greater length.

"Serous cysts, which are rarely met with in other parts of the body, are very frequent in the ovaries. The peculiarity is explained by the structure of these organs, which contain within their interior fifteen or twenty small vesicles, which are probably the most frequent starting-points of the malady. The tumour, however, does not always begin exactly within the ovary. Sometimes it is external to it; that is to say, immediately under the peritoneum, but without the fibrous

coat. It is even not unfrequently situated upon the uterus, or some other of its appendages, besides the ovaries. However, some persons believe that, even in these cases, the ovary is the original source of the disease. They suppose that an ovum spontaneously detached under the influence of venereal excitement has fallen into the peritoneum, where it has contracted attachment, and continued to grow.

"This opinion, of which the accuracy cannot be demonstrated, is nevertheless very probable, for cysts within the pelvis are perhaps never met with but in the female. It is therefore natural to refer the production of these tumours to some natural organic condition of the female, and this condition can be no other than the presence of an organ which does not exist in the male."

Without questioning the strong predisposition which exists in the ovary and its vicinity to the production of serous cysts, we cannot unite in the theory here adopted, since cysts, having in every respect the anatomical character of those occurring in the ovary, are found in various organs and textures of the body, in males as well as in females. That they are not degenerations of the vesicles of De Graaf, is evident from the fact, that the morbid enlargement of these vesicles, when it does occur, gives rise to cysts materially different from those which constitute the ordinary form of ovarian dropsy It is not, however, improbable that the peculiar microscopic texture of the ovary, which admirably fits it for the production of a succession of Graaffian vesicles, may, by some disturbance of this process, bring about the formation of the cysts in question.

A good description is given of the symptoms, varieties, and terminations of these ovarian cysts. A case observed by Dr. Bonfils, in which the rupture of the cyst was followed by radical cure, is mentioned as a solitary instance. Several instances of the same kind, have, however, been seen and recorded in this country.

With regard to the treatment, our author admits the entire failure of all medical means. The strong support and compression of the abdomen is to be adopted as a palliative. Puncture, generally employed with this view only, has sometimes obtained a radical cure. The puncture, which is generally made through the abdominal parietes, may sometimes be performed through the vagina, which is not only favourable to the escape of the fluid, but also to the introduction of the canula, by which a gradual and continued discharge may be procured; and such evacuation has been known to lead to an entire cure. The operation is only practicable in this mode in some cases, and when practicable is not always safe.

Some persons have ventured to attempt a radical cure by injecting irritating liquids or vapours into the sac after evacuation, and others have incised the cyst after having caused adhesion to the abdominal parietes. All of these plans our author rejects as more or less imprudent, and whatever success may have been related as accompanying them is not sufficient for their justification. On the same grounds he rejects the extirpation of the cyst after merely alluding to the operation, and stating that it has been performed. He, however, recommends a treatment stated to have been successfully performed by Professor August Bérard. It consists of making punctures through the cyst with a long acupuncture needle. He considers that, by this means, the cyst is so modified as to promote a slow but ultimate absorption of the liquid.

Amongst the horny and epidermic productions are noticed horns, which are more often met with in aged females than in males. They are sometimes solitary, more seldom numerous. They are about two centimetres in length, but have been seen upwards of thirty. Malpighi and Raspael thought they were formed by a nervous prolongation from the dermis, but Breschet thinks, with more probability, that they are a morbid secretion. The other forms of corneous production are ichthyosis, pityriasis, lepra, and psoriasis.

After noticing cartilaginous and osseous formations, Dr. Grisolle speaks of Polypi, merely enumerating those which, from their position, come more particularly under the attention of the surgeon, as for example, in the nose, pharynx, and uterus, but dwelling on those of the stomach and intestines, and of the larynx and bronchi. He states that most of the cases of polypi of the heart which have been published in France and England, are only cases of sanguineous concretion, but that there are some well-authenticaied, though rare examples of accidental productions of the same character, or analogous to the fibrous and fungous polypi of the nose and uterus. Their most frequent seat appears to be the left auricle, and in two instances they were attached to the spot at which the foramen ovale had been closed. In two cases, which we have seen, adventitious productions somewhat resembling this character were attached to the margin of this valve, which had remained open to an advanced period of life.

The so-called vegetations of the heart noticed by Corvisart and Laennec, and which the former, erroneously, supposed to be of the character of syphilitic warts, are formed in the ventricles, or attached to the valves. Some are regarded as unquestionably the result of endocarditis, whilst others have more the character of coagula.

Accidental Productions foreign to the System.

We shall pass by all the calculi, and the parasitical animals which are included by the author in this class, only observing that he inclines to the recent, and not improbable, opinion, that acephalocysts are merely envelops to the echinococcus; and that the vesicular mole, or placental hydatid, is, to our surprise, included under the head of hydatids, which is the more remarkable as the author is acquainted with the researches of Madame Boivin.

In the same section, and without any subdivision, the author treats of the whole group of cancerous, scrofulous, and melanotic productions, these last being separated from the other forms of malignant disease. In noticing the microscopic characters of cancer, as marked by various forms of nucleated cells, he only mentions the observations of Lebert, without alluding to those of Müller; which were, we believe, the first, and which have been followed up by many others.

There is nothing new or remarkable in the description which is given of the various forms of cancer, and which appear to have been drawn from various sources, including several English authorities. The author adopts the opinion of Bérard, with regard to the softening process, depending on increased vascularity, which is doubtless a part, though we believe not the whole of the truth. He agrees with those who of late have properly distinguished the colloid or gelatiniform cancer from the encephaloid The

distinction is doubtless correct with respect to the most characteristic specimens in which there is no blending of the two diseases, but there are examples in which, judging from external and manifest appearances, they seem to concur and insensibly pass into each other.

Cancer is described not only in its general characters, but in relation to the most important organs liable to be invaded by it, such as the lungs, pleura, stomach, intestines, liver, uterus, &c.

The author treats of tubercle in general as a peculiar deposit, following Laennec and Louis for its manifest characters, and Lebert for the microscopic. He notices the views of Baron, Cruveilhier, Lallemand, Piorry, and Carswell, as to the mode of origin, and those of Broussais, Lombard, Andral, and Louis, as to the mode of development and the influence produced on surrounding parts.

Under the special affections of this group, phthisis pulmonalis is of course the most prominent, and the article on this disease contains a summary of the observations of the best authors, but he does not appear to be aware of the beautiful microscopic observations and researches of our countryman, Mr. Raine.

Tubercular meningitis, which forms another member of this group, is made to include hydropcaphalis internus. Tubercles in the substance of the brain are treated of separately.

Tubercles in the mesenteric glands, constituting mesenteric atrophy, forms another division under the name of Carreau.

The article on scrofula is short, and we regret to observe the absence of any notice of the laborious inquiries of our countryman Benjamin Phillips.

We pass over the *9th Class* of nervous affections to notice the last, viz. *10th Class*, in which are placed affections peculiar to certain organs or tissues. This comprises the following list:—

Disorders peculiar to the digestive organs, the first of which are those accompanying the first dentition; secondly, Indigestion; thirdly, Gastric disturbance; fourthly, Intestinal disturbance; fifthly, Constipation; sixthly, Intussusception, or volvulus; seventhly, Diabetes.

Peculiar to the liver—Jaundice or Icterus.

Peculiar to the kidneys—Bright's disease.

Peculiar to the heart—Insufficiency of the valves, viz. of the Sigmoid, of the tricuspid and mitral.

Those peculiar to the air-passages;—Asphyxia, divided into three forms—1, by compression of the chest,—2, by strangulation,—3, by submersion.

Disorders peculiar to the genital organs;—Dysmenorrhœa; Irregular menstruation; Amenorrhœa; Cessation of Menstruation, or symptoms attending the change of life.

Disorders peculiar to the fibrous and muscular tissue:—Rheumatism, subdivided by its several seats (one of the most remarkable of which is rheumatism of the uterus during pregnancy and parturition;) Acute rheumatism and rheumatic gout; Chronic rheumatism.

Diseases peculiar to the skin:—Prurigo, lichen, urticaria, elephantiasis, lupus, maculæ, lentigo, eplelides.

Were we disposed to criticise the arrangement adopted by the author we should, perhaps, object, in the first instance, to the formation of this

last and miscellaneous class; and secondly, to his having placed in it some affections which might have been arranged in some one of the preceding sections, to which they would seem naturally to belong. Little practical inconvenience can, however, result from the course which the author has adopted, and the practitioner, as well as the student, will find the articles on Bright's disease, on diabetes, and on rheumatism, very worthy of attention. In the short article on deficiency of the valves of the heart, he gives a prominent place to the labours of Corrigan, Hope, and Henderson, as well as to those of his countrymen, but he does not seem to be aware that the affections to which he alludes were known and described before the appearance of the articles to which he refers.

In taking leave of the work we think we are justified in pronouncing it one of the very best and most comprehensive general works on medicine with which we have yet met. The abundant proofs which the author has given of his labour and candour warrant us in anticipating that he will continue to improve the subsequent editions, which the demands of the students in the French schools can scarcely fail to call for.

I. A Treatise on the Structure, Diseases, and Injuries of the Bloodvessels. By *Edward S. Crisp*, M.R.C.S. 8vo. pp. 350. Plates. Churchill, 1847.

II. Observations on Aneurism and its Treatment by Compression. By *O'Bryan Bellingham*, M.D. 12mo. pp. 180. Churchill, 1847.

Mr. Crisp's Treatise is an expansion of the Essay for which the Jacksonian Prize was adjudged in 1844. The institution of this prize has been eminently conducive to the progress of surgical science. Several of the Essays which have gained it have since become standard works upon the subjects to which they respectively relate, and the names of a considerable number of the prize-holders are now European. Mr. Crisp's work, although not admitting of comparison with those of some of his predecessors on the list in originality or novelty of views, is, nevertheless, a very creditable production; and one of which the general practitioners have reason to be proud, as coming from one of their own body, unprovided with any special opportunities in the shape of public appointments, but possessing great zeal and untiring industry in the pursuit of medical science.

After briefly describing the anatomy of the arteries, (assigning to them *four* separate tunics,) and presenting a table of the very varied appearances which those of the chest and abdomen offered in 102 cases of death from different causes, Mr. Crisp goes on to consider the subject of *Arteritis*.

He observes very truly, that this has been but little investigated, in consequence of the hasty and imperfect manner in which *post-mortem* examinations are performed. In the case of the *Aorta*, the signs of its inflam-

mation are so various, and its complication with heart disease so frequent, as generally to defy accurate diagnosis. The only morbid appearances upon which we can depend are more or less permanent reddening, and a pulpy and swollen appearance of the inner and middle tunics—deposits of lymph being also occasionally found in the inner coat. In a few cases the abdominal aorta has been found obliterated by fibrinous coagula. "Although these coagulations are probably in some instances the effects of inflammation, they may, I think, occur from deficiency of the heart's action, combined with a roughened and diseased state of the coats of the aorta. Some of these obstructions may take place a short time before death when the *vis a tergo* is not sufficient to propel the blood through the diseased tube. John Hunter says that the blood will not coagulate in a healthy artery, an opinion which I believe to be correct." P. 29.

When the inflammation attacks the *arteries of the extremities*, its diagnosis is less obscure. Burning, shooting, or pricking pain and tenderness in the course of the vessels are often followed by absence of their pulsation, and with a disposition to dry gangrene of the limb affected. In the aged the inflammation is more subacute, but the tendency to grangrene is greater. A common pathological effect is the obstruction, or even obliteration, of the calibre of the vessel by adherent fibrinous deposits—these extending even sometimes to the collateral and terminating branches.

"Some high authorities believe that arteritis is not present in the dry gangrene of old people. I have paid much attention to this complaint for many years and have had the opportunity of inspecting the bodies of several who have died of it, I have invariably found the lining membrane of the artery of a red appearance, the vessel often obstructed by fibrinous coagulum, some distance above the gangrenous part, as well as in the arteries near the seat of the disease. * * * * *

"Many who, like Dr. Carswell, believe that dry gangrene is the result of debility and not of inflammation, appear to have forgotten that inflammatory diseases are very common in old persons. According to the researches of MM. Hourmann and Dechambre at the Salpêtrière, inflammation is the most frequent cause of death in old persons. How often the aged are affected with cutaneous inflammation of the legs; and reasoning from analogy, why, let me ask, may not the internal coats of the arteries be subject to the same engorgements? We have, I think, abundant evidence to show that ossification alone is not sufficient to produce the disease, as this deposit exists to a great extent in the majority of individuals who have attained advanced age. The most common of the exciting causes of acute arteritis in the aged is, I believe, the detachment of a portion of the bony plate of the artery. The great toe, a part the most exposed to mechanical injury, is often the first affected." P. 34.

In treating arteritis, we must resort to the same measures we employ for combating inflammation of other organs: but in old subjects, in whom the disease attacks vessels already much altered in structure by bony deposition, Mr. Crisp has found most benefit from the employment of large doses of opium, combined with a mild, non-stimulant diet. When gangrene has supervened, a line of demarcation having formed, the patient being young, and the collateral branches sufficiently enlarged, Mr. Crisp believes amputation is indicated.

Several cases, which have occurred, either in the author's own practice or in that of his friends, some of which are of a highly interesting charac-

ter, are related in illustration of the above opinions. That dry gangrene, occuring in the young and robust, is very commonly produced by arteritis may be readily allowed; but we doubt very much whether the same term should be applied to the perverted action of the already diseased vessels of the aged. After all, the inquiry is merely a speculative one, for neither the author, nor any one else, we suppose, would be willing to treat the gangrene occurring in these subjects antiphlogistically.

Arterial Deposits.—These, Mr. Crisp believes, are always situated in the subserous tunic, and not in the fibrous coat, as generally stated. The innermost membrane may often be easily removed, it may be broken by the large bony or atheromatous deposits beneath it, but it is never found ulcerated. The fibrous coat may be reddened or softened by inflammatory action, and in old age it is preternaturally dry, but the deposits are not seated within it. It is liable to hypertrophy or attenuation, without change of structure. Mr. Crisp refers to the frequent occurrence of, in *young subjects*, numerous small spots of opaque white deposits along the origins of the *intercostal arteries*, the membranes around the spots being seldom altered in colour. The *ossification of the arteries* of old persons is generally preceded by an atheromatous or cartilaginous condition of the portions of the tubes affected.

"Writers on this subject appear to think that, if bony deposition takes place to any extent, the elastic property of the vessel must be entirely destroyed. This error has probably arisen from the inspection of dry specimens, in which the patches become consolidated and sometimes overlap each other. If the arteries be examined soon after death, the intervening substance will be found to retain its elasticity. The seat of these deposits is various, and large portions of the artery may be perfectly free from them, whilst others contain them in great abundance. I have recently examined the body of a woman, 84 years of age, in whom the ascending aorta was in a normal state; but the abdominal and thoracic portions were covered with bony patches. Many persons have lived to a great age, in whom these lesions have existed to a great extent. I think, whether this condition is the effect of inflammation or otherwise, it may be often looked upon as the result of the wear and tear which the arteries, like the other parts of the body, are subjected to; and probably it is a wise provision of Nature to accommodate the circulatory apparatus in old age to the lessened supply of blood required by the secretory organs. These depositions are comparatively infrequent in the young, although instances are recorded of their occurrence at an early period of life." P. 73.

Although the elastic property of the arteries containing osseous deposit may not be entirely destroyed, it must manifestly become much and proportionally diminished. Indeed, Mr. Crisp refers the frequent rupture of the cerebral arteries, and the consequent effusion of blood in the neighbourhood of the corpora striata, to the diminution of their elasticity, by reason of the cartilaginous or atheromatous deposits consequent on chronic inflammation. He suggests, also, that a diseased and consequently obstructed state of the cerebral arteries may give rise to an impaired condition of the functions of the eye and ear.

After detailing several interesting cases of Disease of the Aortic Valves of Abdominal Pulsation, the author proceeds to the principal subject,

ANEURISM.

Mr. Crisp is of opinion, as the result of an extensive investigation of morbid specimens, that the *fibrous* coat of the artery is that first affected. He objects to the present terms designating the different kinds of aneurism, but we doubt whether those he suggests will prove very acceptable. "If the term endogenous were substituted for that of spontaneous, when speaking of aneurisms which arise from lesions of the inner coats; and the term exogenous or traumatic applied to those which are produced by external division of the arterial walls, I think the nomenclature would be more appropriate. The various names before alluded to might still be retained to distinguish the varieties of aneurism." After referring to the statements of various authors upon the subject of the proportionate liability of the different arteries to aneurism, he furnishes us with a summary of an extensive table—a most useful *catalogue raisonnée*—which he has constructed of all the cases of aneurism (551) that have been recorded in the British periodicals from 1785 to the present time.

Thoracic Aorta	175	Innominata	20
Pulmonary	2	Carotid	25
Abdominal Aorta & Branches	59	Cerebral	7
Common Iliac	2	Temporal	1
External Iliac	9	Ophthalmic	1
Glutæal	2	Subclavian	23
Femoral	66	Axillary	18
Popliteal	137	Subscapular	1
Posterior Tibial	2	Brachial	1

Total, 551.

The influence of *sex* is very great, the much larger proportion of males arising, the author believes, from the nature of their occupations. In women, however, whose necks are so much exposed, the occurrence of carotid aneurism is nearly as frequent as in men, viz. 12 to 13 cases. In the whole 551 cases the proportion of females is rather less than one-eighth. In the 243 aneurism of the aorta, pulmonary and cerebral arteries, it amounts to about a fifth. In the 308 cases of external aneurism there are 276 males to 32 females; but in 21 examples of dissecting aneurism the females amount to 14, the males to 7. The disease usually occurs between 30 and 50 years of *age* (it did so in 327 of the cases,) and after 60 it becomes comparatively rare. Although the usual *predisposing cause* is supposed to be a diseased condition of the arterial coats, Mr. Crisp believes that it may result from injury done to an artery perfectly free from disease during muscular efforts. The most common of the *exciting causes* of aneurism, indeed, whether internal or external, are violence and sudden muscular effort. The disease seems to be of much more frequent occurrence in England than elsewhere, which the author is disposed to attribute to the more laborious exertions employed by the inhabitants of this country, and their great addiction to the use of spirituous liquors. Sailors and soldiers, and especially the former, are particularly liable to it. Agricultural laborers, owing probably to their more temperate lives, are comparatively free from it. According to the accounts Mr. Crisp has been able to obtain, aneurism seems to be of very rare occurrence in the East and West Indies.

The author considers the various forms of Internal and External Aneurism in full detail, and we regret that we cannot follow him; but we devote the remainder of the space at our disposal to the important subject of—

The Treatment of Aneurism by Compression.

Of this, the greatest improvement of modern times in this portion of surgery, Mr. Crisp gives us but a defective account, furnishing no indication of acquaintance with the principles of its employment, which distinguish it from the mere revival of a practice heretofore frequently attempted and abandoned. For the suggestion, development, and verification of these, we are indeed solely indebted to Dr. Bellingham; and we feel surprised that Mr. Crisp has omitted all mention of the great credit due to him. It is true, he says, that we are indebted to the "Dublin Surgeons" for the revival of the procedure; but this statement is too general, many of the Dublin surgeons being just as ignorant of the true principles of its application as those of other places. To the prevalence of the erroneous opinion that the present procedure is a mere revival of former ones, which, from their inefficiency, fell into disuse, we must attribute the extraordinary remissness which the general body of surgeons have shown in putting it into force. Dr. Bellingham's republication, in an enlarged form, of his essay upon the subject, formerly favourably but briefly noticed in this Review, will doubtless do much to remove the misconceptions which prevail upon the subject.

Compression, as originally employed, was directed over the aneurism itself, and was even then successful in comparison to the old operation, which consisted in cutting into and clearing the sac of its coagula, and then tying the vessel above and below the opening—an operation so fatal, that Pott and the best surgeons preferred amputation to it. Compression so employed was however too painful, tedious, and uncertain, to obtain other than occasional success; and the greater simplicity and safety of the Hunterian operation naturally diverted attention from attempts at improving the mode of its application. The failure of this in some cases was the means of reviving attempts at cure by compression with the important improvement of directing this over a healthy portion of the artery, instead of the aneurism. Owing, however, to the objects of the pressure being supposed to be the total arrest of the stream of blood through the aneurism, and the securing adhesion of the parietes of the vessel as from the ligature, the compression was so painful and so continuous that few could endure it; and although a very few cases were thus treated with success in France, and one or two in Dublin, it was not until 1843 the procedure met with any favour here. From this period, however, cases have multiplied, so that Dr. Bellingham is now enabled to present us with the accounts of 27 in which compression has been employed. This gentleman first showed that the degree of compression need not be so excessive as once supposed; and that it may be made bearable by applying two separate instruments to the limb, and compressing the vessel alternately with one of these, while the other is relaxed. He first furnished the true rationale of the employment of compression. The following are among his observations, made at the Surgical Society of Ireland in 1843.

"It would appear that it is not at all essential the circulation through the

vessel leading to the aneurism should be completely checked, but rather the contrary; it may perhaps be advantageous at first, for a short period, by which the collateral circulation will be more certainly established; but the result of this case, if it does no more, establishes the fact, that a PARTIAL CURRENT *through an aneurismal sac will lead to the deposition of fibrine in its interior, and cause it within a few hours to be filled and obstructed, so as no longer to permit of the passage of blood through it.* Pressure, so as altogether to obstruct the circulation of an artery, must necessarily be slower in curing an aneurism, as it must, in some measure, act by causing obliteration of the vessel at the part to which the pressure has been applied; whereas, a *partial current through the sac enables the fibrine to be readily entangled in the parietes of the sac in the first instance, and this goes on increasing until it becomes filled;* the collateral branches having been previously enlarged, the circulation is readily carried on through them." P. 128.

If the process Nature adopts in the *spontaneous cure of aneurism* had been more attentively watched by surgeons, they might sooner have arrived at these conclusions. It will be found she effects this by depositing fibrine in concentric layers, for which a current of blood through the sac is essential. Writers speak of the *deposition of fibrine* and the *coagulation of blood* in the sac as if these were one and the same thing; whereas both their physical characters and the requisites for their production are quite different. For the production of coagulation a degree of compression which prevented all ingress of blood would be requisite; while fibrine is separated whenever the circulation through the tumour is only diminished. Even the *ligature*, when placed at a distance from the aneurism, operates by producing the diminution of the current through the sac favourable to deposition. If coagulation only took place, which indeed is the case when the ligature is applied close to the aneurism, we should much oftener see suppuration of the sac or secondary aneurism produced.

"Every thing which has now been said makes it probable that the ligature of the artery at a distance from the sac, and compression of the vessel at the cardiac side, effect the cure of the aneurism in the same way. It has already been shown that the mode in which Nature brings about a spontaneous cure is precisely similar; which of itself is a strong argument for its correctness. But there is another circumstance which tends still further to confirm it, viz. that several phenomena connected with the surgical treatment of aneurism which were hitherto obscure and inexplicable, upon the theory that the coagulation of the contents of the sac always followed the operation by ligature, are readily explained by this theory. For instance, we have seen that a slight pulsation is not unfrequently felt in the sac subsequent to the operation. Now, this theory not only explains its cause, but it shows why it ceases after a short time, and why, instead of being an unfavourable sign, it should rather be regarded as a favourable one. By this theory alone, we can account for the artery being obliterated after the ligature at the point from which the aneurism springs, while it remains pervious between the ligature and sac. Lastly it enables us to explain why suppuration of the sac should occur after the operation in one case, and not in others apparently similar; and how a secondary aneurism forms in certain cases a long time after the operation, while in general it must be a rare result of the ligature." P. 147.

The last chapter of Dr. Bellingham's little work is occupied with contrasting the advantages compression possesses over the ligature in respect to its greater simplicity, safety, certainty, and permanency, and its applicability in a variety of cases where the operation by ligature would not be considered justifiable, or when the patient refuses to submit to it. He

likewise replies to the few objections which have any apparent plausibility; but these are so easily disposed of that we need not cite their refutation. In our opinion, he has conferred a signal benefit upon the art of surgery by his improvement of the mode of employing pressure, and upon the science by his ingenious and philosophic exposition of its operation. We quote his final conclusions:—

"1. The arteries to which compression is applicable being far more frequently the subject of aneurism than those to which it is inapplicable, compression is calculated to supersede the ligature in the great majority of cases.

"2. The cure of aneurism by compression upon the artery between the aneurismal sac and the heart, according to the rules laid down here, is accomplished by the gradual deposition of the fibrine of the blood in the sac, until both the latter and the artery at the part are completely filled. The process is in fact exactly similar to that by which Nature effects a spontaneous cure of aneurism.

"3. Such an amount of pressure as would cause inflammation and adhesion between the opposite sides of the artery at the point compressed is never required.

"4. The pressure should not be so great as to interrupt the circulation in the artery at the point compressed; an essential agent in the cure being that a current of blood should pass through the sac.

"5. Compression by means of two or more instruments, one of which is alternately relaxed, is much more effectual than by any single instrument, and in many instances the pressure can be maintained by the patient himself.

"6. The treatment of aneurism by compression does not involve the slightest risk to the patient, and if persevered in cannot fail of effecting a cure.

"7. A cure of aneurism effected by compression, according to the rules laid down here, must necessarily be permanent; and in every case in which a cure has been accomplished, the patients have remained well subsequently.

"8. The femoral artery remains pervious after the cure at the point at which the pressure had been applied, and no morbid change of any kind is to be detected in either the artery or vein at the site of the compression.

"9. When a cure is effected by compression, the vessel is obliterated only at the seat of the aneurism, and the artery at this part is eventually converted into an impervious ligamentous band.

"10. Compression effects the cure of aneurism by more simple and safer means than the ligature, while it is applicable to a number of cases in which the operation is contra-indicated or inadmissible.

"11. Compression is not necessarily a more tedious or more painful method of treating aneurism than the ligature, while it is much more certain, more likely to be permanent, and is free from all danger.

"12. Compression, according to the rules laid down here, has little analogy with the old method which went by this name; and in fact has no greater resemblance to it than the Hunterian operation had to the operation for aneurism which it superseded." P. 181.

We have no space for any further notice of Mr. Crisp's work; but may observe that his account of the various forms of aneurism, and of the accidents, &c. to which arteries are liable, is very complete; but that the portion of the book relating to Diseases of the Veins is exceedingly meagre. Upon the whole, the work is a valuable contribution to surgery. It is illustrated by some well-executed lithographs.

RECUEIL DE MEMOIRES DE MEDECINE, DE CHIRURGIE, ET DE PHARMACIE MILITAIRES. Vol. LXII. Octavo, pp. 400. Paris, 1846.

IN availing ourselves of the contents of the successive volumes of this "Recueil," issued under the superintendence of a Committee of the Council of Health of the French Army, which selects from the various documents forwarded by the medical officers of the service those that it deems most fitting for publication, we have often regretted that our own military medical authorities have not, in a similar manner, contributed to the diffusion of the ample and valuable materials which have been, from time to time, deposited in such abundance in the archives of the war department. Something of the kind has been done of late by the publication of the Army and Navy Medical Statistical Reports; and it is to be hoped that the universal approbation bestowed upon these valuable works will incite those in power to continued analogous undertakings.

The present volume contains some interesting papers, which we proceed to notice.

I. FRAGMENTS FROM THE SURGICAL CLINIC OF M. *Sedillot*, Senior Surgeon to the Hospital of Instruction at Strasburg.

1. *Purulent Infection.*—M. Sedillot believes that authors have too generally regarded this affection as constantly fatal in consequence of their only taking into consideration extreme cases. He establishes a distinction between *purulent infection* and *metastatic abscesses*. As long as the disease is confined to the former condition it may be cured; if there are abscesses only of small size, or few in number, all hope is not extinct; death only being inevitable when these are very numerous or large, or open into the pleura, the articulations, &c. The effects vary much also, not only according to the quantity of pus mingled with the blood, but also according to its qualities—the pus from a phlegmon producing much less deleterious effect than a sanious pus. Wounds of the perineum, in which there is a mixture of pus and urine, produce, even when a suppuration is not very abundant, fatal effects in a very brief space of time. It may be replied to the statement that the less advanced cases of purulent affection recover, that such were not examples of the disease at all; but M. Sedillot believes the pathological changes induced in man and animals from this cause are the same, and numerous experiments upon these last have proved to him—1. That a small quantity of pus injected into the veins only produces slighte ffects. 2. If the injection be repeated for several successive days, thirst, shivering, &c. are produced; but the animal continues to live if they are then discontinued—so that we must kill it in order to observe the pathological alterations at this period, such as patches in the lungs, emphysema, &c. 3. If a new portion of pus be daily injected, death takes place, always producing the same changes.

The lungs are the organs in which pus is found to be most frequently

deposited in this affection; then follow the pleuræ, the joints, the liver, and the muscles. Although veins are constantly found leading from the source of pus, in a great number of cases, no trace of *phlebitis* is visible. After amputations, in deep-seated phlegmons, in chronic suppuration, caries, &c., it is always by means of the divided or eroded veins that a direct communication between the purulent centre and the circulation is established, and the mixture of pus with blood which this gives rise to is one of the best ascertained phenomena of the disease. The constant obliteration of the veins by coagula, even in the cases in which they are inflamed, is, contrary to the statement of most authors, an exceptional occurrence. The coagulum, when it exists, does not adhere to the walls of the vein, but floats in the pus, having an elongated, fusiform, shape. If it is interrupted from place to place, the blood remains fluid in the intervals, having lost its red colour, and become converted into a sanies by admixture with pus.

Recognizing different stages of this affection, and its curability in some of these, M. Sedillot enumerates the following indications of treatment. 1. Combating the inflammatory symptoms, if intense, by bleeding, especially local. 2. Modifying the surface secreting the pus, in the case of a wound. This is to be done by stimulant lotions or baths, or injections of aromatic wine. In this way the vitality of the tissues becomes modified, and the pus changed in qualities, or its secretion arrested. 3. Furnishing ample exit for pus by prompt incisions if necessary. 4. The frequent renewal of dressings. 5. The use of the actual cautery. This is often very efficacious. 6. If purulent infection seems threatened after attempting union by the first intention, the commencing cicatrix is to be broken, and the edges of the solution of continuity irritated. 7. A revulsive action of the secretory organs is to be maintained, especially by the use of purgatives. 8. Cold fluids should be drank in abundance, to maintain the venous system in a state of repletion, and diminish its absorbing powers as much as possible. 9. Counter-irritants should be applied in the vicinity of any organs suffering from derangement of function. 10. Tonics are not indicated until the febrile action has declined, and true prostration set in. 11. In the case of symptoms of infection occurring in a carious limb, amputation offers the best resource, if its performance be not too long delayed.

2. *Union by the First Intention.*—In a section upon extirpation of the cervical glands, M. Sedillot takes occasion to express his opinion upon the union of wounds by the first intention. He is not an advocate in general for attempting this, as, in the hands of himself and the other surgeons of Strasburg, it seldom succeeds. When it is incomplete, the inconveniences it gives rise to are not compensated by its advantages, and the wound is healed more certainly, and nearly as rapidly, by dressing it with a barred compress and lint. When immediate union occurs, sometimes the integuments are alone agglutinated, the deeper portions of the wound not having undergone reparation. By the other method, the healing takes place from within outwards. In determining the question, *climate* must be taken into consideration. In the high temperature and dry air of Egypt union by the first intention produces wonderful results, and M. Serres has ob-

served the same thing to some extent at Montpellier: but in cold and damp climates, like those of Paris and Strasburg, such a mode of union is only the exceptional case. Alluding to the same subject, when treating of amputation of the thigh, M. Sedillot adds:—

"The most exact approximation cannot prevent an excavation which serves as a reservoir for the fluids exuding from the wound. A pultaceous, greyish matter, a species of false membrane formed of the fibrous portion of the blood, which should be eliminated, is retained within the wound, and converted into a purulent and fetid sanies, which may give rise to severe consequenses, such as phlebitis, purulent infection, &c. The bone, macerated in the pus, becomes denuded of the periosteum and necrosed. All this while adhesion of the skin, which is easily produced, masks what is going on in deep-seated parts until tension, tumefaction, and heat reveal it. Unless incisions are practised, or the cicatrization torn open, collections of matter are formed, and the *morale* of the patient, who believing himself nearly cured finds that all is well nigh to suffer again, becomes injuriously influenced.

"More security attends a different practice. After the ligature of the vessels the blood is to be carefully staunched, and the wound is neither to be closed by strapping nor crammed with lint. A barred compress spread with cerate, and a few pieces of fine lint, are alone to be applied, the muscles of the limb being supported by a roller. During the first days the surface of the wound becomes covered with a greyish, pultaceous crust, which however is soon detached, leaving red, healthy granulations—the precursors of cicatrization. The fears of the patient of the first dressing have been especially objected to his plan: but such objection is only valid when the dressing is performed prior to the complete establishment of supuration, and ceases to be so if this be delayed to the fourth or fifth day, when the compresses are nearly detached by the suppuration, and their removal gives relief instead of pain to the patient. After this the wound should be dressed daily, for frequent dressings are the best means for the prevention of danger from the sojourn of pus."

For the union of wounds by means of *pins, needles*, or the *twisted suture*, M. Sedillot gives the following precepts.

"When the solution of continuity is not very considerable, two or three points of suture are sufficient, as strips of plaster may be applied along the rest of its course. Surgical incisions or accidental wounds of more than certain extent require a greater number. In this case we should apply the first one at about the middle of the incision, as we can in this way judge much better of the disposition of the two lips of the wound, and remedy any imperfections or defects of relation. Before passing the needle we must smear its point with grease, taking especial care to free our fingers from this afterwards. When we wish to withdraw it, we should carefully free its point from all matters which can impede its exit, support the edges of the wound by our fingers while removing it, and leave in the threads for some days longer. The third or fourth day is generally the proper period for the removal of the needles, unless severe pain, tension, redness of the edges of the wound, or inflammation accompanied by fever, occur. In these cases we must remove them at once, and combat the accidents by appropriate measures."

3. *Extirpation of the Amygdalæ.*—The humid climate of Stratsburg seems very favourable for the production of angina and consequent tonsillar hypertrophy, for in one year M. Sedillot performed the operation of extirpation no less than thirty times. The operation may be performed by means of Fahnestock's amygdalotome, or by the bistoury. The former,

requiring the aid of no other instrument, simplifies the operation, frightens the patients much less, and causes less pain: but it is a bad instrument, as only a part of the structure is removed, and resolution very rarely follows. The straight, probe-pointed, narrow-bladed bistoury, protected by a piece of linen to within a certain distance from its extremity, is the best one, requiring however the assistance of a hook or forceps. The section should be made from below upwards, with the double object of not covering with blood the parts yet to be divided, and being able, in case of need, to suspend the operation before complete section, the divided portion still held by its upper segment not being then liable to produce mischief by falling back upon the glottis. In operating upon children, a difficulty presents itself in keeping the mouth open. Cork is usually used for this purpose; but if it is too small it is easily displaced, and if too large it takes up too much space. M. Sedillot employs with advantage gold or silver *thimbles*, by which an assistant guards his fingers, and is then easily enabled to keep the mouth open as widely as may be required.

II. Several Cases of Congelation. By Dr. *Shrimpton.*

An episode in the Algerian Campaign of 1845-6 furnishes the material for this paper. During the retreat of a column of the army (its numbers are not stated) from Bou-Thaleb in January, its progress became arrested by heavy snows for two or three days, and, provisions falling short, above 200 men died on the route. The particular detachment which Dr. S. accompanied lost no one, nor did any one in it suffer *en route* from congelation, which results, he attributes, to the beneficial effect of a few drops of brandy which he was twice enabled to distribute with some bread, and to the circumstance of the men when lying down keeping huddled close together. Although a strong north wind blew, the cold was by no means intense enough to account for the large mortality. Prior to the retreat, too, the labours of the soldiers had not been excessive, nor had they been subjected to any privations. The great scarcity of food, and the fact of some of the most robust perishing, seem to indicate that defective alimentation was the cause of these results; man requiring an energetic nutrition to resist the influence of low temperatures. The phenomena accompanying death by congelation, Dr. S. thus describes:

"I have frequently been a witness of the symptoms which precede death from congelation. The persons experience a general sense of stiffness or numbness, accompanied sometimes with pains in the limbs and groins. Muscular contraction is performed with difficulty. The face becomes red and swollen, the lips bluish, the eyes projecting, and the hands red and tumid. The pulse is small and feeble and the respiration slow. All these symptoms rapidly increase. The eyes then take on a wandering expression, the step is uncertain and vacilating, and at last the man falls down. The skin of the hands cracks, and frequently gives issue to from two to four ounces of blood. The patient preserves his senses, but has all the appearance of a drunken man. If he is raised up, he falls down again, and he cannot be kept in a litter without being tied to it."

Upon the arrival of the column at Setif, 532 patients were admitted into the hospital on account of the effects of congelation. Of these, 186

were cases in which phlyctenæ or slight ulcerations followed congestion of the feet or hands, 60 in which the loss of one or more of the nails occurred, and 221 in which superficial gangrene took place without implicating the bones or joints. In 52 cases, in which these latter parts became affected, amputation was performed, three of the patients only dying after the operation, although the wounds for some days put on a very flabby aspect, and were in some parts disposed to slough. Eleven cases perished from sphacelus of the extremities, or consecutive internal lesions. The feet, as might be expected, from their constant exposure to melting snow, suffered in the vast majority of cases; and thus, of 532 cases, in only 26 was there congelation of the hands. The *local* effects of congelation are strikingly *identical with those produced by burning* in its various degrees; so much so, indeed, that the one case could not by mere inspection have been distinguished from the other. In the fatal cases, marked pulmonary and cerebral congestions, or even inflammation, were found, and, as in severe cases of burns, gastro-intestinal irritation was set up, which sometimes proceeded to ulceration of the ileum and colon. The local inflammatory re-action after congelation, was, however, less prompt and marked than after burns, and was often absent, giving rise to extensive lesions after the fall of the eschars. On the other hand, parts which were even blue, or black, from local asphyxia, sometimes promptly recovered their normal condition under the influence of this re-action.

In respect to *treatment*, the patients who seemed in danger of asphyxia, were enveloped in bed-covering, while some hot broth was given them. Very soon after this they fell asleep, and a mild re-action generally became established. In some cases detailed, fatal inflammation of the lungs or membranes of the brain was developed. The milder local effects were generally cured in from four to eighteen days, pains however persisting in the joints of the hands and feet for one or several months. The wounds were simply dressed and the limbs enveloped in flannel moistened with camphorated oil. The benumbed parts were gently rubbed until re-action became established. The phlyctenæ were preserved as long as possible, especially in the slighter cases, and, when the parts were much engorged, incisions were practised in order to prevent the spread of gangrene. Warm fomentations and poultices were also employed during the first days. All those who were enabled to leave their beds were encouraged to do so and go into the airing courts, while the wards, bedding, &c. were thoroughly cleansed and ventilated—a precaution rendered essential by the crowded state of the hospital, 528 patients occupying a space intended only for 200. Substantial diet, and as soon as possible, wine were prescribed.

III. On the early Use of Injections in Gonorrhœa. By Dr. *Poullain*.

The employment of injections as an abortive mode of treating gonorrhœa, a practice never received with much favour in this country, has recently re-excited great attention in France. M. Poullain, from his position of Surgeon to the Military Hospital at Lyons, has had great opportunities

of investigating the practice, and in the present very able paper details the results he has obtained.

Regarding gonorrhœa as a *specific* inflammation of the urethra (*blenno-urethritis*), he believes our object should be to destroy this "*specificity*," and reduce the disease to the condition of an ordinary or "normal inflammation." This he believes is best accomplished by the prompt use of injections. He animadverts upon the opinion advocated by Baumès and others, that the sudden suppression of the discharge in this way is sometimes followed by the outbreak of *secondary symptoms*. In 600 patients treated by the abortive method by Dr. Poullain, during the last fifteen years, he has never met with an instance of this, and in 200 other cases, whose subsequent history he has been in the position to assure himself of, not one occurred. He fully agrees with M. Ricord, as we believe most other observers now do, that the apparent examples of constitutional symptoms following gonorrhœa, are really examples of *concealed urethral chancre*. To this opinion it has been objected, byBaumès and others, that the chancrous matter applied at the orifice of the urethra can scarcely be expected to travel to the prostatic portion before producing its local effects. But these observers are guilty of the inconsistency of attributing this very power to gonorrhœal matter when they state the vicinity of this portion of the urethra to be its commonest source. But the possibility is best decided by facts. A soldier who had been long fruitlessly treated for gonorrhœa was carried off by typhus, and, on examining the urethra, a well-characterized chancre was found at 10 or 12 lines behind the *fossa navicularis*. In two other cases of gonorrhœal patients, dying of other diseases, well marked cicatrices of venereal ulcers were found at nearly three inches from the orifice.

As in all other inflammations the prevention of the formation of a *chronic* stage, and the consequent pathological changes in the parts implicated, is highly important, and M. P. believes this has not hitherto been accomplished by injections, because they have been employed too tardily. Reversing the ordinary procedure, he always uses them in the *acute stage, rarely in the chronic one;* and it is their having been confined to this last that has obtained for them the ill-reputation of producing various ill-effects which really result from the disease having already passed into a chronic condition. The object in recommending early injection is not to procure the cessation of the discharge, but a change in the character of the inflammation; for, this once accomplished, as the diminution of pain and other symptoms prove it to be, the discharge usually soon disappears of itself, or is easily removable, by means of balsams and revulsives. The prejudice against their use, founded upon their supposed tendency to the production of *stricture*, is a very erroneous one; for we cannot suppose this to be formed without the prior existence of inflammation.

In fact, they are the best means for the prevention of this affection. If stricture arises from the use of injections it ought to be formed at the various portions of the canal the stimulating fluid comes in contact with; but in the vast majority of cases it is so in a very limited portion of the canal, and one to which the injection hardly ever obtains access. If, too, we question persons suffering from stricture we find that they have rarely made use of injections, and that they have found the canal become narrowed

only after protracted duration of the discharge. Injections are indicated then during the whole of the acute stage, and no other means is so efficacious for calming the ardor urinæ, and for relieving painful erections. All these symptoms, however violent they may be, yield, as if by enchantment, to the use of injections, and, so far from contra-indicating them, form the strongest argument for their employment. "In this way I constantly arrest numbers of gonorrhœas without any ill-effects resulting." Dr. Poullain employs the following modification of Bell's formula—*Sulph. Zinci, a scruple, Aq. Destil.* 10 *oz.* Dissolve and add *Liq. Plumb. Acetat. gtt.* 20, *Laudanum of Sydenham* 1 *drachm.* The last ingredient he regards as an important one. This injection is to be employed as soon as slight irritation, redness, or discharge is seen, the sooner the better. Three jets should be thrown in three or four times daily and the fluid retained in the canal by slight pressure for half a minute. In most cases the pain and inflammation disappear in 24 hours, or at farthest in a day or two, and the patients often are quite surprised at the rapidity of the relief. The injections must, however, be continued until the discharge becomes sparing and colourless, which generally occurs in about from five to ten days. They may then be discontinued, as the discharge soon ceases, or may be removed by ordinary means. Although injections act most beneficially when employed at the very commencement, they are still useful at a more advanced stage, and we must not allow the severity of the symptoms to prevent our employing them, as they will promptly alleviate these. The eight or ten first days constitute the most elegible period for their use, but we may still have recourse to them at the fifteenth or twenty-fifth day when however their success becomes less prompt and less complete; and, although they may be used without danger, they do not always then arrest the discharge; but require the aid of balsams or cubebs administered internally.

Gleet.—The means advocated does not require any special abstinence, save from wines or spirits, &c., and will, as a general rule, effect a cure in fifteen days, and frequently in ten, five, or fewer still. When injections have been delayed until the decline of the acute stage the discharge may persist under the form of *gleet* or military gout. The persistence of gleety discharge seems dependent upon four different conditions of the urethral mucous membrane. 1. A simple state of local relaxation. 2. A superficial but circumscribed ulceration. 3. A concealed chancre; and 4. A commencing or already formed stricture. The first of these is the most common variety, and generally arises from an atonic state of the exhalent vessels of the urethra, consequent upon a condition of excitement which had been treated by demulcents, antiphlogistics, and diuretics. In such cases, *tonic* rather than astringent *injections* best succeed, and Dr. P. usually employs aromatic wine, diluted with equal parts of water, or cold water to which a sixth part of alcohol has been added. In some cases, cold lotions to the penis and scrotum succeed. This variety of gleet always exists without pain, even during the passage of urine or instruments, or the presence of erections. The discharge varies in appearance, according to the manner it has been already treated. It is opaque, milky, or yellowish if it has been left to itself, or what is nearly the same thing, treated by de-

mulcents. It is clear and transparent if injections have been employed. Contagious in the first case, it is never so in the second—an additional advantage for the abortive method. (An advantage, if true, which is not the case.—*Rev.*)

(2.) If this discharge persists for a certain period the portion of the mucous membrane wherein it originates becomes *excoriated*, constituting the second variety, which may always be easily recognized by the change in colour of the discharge, which becomes a dirty white or sanguinolent. In urining too, the patient experiences a sharp pain at the excoriatd spot, and he is much tormented by nocturnal erections. In this case, the *nitrate of silver* (1 to 2 grains to the ounce of water) forms the best injection, throwing it in once daily, while in other cases, much success attends its use in the solid form. The discharge, however, after disappearing returns again with the greatest ease, and upon the slightest excess: and in such cases, a large blister applied to the upper and inner part of the thigh, kept open for a fortnight or even a month, rarely fails in entirely suppressing it.

The *third* variety depends upon a *concealed chancre*. The author relates two remarkable cases of gleet which, obstinately resisting all other modes of treatment, yielded readily to the use of mercury, which such obstinacy and the local suffering induced him to administer. In this way he explains similar examples of the utility of mercury in gonorrhœa recorded by various authors. In consequence of the passage of the urine, the cure of a chancre, under such circumstances, is always tedious and difficult; and even those which are situated near the orifice of the urethra are months in healing, protect them how we may. The diagnosis of a concealed chancre is usually obscure, and we must much depend upon the prior history of the case and upon the presence of certain symptoms, especially an almost constant pain at one point of the canal, increased by urining, by external pressure, or the passage of an instrument. Erections, too, are often as frequent and as painful as in the acute stage of the disease. The discharge is of a sanious and purulent character, sometimes sanguinolent, and after urining, drops of pure blood may be passed.

The *fourth* variety, thatd ependent upon stricture, is to be relieved only by the cure of the latter. Stricture, as a consequence of venereal effection, may form in two modes. In the first, the canal in one or more places is obstructed by a kind of vegetation or fleshy excrescence, which M. P. has often met with in opening the urethra after death, sometimes disposed in a parallel direction to the canal, and at others forming a transverse bridle. Upon the passage of an instrument, during life, these are sometimes detected by the readiness with which they bleed. This species of stricture is neither the most common nor the most difficult of cure. The removal of the vegetations by cauterization, and the subsequent employment of mercury constitute the treatment. The most common variety of stricture is formed very gradually, and the calibre of the canal is narrowed, generally just anterior to the prostatic portion, in consequence of the thickening of its mucous membrane induced by chronic inflammation. There is usually but little pain, unless the narrowing be sufficient to produce great obstruction to the expulsion of urine. Bougies form the appropriate means of treatment, and it is an error to substitute cauterization

for them. They must, however, be most perseveringly employed, and allowed to remain in the urethra for a longer or shorter period, or the stricture will soon re-appear.

IV. Frequency of Tænia in Algeria.

From the Reports furnished to the compilers of the Recueil this parasite seems especially to infest the soldier in Alegria, although the relative degree in which it does so would have been better judged of if the respective numerical amount of soldiers in the different localities had been stated. The military hospitals of the interior of France furnished, during the years 1840–6, only two cases of tænia, and those of Corsica no case; while no less than 34 cases were observed during the same space of time in Algeria.

V. Report of the Venereal Patients under the Care of M. Marchal (de Calvi), of the Val de Grace. By *M. Souhaut.*

This is an interesting account of the practice pursued by M. Marchal in the cases of between 600 and 700 patients, who passed through his hands in this military hospital during four months of 1846. The doctrines of M. Ricord upon syphilis were put into practical operation to a greater extent than we should have expected or can approve of. For example, the following positions are, in our opinion, full of the most dangerous fallacies.*

"If the chancre continues simple, that is not *indurated*, there is no cause to fear general symptoms. To this primary law, laid down by M. Ricord, we have met with no one exception. Suppurating bubo is always coincident with simple chancre, and excludes constitutional syphilis. We might say that the inflammation developed in the gland prevents the virus passing further, and that this is eliminated by the suppuration. We have met with facts which throw doubt over the first portion of this second law. Induration, according to M. Ricord, is the first general or constitutional phenomenon of syphilis."

Chancre varies in its form, duration, and nature. In treating of its seat, several cases of *urethral chancre* are detailed. A reddish appearance of the discharge from the canal is much relied upon as a sign of this, added to which, induration may be sometimes felt by external examination, although, in other cases, a simple inflammatory engorgement may be mistaken for this. The two, however, are distinguished by the dispersion of engorgements by suppuration or antiphlogistics. A severe pain at one point in urining, though not pathognomonic of concealed chancre, deserves attention. Chancres attacking the side of the *frænum*, burrow under it until they leave only, as it were, a little bridge, which, containing in its

* For some excellent criticisms upon the views of M. Ricord, as well as for a most masterly estimate of the various debateable points in the history and treatment of this disease, we beg to refer our readers to Mr. Porter's Lectures, publishing in the Dublin Medical Press of the present year.

substance a small arterial branch, remains undestroyed. The duration of the treatment is abridged by the division of this portion, which M. Marchal accomplishes in the manner recommended by M. Ricord. Two threads are, by means of a needle, passed under the frænum, and the posterior one is first tied round this part, giving the patient much pain, which however he does not feel when the other thread is tied. The frænum is divided between these two, and no blood flows. When chancres exist in considerable numbers around the free *margin* of the *prepuce*, a phymosis, sometimes necessitating operation, is often induced. The case is thus expedited, providing the incision can be made at some little distance from the chancre, so that it may cicatrize as a common wound.

In respect to *form*, the chancre is usually circular, or, as in the case of that of the frænum, very elongated. There is every variety as to size; but it is an error to suppose that small chancres are more readily healed. They sometimes seem to require a time inverse to their size. The chancre may be elevated, superficial, or excavated. In respect to this last, there is one form which may be termed the *pigeon's nest chancre*, which is produced when the ulceration, having destroyed the entire thickness of the preputial mucous membrane, has for its basis only the extremely loose cellular tissue which unites this membrane to the external skin. This cellular tissue becomes excavated beyond the borders of the ulcer, giving the pigeon's nest appearance. The same effect is produced by the chancre commencing externally, and destroying the skin of the prepuce down to the cellular tissue. This variety is especially distinguished by its tedious reparative process.

The average period of *duration* of chancre at Val de Grace was 28 days, some healing far sooner, and others much later. Some chancres may, from their rapid disappearance, be well termed *ephemeral*, and yet these possess all the characteristics and virulence of ordinary chancre. These cases may furnish matter for a bubo which does not appear until after the sore is cicatrized, and then may be taken for a primary bubo. These are not the only cases in which bubo may form after cicatrization. Sometimes patients apply a month after their dismissal, having an open bubo without any new infection.

In respect to the *nature* of chancre, as long as it is *not indurated*, it is a mere *local* disease, but no sooner does induration present itself, than a general infection of the system is imminent or acquired. *Simple superficial chancre* may be confounded with *herpes*, but herpetic ulcerations are usually in clustres, are preceded by pruritus, and accompanied by vesicles in some other portion of the prepuce. In M. Marchal's opinion, however, the distinction is of little practical consequence, as general infection does not follow any simple unindurated ulceration, which promptly cicatrizes. In the hospital many chancres were of a *phagedænic* character. M. Ricord describes three varieties of this, the pultaceous or diphtheritic, phagedæna from excess of induration inducing mortification, and phagedæna from excess of induration inducing gangrene. It is the first of these that was met with in Val de Grace. This diphtheritic sore, according to M. Ricord, is marked by absence of induration, the aspect and form of hospital gangrene, and great pain and irritability. This is usually correct, but in one case cited of pultaceous chancre, committing great destruction,

induration existed. The diphtheritic appearance is given this chancre by its surface being covered with the tissue destroyed by the ulcerative process—resembling the pulpous variety of hospital sore—and better named *pultaceous*. There is, however, a form of simple non-destructive chancre, which is covered by a thin white pseudo-membrane, which may be properly termed simple *pseudo-membranous chancre*. As long as this pseudo-membrane continues, cicatrization will not usually take place, but this must not be mistaken for the white pellicle sometimes seen at the circumference of a healing chancre. There were several examples of bleeding or *hæmorrhagic* chancres—usually very tedious in cicatrizing.

Induration.—As this is the point upon which M. Ricord's views may be said to hinge, M. Souhaut enters into the subject of induration somewhat elaborately. M. Ricord considers it as an indication that the economy is infected, and that the disease has become constitutional. M. Marchal hardly goes so far as this, believing rather that it is an indication that infection is imminent, unless therapeutical agents are forthwith resorted to. Induration is not always visible, and must be sought for by seizing the base of the sore between the thumb and index finger. It may often be felt in this way when no-wise in relief, giving the sensation of half of a great pea or a semilunar body under the fingers. There is one form which, to the touch, seems like an empty space under the mucous layer, returning upon itself, like a dry leaf, after the pressure is removed. The induration will vary in degree, according to whether mercury has been employed or not. Until it is removed, slight irritation of any kind suffices to re-produce the ulcer.

Treatment of Chancre by Cauterization.—Believing that as long as the chancre continues open, the danger of general infection of the economy exists, M. Marchal endeavors to bring about a change in its character, and its prompt cicatrization by its destruction by caustic. The *Nitrate of silver* is applicable to a great number of cases, but, in others, where it is desirable to act more deeply, the *Vienna paste* is preferable. Employed on the prepuce, however, this acts too deeply, and gives rise to the pigeon-nest chancre. The cauterization must be repeated as long as there is a pultaceous or pseudo-membranous matter reproduced on the surface of the sore. As soon as the sore puts on a healthy rose-coloured aspect, we must cease. If the chancre is phagedænic, the most active cauterization is indicated. When there are chancres within the prepuce producing *phymosis*, M. Marchal injects a strong solution of nitrate of silver (2 parts to 20) between this and the glans, morning and evening, using injections of opiated aromatic wine in the intervals. This relieves the pain, soon enables the prepuce to be drawn back, an operation is thus usually averted, and the painful tedious wound which results avoided. If called to a patient, while the sore is still in the form of vesicle, it will be the surgeon's fault if this ever become a chancre. A layer of Vienna paste is to be applied to it and beyond its edges. Although so useful for the prevention or changing the character of chancre, cauterization sometimes causes adenitis, but this proceeding not from venereal absorption, but from irritation, soon subsides. This however should prevent our employing caustic,

when from the healing disposition in the sore, it is not required. For local application to the chancre, M. Marchal employs aromatic wine alone, united with laudanum, if there is much pain and irritation, which this much assuages, or with rhatany, tannin, iodine, &c., when the sore is atonic, or retains a bad aspect in spite of repeated cauterization. In some stationary chancres, seated on the glans, diachylon plaister has served to hasten the reparative process; but in chancres of the prepuce this may induce infiltration.

The *induration* requires treatment for two reasons, namely, that it may oppose or retard cicatrization, and that it is a sign of general infection. For the first reason it must be treated with the Vienna paste, and, for the second one, by the internal use of mercury. So, too, induration persisting after cicatrization, slight irritation may lead to the reproduction of the sore, and, as it indicates the continuance of the constitutional disease, as a general rule mercury must not be discontinued as long as it is present. Its disappearance, which is sometimes exceedingly slow, is always to be looked upon as a proof of the satisfactory action of the specific.

Blenorrhagic Epididymitis (*Hernia Humoralis.*)

This disease, in the great majority of cases, is an epididymitis; for, of more than 60 examples in Val de Grace, in only two was the testicle itself affected. On examining the scrotum, the enlarged epididymis may usually be easily felt at some point or another, resembling in shape an acorn and its capsule. The testicle, generally placed in front, retains its characteristic softness, and if pressed, providing the compression do not include the epididymis, no suffering is caused, while acute pain is induced if the epididymis be touched. The error of supposing the testis affected arises from the common occurrence of effusion into the tunica vaginalis, which sometimes becomes exceedingly distended.

The *treatment* employed at Val de Grace as a general rule is antiphlogistic. A general bleeding, and from 20 to 30 leeches being resorted to, repeating these latter (and warm cataplasms) once or oftener. Afterwards, mercurial or opiated camphorated liniments are applied, and pills, containing equal parts of hemlock and calomel given—the action of the mercury on the mouth being prevented by maintaining a lax condition of the bowels by the aid of injections or Seltzer water. The horizontal position and a spare diet are insisted upon. In spite of this treatment, there is usually left an indurated point of the epididymis, which time alone seems competent to disperse. M. Marchal has very frequently put into practice M. Velpeau's plan of evacuating the serum of the tunica vaginalis by means of numerous punctures, and has observed great relief from the pain, and a more prompt resolution of the inflammation, as the consequences. Frequently, however, the fluid is rapidly re-produced. M. Vidal (de Cassis) has proposed the division (*debridement*) of the tunica albuginea in *parenchymatous orchitis*—a variety so rare that in more than 60 cases not one example presented itself. In more than 20 cases M. Marchal tried with great care the plan recommended by M. Songy,* of keeping the scrotum covered with a thick layer of cotton and wool, well supported in a suspen-

* Medico-Chirurgical Review, No. VII. N. S., page 100.

sory against the external ring, by which means according to its author, the patient is enabled at once to walk about. In three slight cases this succeeded very well. In several, so great pain was created as to require the instant discontinuance of the attempt, and the rest of the cases remained stationary (save some diminution of pain), and required recourse to be had to antiphlogistics. Several patients, treated antiphlogistically, found the cotton, after the acute stage had disappeared, and while employing ointments and liniments, a better application than poultices. The plan is only of service, therefore, in slight cases.

Sullo Scorbuto Indagini, Osservazioni ed Esperienze di *Carlo Novellis*, Dottore nelle Facultà Medico-Chirurgica.

[Annali Universali di Medicina, Vol. CXVIII. 1846.

Researches, Observations and Experiments concerning Scorbutus. By *Charles Novellis*, M.D.

The Italian physicians seem to have greater opportunities for the observation of the phenomena of Scorbutus than fall to the lot of their brethren in this part of Europe at the present day—the comparative extinction of a disease which once committed such dreadful ravages, being one of the greatest achievements of modern hygiene. Dr. Novellis, as physician to the military prison of Alexandria, has had a great number of cases under his care, and an abstract of the interesting account of the disease he has furnished to the "Annali Universali," will we doubt not be acceptable to our readers.

The term *Scorbutus* was unknown to the ancients, and Ronseo was the first writer who employed it (in 1564), although long prior to this it was in popular use. Much difference of opinion prevails amongst authors as to whether the disease itself was known to the ancients or not. Dr. Novellis agrees with those who believe in the affirmative, and cites certain passages from Hippocrates and Paulus Ægineta in support of this view.

Predisposing causes.—The principal of these are circumstances which operate changes in the organism, such as tedious convalescence, chronic disease, and cachexia, however induced. In a given number of persons, the indolent and inactive will be those most liable to suffer, and thus, in long voyages, it is upon the marines rather than upon the crew the disease commits its ravages. Hence the necessity of keeping the inmates of prisons employed. A sedentary life, confinement in close places or in the vicinity of foul air, prolonged moral affections—and in fact all that can influence the gastro-hepatic systems, in like manner predispose. If a too inactive life has this effect, so on the other hand have exhausting labours, as the forced marches of armies, &c. The cold and humidity of the climates have been cited as explanatory of the endemic prevalence of the disease in Greenland, Russia, Holland, &c.; but, if this be correct, Dr.

Novellis observes some unknown influences must, in other regions, neutralize the effects of these.

"In our Alps are districts that, for six months in the year, enjoy not the sun's rays, which are damp, and peopled by filthy peasants, who, for months together breathe an atmosphere into which a ray of light scarcely penetrates, and who are yet unaffected by this disease. I had an opportunity of verifying the same fact in the valley of the Chison, when on duty at the fortress of Fenestrelle, near where an immense number of districts contain only damp, dirty, dark caves and huts, in which human beings are shut up with the dirtiest animals, fed with the worst aliments, deprived of all fresh articles of diet, especially vegetables—and yet is the scorbutus unknown to the inhabitants of these Alpine rocks."

Occasional Causes.—Dr. Novellis' investigations lead him to agree with Millman, in assigning indigestible, insufficient, or innutritive food and vehement passions of the mind as the chief causes of the development of this disease; and does not believe in the supposed injuriousness of salted meats, providing these be of good quality and properly prepared. The explanation of the development of the disease during long voyages sometimes defies the ingenuity of the most attentive observers, the vessels being supplied with every appliance conducive to the health of their inmates, while other vessels in defective sanitary conditions entirely escape.

"In this point of view the citadel of Alexandria presents circumstances worthy of attention. It possesses two prisons, one civil, the other military. The former is exposed to the north and is damp, dark, and badly ventilated. The latter has a north-western aspect and consists of two floors, the ground one being a little damp, but the other perfectly healthy. The civil prisoners were not subjected to any labour, but led completely idle lives, only going into the air for two hours in a kind of court. Their diet consisted of 24 ozs. of bread per diem, 4 ozs. of rice morning and evening, with 4 ozs. of fresh or 2 ozs. of dry vegetables, with butter or lard as a condiment in proportion. After two years of such seclusion, no example of scorbutus presented itself. The military prisoners are usually young and robust, are much in the open air, and actively employed. Their diet is somewhat more sparing (24 ounces in all) than the above, except when very laboriously employed, when it is somewhat increased, and a few ounces of meat and wine added. Among them scorbutus is of very frequent occurrence.

"I can only explain this difference by supposing that the civil prisoners, more advanced in years, and leading sedentary lives, are sufficiently nourished; so that a physiological condition of nutrition still being maintained, though amid so many morbigenous causes, they continued exempt from scurvy. I am the more persuaded of the correctness of this opinion, as formerly this civil prison was the receptacle for convicts condemned to hard labour, and who, living upon the poorer diet as the soldiers, were continually scourged by this disease—assimilation in them being in a vitiated condition."

Most writers upon this disease have noticed the agency of the various passions in its production. Dr. Novellis relates the case of an officer confined in a warm, dry, comfortable apartment of the fortress of Fenestrelle, having ample food with exercise in the open air for several hours, but who was firmly persuaded that at a given epoch he should be set at liberty. Disappointed of this he fell into a state of the deepest dejection, under the influence of which scorbutus soon became developed, which obstinately resisted all remedial agency, until at last the desired pardon arriving, joy again illumined his features, and his health was rapidly re-established.

The powerful effect of humidity upon the development of the disease was shown in the military prison of Alexandria, wherein our author observed thirty cases among those who slept on the ground-floor, and but one in those who slept above—the men being all placed under similar circumstances excepting as regards dormitories. He considers that the cold humidity exhaling from walls is especially pernicious, dampness uniting with heat inducing a different class of affections.

The disease has been divided into varieties according to the circumstances under which it has been observed or the nature of dominant theories. Dr. Novellis believes a natural division to be a non-febrile and a febrile variety.

Apyretic or Chronic Scorbutus is by far the most common affection, and perhaps no other disease presents so multiform a category of symptoms. It possesses no one pathognomic sign, several concurrent symptoms being required to characterize it. Of these, the chief are a pallor of the countenance, and an expression of melancholy which is *sui generis* —swollen, red, and bleeding gums—fetid breath—more or less deeply-coloured violaceous spots dispersed over the surface, especially of the lower extremities—pains in the hips and legs—induration or swelling of the calves, knees, or feet—amidst all which infirmities the patient retains his appetite and relish for food, and digests it. These symptoms are liable to endless varieties and anomalies, and none more so than the spots which assume different shapes and colours. Of one of these varieties Dr. Novellis remarks :—

"Few authors make mention of the *granular* eruptions, cases of which are yet by no means rare. The skin of the lower extremities puts on the aspect of what is called 'goose-skin,' each little pustule having a livid brownish apex. Rarely do they enlarge, but some of them which have taken on a furuncular aspect I have opened with a lancet, and found them to consist of only a subcutaneous sac filled with black blood. Sometimes they cicatrize afterwards and sometimes fill again, but they are always insensible."

The progress of the disease may be naturally divided into four stages, namely, the precursory, that of invasion, of increase, and the extreme stage. As the author's description of these in no-wise differs from that usually given, we need not cite it. *Morbid anatomy*, he observes, has, as yet, thrown little light upon the nature of the affection. "The post-mortem examination of a person who has so long suffered in every form and in every system of the economy, always presents to the anatomist a mass of strange and inconstant phenomena, useless as a guide to practice." Few parts of the body indeed are there but offer traces of the diseased actions which have praviled.

Pathology of Scorbutus.—The great diversity of opinions that have prevailed upon this subject sufficiently testifies to its difficulty. To cite only some of these: Hippocrates, Celsus, Willis, Riverio and a number of other writers placed the seat of the disease in the spleen; Hoffman in the liver; Murray in the pancreas; Allen and Boerhaave, in the blood. Milman, Haller and others opposed this hypothesis. Lind believed the disease to consist in a relaxation of the solids with a tendency of the blood

to spontaneous corruption. Milman referred its production to a diminution of the vital powers. Trotter, looking upon it in a chemical point of view, asserted the want of oxygen to be the cause of its production. Kreysig, Tommasini, Baruffi and others, observing the frequency of inflammation of portions of the venous system, resolved the disease into a phlebitis, while Versari believed it to consist in a vitiated assimilation which produced an inflamed condition, first of the venous and then of the arterial system, and ultimately of one or more of the other tissues or systems.

Many writers have characterized the disease as an *asthenic* diathesis; but this will not explain many of its phenomena, such as its frequent development under the influence of chronic phlegmasia and the abuse of alcoholic drinks—the aggravation of the symptoms by stimulating, heating remedies—their constant alleviation from the use of cooling remedies, baths, vegetable diet, &c.,—and the frequent complication of scorbutus, with well-marked inflammatory diseases of the lungs, heart, liver, &c.; both diseases following the same course, and yielding to the same antiphlogistic treatment. Has scorbutus then a decidedly *inflammatory* basis? The following reasons tend to an affirmative reply. 1. Its property of changing its seat, a property belonging to phlegmasiæ. 2. The swelling of the calves and joints has all the characteristics of inflammation. 3. Pathological anatomy reveals abundant signs of the various stages of inflammatory action. 4. The eminent utility of blood-letting, as testified by almost all the principal writers upon the disease. Most of the modern Italian physicians, who have had much opportunity of observing the disease, agree in this view of its phlogistic nature; but others of them deny inflammation is other than an accidental complication, the disease itself being producible under a variety of injurious influences.

"These different opinions of great authorities sufficiently prove the difficulty of arriving at a decision upon this somewhat obscure subject. Nevertheless, in consequence of the careful investigation I have made into the cases which have come under my care, I believe the following corollaries may be drawn 1. That this disease in no respect belongs to the asthenic diathesis. 2. That apyretic scorbutus depends upon a specific diathesis, which from accidental causes may put on either a sthenic or hyposthenic form; and in consequence I consider it neither a phlebitis or an arteritis, the blood-vessels always being only secondarily affected. From this a sufficiently easy explanation of the scorbutic pathogenesis seems to result, the affection being, as Paccinotti denominates it, a *cacotrophy*, or process of malassimilation. Mental passions, cold-damp, insufficient or bad aliment, and indeed every thing which can interfere with the digestive functions, necessarily lead to a vitiated hæmatosis, whence arise the debility, the yellow colour of the face, the dejection of spirits, &c., which are the earliest and most constant signs of the disease. Sanguifaction vitiated (for the blood is black and fluid, and, notwithstanding the opinion of Polli not coagulable or very slightly so), the heart and its vessels are stimulated by a fluid foreign to their ordinary mode of sensation, and the various secretory organs become slowly irritated. In a word, every system of the animal economy undergoes change, and becomes perverted in its action. The secretions are incomplete, and gradually every part of the machine tends to dissolution."

Acute Scorbutus or Scorbutic Synocha.—Isolated examples of this disease are of rare occurrence, but it is not unfrequently met with where scorbutic

influences prevail. It may be briefly defined as *inflammatory fever, accompanied by symptoms of scorbutus.* Frequently a slight disease, at other times it is a very fatal one, the swollen gums then passing into a state of gangrene with great rapidity. The diagnosis of this disease at first is not always easy. To the early symptoms of scorbutus, such as the greenish-yellow cast of countenance, swollen state of the gums, various shaped spots, distributed over the surface, we may add great heat of skin and a hard and vibrating pulse, with cephalalgia. The addition of other symptoms, such as those of gastritis, pleuritis, &c., would rather indicate an inflammatory complication supervening upon ordinary scorbutus. Whatever doubts may be entertained respecting the other form, of the sthenic nature of this there can be none. General bleeding and the application of leeches to the gums, the copious use of acidulated drinks, laxatives, and sinapisms to the soles of the feet, and abstinence, with ice, or muriatic acid gargles for the mouth, constitute efficient remedies.

As this form of scorbutus has been but little noticed by authors, we will here give a short abstract of one of the three cases furnished by Dr. Novellis.

Case.—"Antonio B. æt. 46, of sanguine temperament and robust constitution, and liable to inflammatory diseases, had been six months in prison, when he applied to be admitted to the hospital. He had lost his fresh colour, and from being merry and gay had become gloomy. He presented all the signs of fever, as a hard, vibratory pulse, dry rough skin, pains in the head and legs, intense thirst, heated mouth and fetid breath. He was bled at once, and again in the evening, cooling drinks and abstinence being also ordered. The disease, from the pains of the extremities, was termed a *rheumatic* synocha. Next day, no improvement being observed, the blood presenting a lardaceous crust, and his gums having become painful and bleeding at the slightest touch, two more bleedings were practised. The third day all symptoms were much alleviated, save the acute pains of the lower extremities. On examining these, to my great surprise, I found them covered with violaceous petechial spots, and, joining this symptom with the swollen state of the gums, which emitted an insupportable fætor, I could not doubt the case was an example of *scorbutic synocha.* The phlogistic symptoms re-appearing, the bleedings were repeated (in all amounting to seven), and, by the seventh day, the patient might be said to be cured, but that the petechiæ and swollen gums persisted. Purgatives and fomentations of the legs were required for several days before the skin resumed its natural moist condition; and it was not until the fourth week the patient was discharged entirely well. The following year he was admitted with exactly the same symptoms, and discharged cured in two weeks."

Complications of Scorbutus.—Other affections may become associated with scorbutus in two modes. Either the scorbutus supervenes upon any disease, when the complication may be called *primary*, or the disease may associate itself with a pre-existing scorbutus, giving rise to a *secondary* complication. This distinction is not merely scholastic, but practical. In the first case, the disease is always advanced and generally chronic, and, if it is serious, the scorbutus rapidly runs through its stages, and frequently proves fatal. In the second case, the disease may associate itself with scorbutus at any period of its progress, and it is generally an angina, bronchitis, gastro-enteritis, carditis, or pneumonia. If the patient has not become exhausted by the scorbutus, and this has not advanced beyond its second

stage, it and the new disease may pursue the same course, and yield to the same antiphlogistic remedies. Nevertheless the complicating disease sometimes disappears, while the scorbutus goes through its usual course. The most fearful complication, according to Lind and Milman, is typhus, whether this be primary or secondary, sufficing, as it does, to rapidly diminish the numbers of an army, or entirely destroy the crew of a vessel. Sometimes a complication acts as a *crisis*. "I have frequently observed that, after the supervention of scorbutus, towards the end of an inflammatory disease, especially bronchitis, every appearance of this has rapidly disappeared; and, in like manner, I have seen the occurrence of an intermittent banish in two days every sign of scorbutus."

Prognosis.—In acute scorbutus the prognosis may usually be easily delivered; but in the apyretic form it is often very difficult, as the disease may remain stationary in its second period for months, and then proceed precipitately to an unfavourable issue. When the complication is primary, little hope of a favourable termination can be held out; and when it is secondary, the prognosis depends, during the first two stages, upon the intensity of the new malady; but when this only appears in the third or fourth stage it is very unfavourable. The complication with hepatic disease is generally fatal.

Contagion of Scorbutus.—This question has been a subject of great dispute among the most estimable writers; but Dr. Novellis agrees with those who utterly deny any contagious property, and, after noticing the ordinary arguments favourable to his views, he observes that *acute scorbutus* furnishes an additional one, inasmuch as, unlike variola, scarlatina, and other contagious diseases, it may be communicated to the same individual repeatedly. Since his appointment to the prison-hospital at Alexandria, he has bestowed much attention upon this question of contagion, and has never met with an instance of its propagation among the servants of the establishment, nor among others of the prisoners who constantly lived and partook of the rations of the affected. So, too, he has found in the case of the convicts, who are chained two and two, and constantly live and sleep together, that although in one of these the disease may have become developed a week prior to their separation, the other has not contracted it. The predecessor of Dr. Novellis, a believer in contagion, had taken great precaution for the separation of the scorbutic patients; but he found that the discontinuance of these, and the allowing the other patients access to the secluded portion of the building, in nowise increased the number of those attacked by it. Assured of the non-communicability of the disease, he yet determined to try the effects of inoculation, and conveyed into the gums, arms, and legs of two volunteer convicts, some of the blood and pus from a well-marked scorbutic case. They were retained among the scorbutic patients for several weeks, being allowed a substantial diet, but no effects whatever resulted.

Prophylaxis.—Experience has now amply shown that this is to be sought for, not as once thought, in drugs, but in a rational hygiene, modified according to the varying circumstances of climate, locality, &c. By the

aid of this, the improvement accomplished in the condition of prisons, and ships at sea has been immense. A judicious alimentation seems to be a principal agent, the portions of flesh and vegetable being duly proportioned. Dr. Baly's observations upon the anti-scorbutic proportions of the potato are in this point of view quoted with approbation by the author.

Curative Measures.—Innumerable as have been the remedies vaunted for the cure of scorbutus, not only does not any one of them deserve the title of a specific, but we cannot even say that we have any certain guide for its rational treatment. Thus it is a well-known fact, that vegetable substances are of great advantage in scorbutus, and yet the disease has been frequently known to rage on board vessels well supplied with them, and Wilson furnishes us with an account of an eruption of the disease in places where vegetables were the sole diet, and which was cured by having recourse to flesh diet.

Our first effort must be directed to the removal of the patient, where this is possible, from the locality or climate in which he is in, after which our remedial agents may act efficiently. Every effort must be made to encourage him, and remove the melancholy by which he is oppressed. The effects of hope are sometimes wonderful. Thus Frank relates that, after it had been announced to the French army that its return to France was decided upon, of 220 patients suffering from scorbutus, the greater part very severely, only 18 died, the remainder becoming rapidly well.

When a prisoner presents signs of scorbutus, Dr. Novellis follows the practice of Van Swieten, of administering a laxative. He is cautious in the use of emetics, especially of antimony, inasmuch as gastritis easily complicates scorbutus. All complications of an active kind must be promptly dealt with. Thus, gastro-enteric irritation is to be relieved by applying leeches to the hæmorrhoidal vessels; and, if there is cough, palpitation, dyspnœa, or those wandering pains termed by Sydenham scorbutic rheumatism, or darting pains in the chest, with a hard full pulse, venesection is required, and although the blood drawn is black and of little density, a persistence of the symptoms will call for a further loss. Much discrepancy of opinion prevails as to the employment of venesection in *simple apyretic scorbutus*. Many declare it is indispensable, others prohibit it, and others again employ it sparingly. Dr. Cima declared, at the Milan Congress, that when he used to bleed he lost from 5 to 10 per cent. of his patients, but that since he had ceased doing so, he scarcely lost 1 per cent. In an epidemic which occurred in Montevideo in 1844, two Italian physicians, MM. Odicini and Antonini, regarding the disease as a phlebitis, resorted to depletion and other lowering remedies, and declared that the mortality in their hospital was much less than that among the patients of the French, English, and Spanish physicians, who were treated by stimulants and tonics. Dr. Novellis' own experience leads him to believe this remedy is of slight utility, unless manifest signs of plethora be present.

Baths are of great utility, diaphoresis being frequently induced after the second or third, and followed soon after by disappearance of the maculæ and other bad symptoms. In France they are frequeutly medicated, but Dr. N. prefers the simple water. The partaking of *acid drinks* of various kinds is of great advantage, although not so exclusively a curative measure

as sometimes supposed. The use of *vegetables* has been prescribed by almost all writers; but some of these of a stimulant nature, such as celery, onion, garlic, &c., when used immoderately, Dr. N. has seen several times give rise to the disease. He imitated the series of comparative experiments performed at Haslar by Lind, and came to the same conclusion with that writer, that preference was not to be specially given to any one vegetable, since plants of quite opposite qualities, as the *nasturtium aquaticum* and the *lactuca sativa*, produced just the same effects. He agrees with those writers, also, who consider plants most efficacious when eaten not only fresh, but uncooked. Upon the strength of the statements of Blane, Smith, and Baly, he also tried the potato, but found it of little or no use, and considers that it, or any farinaceous food, is improper for the treatment of the disease, however suitable an adjunct to meat diet as a prophylactic.

Dr. Novellis speaks approvingly of the *nitrate of potass*, given with acidulated drinks, so warmly praised by some authors. The preparations of *iron*, also recommended by some moderns, have been freely tried by him, but without any successful result, and often with the effect of exciting gastric disturbance and diarrhœa. He relates a series of experiments he performed on a number of prisoners, first with preparations of iron, and then with simple doses of nitrate of potash. Under the use of the first, the patients continued stationary or retrograde for months together: while by the employment of the nitrate, with the aid of fresh vegetable and good meat diet, and the moderate use of wine, they rapidly got well. We must here protest against the cool manner in which our author submits those assigned to his care to a treatment which prior experience had already convinced him was a bad one, merely, as it would seem, to be able to state in figures the exact proportion in which it is so. He had already determined that iron was useless or hurtful, when he instituted a new set of experiments to show how soon nitre would cure those who had been for *several months* submitted to such injurious influences. In the name of the dignity and usefulness of our art we protest against this wanton procedure.

From the time of Huxham, who relates the case of a woman who induced scorbutus by taking *marine salt* for the cure of scrofula, this substance has been reputed as a frequent cause of the disease. Russell, Lind, Milman, and others, have protested against the doctrine, but it has become somewhat rooted in popular opinion. Our author found it very prevalent, so that the food employed in ordinary was quite insipid. He increased the quantity of salt, and soon found the number of scorbutic patients diminish. So, also, he substituted for the gargles in use one composed of one ounce of salt to four ounces of water with the effect of rapidly improving the condition of the diseased gums; the ulcerations, flux of blood, and sphacelus, once so common, now no longer appearing. The salt, too, proved a refreshing cooling application to the heated mouth.

Differential Diagnosis.—The author examines this in relation to the various local and general diseases scorbutus is liable to become confounded with, such as Gingivitis, Hæmorrhage from the Gums, Aphthæ, Stomatitis, Syphilis, Purpura, Petechial, Typhus, &c., &c. We need only notice the two last.

The causes capable of producing *purpura hæmorrhagica* much resemble those which produce scorbutus; and the symptoms of the two diseases are very similar. Both, too, may continue in a chronic condition for months. It is of importance to turn attention to the concomitant symptom of purpura, the hæmorrhage, which is not found in the early stages of scorbutus. The scorbutic synocha is distinguished from the purpura by a formication of the surface, the livid colour of the gums and the fetid breath. Although there is much resemblance between the symptoms of *petechial typhus* and scorbutus, the skilful practitioner will easily distinguish them. The third stage of the latter has been called by some writers the *typhoid stage*, on account of the prostration into which the patient falls; but this may be easily distinguished from a true typhus, since, besides the state of the gums, the fætor of the breath, and the pains in the extremities, the scorbutic patient never loses his appetite, desiring even the most extraordinary articles of diet—while in typhus the most complete anorexia prevails.

We consider that Dr. Novellis has produced a very creditable essay upon an interesting subject, and we only wish that he and his Italian brethren would furnish us with more frequent opportunities of making their acquirements better known to the practitioners of this country.

Observations on the History and Treatment of Dysentery and its Combinations; with an Examination of their Claims to a Contagious Character, and an Inquiry into the Source of Contagion in its analogous Diseases, Angina, Erysipelas, Hospital Gangrene, and Puerperal Fever. By *William Harty*, M.D., &c. 2nd Edition, 8vo. pp. 303. Hodges and Smith, Dublin, 1847.

The first edition of this truly valuable work was published more than forty years ago, and received a most favourable character from the leading medical journals of that time: it was mentioned also with commendation by Sprengel in his Critical Review of Medicine To most of the readers of the present day it has all the novelty of a new book; few are acquainted with it, and fewer yet have learned to appreciate the value of the doctrines it propounds. To us it has a peculiar interest; and more especially at the present time, when we have been endeavouring to settle, in some degree, the opinions of our medical brethren on the important subject of Infection, as an attribute of certain diseases, and a cause of their propagation and diffusion. It is, indeed, most gratifying to find that so enlightened an observer as the author of the work now under review had long ago adopted, and advocated with no common ability, the same view of the question as we have sought to establish: the sanction of such an authority goes far to confirm us in the accuracy of our own opinions. In alluding to the subject of Contingent Infection, in the last number of this Journal (p. 459,) we gave expression to the following remark, in reference to the very disease upon whose true history Dr. Harty has happily

thrown so much light:—"If the broad and general question, Is Erysipelas or is Dysentery infectious? were put to a physician, he might, with perfect truth and propriety, answer it in the affirmative; and yet all that he meant by such a response might be the simple declaration of his opinion that these diseases are occasionally or conditionally communicable from one person to another, although such an occurrence does not happen once in a thousand times, and only under very peculiar circumstances." In the present article we shall have an opportunity of explaining what is the nature of those circumstances, to which allusion was then made.

In commencing our observations, we may remark that there is perhaps no malady on which there has been greater discordancy and contrariety of opinion as to many of its most important phenomena and attributes than Dysentery; and, judging from the observations of the very latest writers upon this disease, medical men seem to be nearly as far remote from settled, or well-grounded opinions on some points, as ever. Dr. Copland has said—with too much truth we fear—that, "our knowledge of the disease is but little in advance of what it was two centuries ago, and that even the most recent writers are distinguished rather by confined and exclusive ideas as to its nature and treatment than by comprehensive views of its forms and manifestations in connexion with the various combinations of causes producing it, and the diversified circumstances in which it prevails." And then he adds the philosophic remark:—"Exclusive notions of a disease are the result of a knowledge merely of what has occurred within the sphere of the author's observation; whilst more extended ideas are acquired from what he has remarked in various climates, on different occasions, and at distant periods, and from an acquaintance with what has been observed by others, believing, truly, that nothing is constant but change; that what has occurred, or prevailed formerly, will recur again; and that one form is as likely as another to appear in future, whenever the concurrence of causes, of which it is a necessary or contingent result, shall take place." . . . . . "That writer but imperfectly performs his duty, who, in giving a history of a most dangerous malady, confines himself to the particular form it has assumed during a few seasons, within the single locality of which he is the centre, and argues that it is always as he observed it: thereby affirming as true of the genus, what may be hardly true of the species."* So much for one high authority; let us see what says another.

In the last edition of the classical work of Dr. Johnson and Mr. Martin on the "Influence of Tropical Climates," &c., we read:—"There is hardly a disease in the whole range of nosology, regarding which so great a discrepancy, among authors and practitioners, has existed as in dysentery; and this must have originated, I conceive, (says Dr. J.) in consequence of mistaking prominent *effects* for proximate *causes;* and, as the means of cure directed against the former have often removed the latter, each individual believed that he alone had found out the true cause and cure of the disease."

---

* *Dictionary of Practical Medicine.* Vol. I., p. 701.

But it is not so much in reference to the pathology and treatment of dysentery, as to its occasional attribute of infectiousness, that we have now to invite the attention of our readers. As upon this point, too, the utmost confusion still prevails, we sincerely trust that the present analysis of the instructive volume before us will be found to disentangle many of its perplexities.

Dr. Johnson has, in the work just cited, given the weight of his authority in too unqualified a manner to the negative side of the question, resting his opinion solely and exclusively on the results of his own experience in a tropical country; where the disease, it is admitted, seldom exhibits an infectious character.* The most recent writer on Dysentery, Dr. Parkes (whose work† we noticed in our Number for October last), is more guarded in his opinion, and thereby more correct.

"Contagion," says he, "undoubtedly does not act in common dysentery; but it is certain, if the statements of many esteemed authors are to be admitted as evidence, that, at times, in slave ships, and after some campaigns, or in besieged cities, asthenic dysentery has become contagious, being then complicated with a low fever. This low or typhoid fever has been supposed by some to be the cause of the contagion; but to this is replied, why should the dysentery be always the resulting disease, and not the fever? But it is a question if contagious dysentery is ever seen separated from the accompanying fever; are not the two diseases always propagated together? Are there instances known in which pure uncomplicated dysentery, sthenic or asthenic, as the case may be, has been derived from a case compounded of dysentery and typhus?" P. 132.

Dr. Parkes is quite right in his conjecture that it is the association or conjunction of Typhus fever with Dysentery that renders the latter disease liable to become infectious. This point in its history Dr. Harty has elucidated far more ably and successfully than any writer, either before or since the publication of the first edition of his work. As we have said, his views have not hitherto been appreciated by the profession as they deserve. Had they been better known and understood, there would doubtless have been less wavering and discrepancy in the statements of many writers—and those, too, men of experience—during the course of the present century.

The following three positions contain the pith and marrow of our author's opinions on the general history of the disease we are now considering:—

1st.—*The genuine and simple dysentery is unattended by idiopathic fever, and is never of itself contagious (infectious).*

2nd.—*That every form of the disease, when epidemic, is a combination of the simple dysentery either with intermittent, remittent, or continued fever; and,*

---

* It would seem, however, from the following extract from the Medico-Chirurgical Review for October 1838, quoted by Dr. Harty, that Dr. Johnson did admit the occasional or contingent infectiousness of Dysentery. "We doubt much whether the disease is ever infectious *per se*, but only when it is an attendant on a low or typhoid fever, when the latter may prove infectious, and carry its intestinal character from person to person."

† *Remarks on the Dysentery and Hepatitis of India.* 1846.

3rd.—*That the combination with continued fever alone is contagious (infectious).*

Having illustrated in, as appears to us, the most satisfactory manner, the truth of these conclusions, Dr. H. then proceeds to examine the subject of the Treatment of the different forms of Dysentery, pointing out as he goes along how necessary it is to have clear and accurate notions on the above pathological positions, if we hope to attain to anything like precision or accuracy in our therapeutic principles. Thus, the practical importance of the preliminary inquiry is rendered obvious, while, at the same time, fresh proofs of the accuracy of the conclusions arrived at are obtained from an examination of the remedies that have been found to be most useful at different times.

The first chapter is devoted to the consideration of *Simple Dysentery.* By this term our author wishes to be understood that species of the disease in which the intestinal symptoms—viz. severe tormina of the bowels, frequent dejections of a mucous and bloody character, accompanied with a most distressing tenesmus—are not necessarily associated with any form of *idiopathic* fever. That fever is not an essential accompaniment of dysentery, is abundantly obvious from the observations of some of the most accurate describers of the disease. Sydenham states that, in the majority of cases, which came under his notice, the gripes attack first, and the purgings succeed, without being preceded by any fever. Akenside* asserts that dysentery is very rarely attended by fever, not once in ten cases of the disease. Stoll† too, when speaking of the rheumatic-bilious form of the disease, says, "febre quidem evidenti carebat, non omni tamen motu febriculoso." Roederer‡ says, "fever was either altogether absent, or it set in only after the disease had existed for some time: few patients sickened from the first with febrile symptoms." M. Vignes|| also, when describing the dysentery prevalent in the French army encamped around Vienna in 1809, states that, in the Spring, "in several cases we noticed the prevalence of slight fever, but in the majority there was none at all. In the latter the symptoms were always less severe, than in the former." As the season advanced and numbers increased, "the dysentery began to be complicated with typhus, adynamia, ataxia, or nervous fever, which prevailed epidemically throughout the army; it spread also amongst the inhabitants of the country. Among the latter, however—and this is a fact especially well worthy of notice—the disease appeared only under the characters of the fever, the dysenteric symptoms not occurring in them." From this circumstance, M. Vignes infers that the intestinal affection was generally owing to the exposure to inclement and variable weather, excessive fatigue, and the insufficient or irregular supply of food; from all which evils, the soldiers in the French army suffered more severely than the people of the country. Frank§ also, who had very ample experience

* *De Dysenteriâ Commentarius.* Lond. 1764.
† *Ratio Medendi.* Vien. 1788
‡ *De Morbo Mucoso.* Gotting. 1783.
|| *Traité Complet de la Dysenterie.* Paris, 1825.
§ *de Curandis Hominum Morbis.* Lib. v. *de Profluviis.* Vien. 1807.

in the disease, states that "in numerous cases there is no shivering nor feverish heat at its commencement, no loss of appetite is experienced, and there is scarcely any perceptible acceleration of the pulse." Dr. Parkes, too, says:—"In some particular forms there may be attendant feverishness; but in the majority of cases there is no pyrexia, in the common acceptation of the term. Often there are no rigors or pains in the limbs, or any affection of the general health."

The passages we have now quoted—and their number might easily be multiplied threefold—will sufficiently serve to show that fever is not a necessary or essential accompaniment of an attack of dysentery. That a greater or less degree of febrile excitement very generally supervenes, if the symptoms be not immediately arrested, will be denied by no one; but this is very different from fever being supposed to constitute one of the primary characters of the disease. Hence its definition, given by Cullen, is clearly erroneous. It stands thus:—"*Pyrexia contagiosa; dejectiones frequentes, mucosæ, vel sanguinolentæ, retentis plerumque fœcibus alvinis; tormina, tenesmus.*" We shall not now allude to the (alleged) fever being here declared to be always infectious; as this subject will engage our attention in the sequel. Akenside's descriptive definition is altogether more accurate:—"*Qui graviora patitur ventris tormina, simul cum frequente desidendi cupiditate, et cum dejectionibus vel sanguineis vel mucosis* (*vel utrisque*), *eum hominem Dysenteria laborare omnes consentiunt medici.*"

Simple dysentery is very generally the result of exposure to wet and cold in the months of Summer or Autumn, more especially when this most prolific cause of disease is aided by the use of unwholesome or insufficient food, acid unripe fruits, pernicious drinks, and, in short, of whatever has a tendency to induce irritation of the mucous surface of the bowels. Almost all sporadic cases of the malady belong to the *simple* form now described; and instances of it are also far from uncommon in every epidemic, whatever be the prevailing type of the fever by which the epidemic may be characterized. Before proceeding to notice those forms of the disease which arise from a combination of the intestinal affection with fever of different types, Dr. Harty devotes a chapter to illustrate the *Affinity between Dysentery and Rheumatism.*

"The opinion that there exists an intimate connexion between these diseases first suggested by Alexander of Tralles (who called dysentery a rheumatism of the intestines), has in modern times been adopted by Akenside, Richter, Stoll, and others. Independently of the authority of these writers in favour of this doctrine, we find in other authors a few scattered facts, which must carry the greater weight, as they are recorded by individuals unbiassed by any theory on the subject." P. 22.

Akenside states that he often observed rheumatism to supervene upon the decline of dysentery and *vice versâ*, that occasionally the two diseases exist together, and that both disappear under the same line of treatment. Every physician must have remarked that the stools in the former disease often exhibit the dysenteric character, consisting almost entirely of mucus, and attended with griping pains and distressing tenesmus.*

* Daily experience shows that not only rheumatic, but gouty disease also, is

Dr. A. mentions several cases of the apparent conversion of the one disease into the other, or at least of their alternating with each other. In one of these cases, the patient had three returns of rheumatism, and as many of dysentery, before he was restored to health. After adducing a variety of details, he concludes thus :—" I have so often had occasion to observe the resemblance between the two diseases, that now I regard Dysentery as a Rheumatism of the bowels, and fairly believe that the cause of both is alike." Stoll held the same opinion and uses nearly the same words to express it. He calls rheumatism and dysentery *παθηματα αδελφεα*, and assigns these reasons for this belief:—" 1. Because he often observed dysentery to supervene upon the sudden cessation of rheumatism ; 2. Because sometimes the one and sometimes the other disease occurred in the same individual ; and 3. Because dysentery often suddenly ceased as soon as any of the joints became swollen and painful." Richter* too declares himself satisfied of the truth of these positions, and has related several instances of the mutual conversion of the two diseases. Zimmermann† also may be quoted to the same effect ; he mentions several cases where rheumatic symptoms immediately supervened upon a sudden arrest of dysenteric symptoms from the use of opiates and other astringents. Tissot‡ and Sir George Baker‖ make a similar remark. Hoffman had at an earlier period observed that, when dysentery is quickly arrested by the use of astringents and opiates, among other evil consequences, severe articular pains are apt to be induced. M. Vignes mentions that, among the numerous cases occurring in the French army around Vienna, " the alvine dejections and other dominant symptoms of the dysentery sometimes suddenly ceased upon the supervention of rheumatic pains in the knees, elbows, wrists and shoulders." Frank says nearly the same thing : " in some patients the pain of the joints seems to have freed the bowels from suffering."

It will have been perceived, from the preceding statements, that the supervention of rheumatic disease in cases of dysentery has generally taken place, when the intestinal discharges have been suddenly or prematurely checked. We may hence derive a most useful therapeutic precept, viz. not to have recourse to astringent medicines at once and without any preliminary treatment.

It may be here worthy of notice, that Frank has drawn an ingenious analogy between Dysentery and Cynanche. Thus : " It is the same sort

---

occasionally apt to induce intestinal fluxes as well as catarrhal affections. The cause would seem to be that there is present, in these morbid states, an acrimonious peccant material in the blood, which requires to be eliminated and discharged from the system ; and if this be not readily carried off by the appointed emunctories—viz. the kidneys and skin—it is liable to fall upon the mucous lining of the alimentary canal or of the air passages. Hence it is that the above diseases are not unfrequently concomitants of Oliguria or deficiency of urine, from whatever cause this originates.—*Rev.*

* Observations, Medical and Surgical. 1794.

† *On Dysentery.* Translated by Dr. Hopson from the German. Lond. 1771.

‡ *Avis au Peuple.* Translated by Kirkpatrick. 1771.

‖ *De Catarrho et de Dysenteriâ Londinensi.* 1764.

of disease which attacks the throat or upper end ofthe alimentary canal, and the anus or its lower end: there is the strongest affinity between Dysentery and many species of Cynanche. In severe cases of the latter we have the sense of burning, the redness, swelling, tension, the discharge of a puriform matter that is sometimes tinged with blood, the constant and painful effort of swallowing, not unlike to the tenesmus that occurs in the former." In a subsequent passage, he points out the striking similitude between the malignant forms of the two diseases.*

M. Bruant, in his official report on the dysentery prevalent in the French army in Egypt, takes notice of a curious sympathy between the pains of chronic Dysentery and those of Ohpthalmia: "The affection of the eyes always brought a mared relief, when it occurred in the course of long standing dysentery; the pains of the eyes and those of the abdomen mutually took the place one of the other; the latter, however, usually re-appeared after the cessation of the former."

We now pass on to the consideration of a very important point in the history of the disease.

Chap. III.—*Combinations of Dysentery; and first of the Intermittent and Remittent Forms.*

Morton,† in his excellent account of the epidemic dysentery which proved so destructive in London about the middle of the 17th century, expressly says that the bowel affection was only a symptom of the remittent fever, which was in fact the primary disease. So convinced was he, from the distinct remissions and exacerbations observable every,or every second day, of the truth of this position, that he proposed that the dysentery should be designated by the appellation of spurious or colliquative Συνεχης.‡

Sydenham, it is well-known, has, in more passages than one, spoken of dysentery as a "febris intraversa," or fever turned in upon the bowels. There has been a good deal of difference of opinion as to the exact meaning of this expression. But whatever this may be, it very significantly

---

* Sydenham, in his description of dysenteric fever, has alluded to the alliance which he observed, one year, between the intestinal affection and rheumatism and cynanche conjointly: "The fever of 1671 was constantly attended, towards its decline, with extreme sickness, a vomiting of green bile, and a marked tendency to diarrhœa; while that of the following year was accompanied with pains in the muscular parts of the body, especially in the limbs, resembling those of *rheumatism*, and also with *inflammation of the throat*, but of a milder character than is present in regular quinsy. Yet both these met in the same specific fever, and both required the same method of treatment; they differed only by respect, or in consequence, of the sensible qualities of the air that prevailed at the time when these phenomena arose."—*Observ. Med.* IV. 4, 6.

† De Febribus. Lond. 1693.

‡ Cullen, in his synopsis, includes the Febris Syneches Epidemica ab anno 1658 ad 1664, et postea ab anno 1673 ad 1691, described by Morton, under the head of tertian remittents. The reader should be on his guard not to mistake the fever termed Συνεχη, for that which is so well known by the name of Συνεχος, or common continued fever.

implies the intimate connexion that this truly Hippocratic observer found often to subsist between the two diseases in question, whether this connexion is evidenced by the one becoming convertible into, or taking the place of the other, or in any other manner. And as we know that the fever, to which Sydenham alludes under that term, was of the remittent type, the evidence of our great countryman may be fairly quoted here in proof of the subject matter we have in hand.

It may be worthy of notice, *en passant*, that Sydenham mentions that in the first Autumn, when the dysentery he saw prevailed, many patients had no purging whatever; but that, in respect of the severity of the gripings, the intensity of the fever, the sudden prostration of the strength, and other symptoms, the disease vastly surpassed the dysenteries of subsequent years.

Willis,* too, has left a very accurate description of the dysentery which proved so wide-spread and destructive in London during the years 1670, 1571. The cholera had raged, it may be observed, with great severity in the early part of Autumn of the latter year. About the Equinox there arose an epidemic intermittent or remittent fever, which had in some cases a quotidian, in others a tertian, type. "While this fever prevailed in the villages and towns throughout the country, the bloody flux committed great ravages in London." Willis attributes many of the characteristic features of this flux to the epidemic fever spread over the land, just as Sydenham did to his dysenteric fever; so that these two fevers would appear to be identically the same.

Mosley† testifies to the general truth of Sydenham's opinions, remarking that "as the flux conforms by the number of stools, and by its rapidity, to the degree, so it does to the state of the fever of the season, when it prevails; the stools being more frequent, and all the symptoms becoming more aggravated at those hours when the current fever are in their exacerbation, and the reverse when these fevers are in their remissions." Now we need scarcely say that it is this very character of having distinct remissions and exacerbations every, or every other, day, that has led so many of the most experienced and enlightened observers to regard the dysentery, on various occasions, rather as one of the symptoms of a periodic fever than as a primary and essential disease.

Cleghorn‡ observed so marked a resemblance between dysentery and intermittent fever, that he was led to exhibit bark for the cure of the former. "When the fever and gripes," he says, "were regularly exasperated every day, or every other day, at stated periods, the bark has often effectually put a stop to both, especially if the exacerbation began with chilliness, and terminated in sweats: *at other times it removed the fever, the flux continuing without much alteration.*"

Roederer, too, when speaking of the relation between the two diseases, mentions a striking instance of their combination:—"During this year, many patients were affected with intermittent fever and with dysentery

---

* Opera Medica. Lugdun. 1676.
† On Tropical Diseases. Lond. 1803.
‡ On the Diseases of Minorca. 1762.

at the same time, or with what might be called a genuine dysenteric ague. In a neighbouring town, however, the fever only prevailed epidemically; in another, which was more remote and surrounded by hills, the dysentery alone existed; while in a third and intermediate one there were but very few cases of either disease."*

Clark† also, in his description of the dysentery, or, as he calls it, the dysenteric fever of Bengal, has remarked that the intestinal affection seemed rather to be a symptom of the endemic remittent fever of the country, than an original disease. Hunter,‡ too, says that between dysentery and the remittent fever in Jamaica, there subsists an intimate connexion, the one frequently changing into the other, and both being often complicated with various degrees of violence. Nicholl states that, at Seringapatam, the disease was frequently combined with remittent and intermittent fevers; and Trotter§ has remarked the same thing with respect to the coast of Africa and the West Indies. Even in our own climate, Willan‖ has mentioned that, in the worst cases of the epidemic of 1800, (a year remarkable for cold, wet, and famine,) a considerable degree of fever prevailed from eight to ten days: the pulse 100 in the morning, and 120 in the evening; there was constant flushing in the face and coldness of the extremities; and a periodical aggravation of pain for three or four hours every forenoon was to be observed.

Frank also has alluded to the frequent connexion of the two diseases. "I have seen," says he, "dysentery follow upon periodic fever, and put a stop to it; and no sooner was the dysentery cured, than the intermittent returned as before. Indeed, the former seemed but as a symptom of the latter, and speedily vanished when the producing cause was got rid of. Dr. Wilson, in his recent interesting volume on China, says:—"In truth, the flux ought to be considered a constituent part of the periodic fever, rather than an independent malady in most instances."

The evidence now adduced is surely sufficient to prove, beyond all dispute, the not unfrequent connexion of dysentery with periodic fevers;—

---

* Degner* has remarked that the fever, which accompanied the dysentery which he saw, sometimes exhibited the character of an irregular tertian; and "this," he adds, "appears to me the reason why, in certain dysenteric patients, the symptoms were always worse on every third day." Huxham,† in his account of the epidemic dysentery prevalent in England in 1743, says:—"The disease was generally associated with fever, or perhaps I should rather say, it was a symptom of this fever; from its very commencement, and before the supervention of any tormina in the bowels, there was usually no inconsiderable feverish heat, with quickened pulse, and dryness of the tongue. Most frequently, it attacked with a distinct shivering, and, in many instances, it exhibited the characters of a tertian fever."

* *De Dysenteriâ Bilioso-contagiosâ. Trajecti ad Rhenum.* 1754.
† *De Aere et Morbis Epidemicis.* 1752.

† *On Diseases of Hot Climates.* 3d ed. 1809.
‡ *On the Diseases of the Army in Jamaica.* 1778.
§ *Medicina Nautica.* Lond. 1799—1803.
‖ *Reports on the Diseases in London.* 1801.

a point, be it remembered, of the highest importance, as respects the treatment of the disease. That there are some anomalies in the nature of this connexion must be admitted; they are alluded to in the following statement of our author :—

"Dr. Buel describes a bilious fever and dysentery as prevalent in Sheffield (State of Massachusetts), in 1796, arising from the obvious influence of marsh miasmata, and states that in some 'the dysentery came on while the patient was affected with the fever, in which case the type of the fever soon became obliterated, and the accompanying febrile symptoms were similar to those in original dysentery.' 'Sometimes the fever came on upon the dysentery: the type of the fever was not in this case easily ascertained, until an abatement of the dysentery took place, when, as the dysenteric symptoms subsided, the fever would appear in its proper form. The two disorders appeared to be complicated; that is, they both seemed to exist at the same time, rather than to act in alternation.' 'In this sickness,' he adds, 'there is every reason to ascribe identity of cause to the two disorders: they were circumscribed in a very striking manner by precisely the same limits, and they both began and ceased to prevail at the same time.' Dr. B. was convinced that 'neither of these diseases was propagated by specific contagion.' Dr. Ffirth also, in his Dissertation on Malignant Fever, says that the crew of the ship in which he sailed to Batavia (seventy-six in number), had, with the exception of eight, either marsh fever or dysentery; that the fever appeared to alternate with dysentery; when the weather was bad, the latter prevailed; when good, the former.' From this latter statement it is not to be inferred that the two diseases 'alternated' in the same individual, in the same sense that Dr. Buel speaks of their alternation; in this case, when one disease was prevalent, the other was not. Hargrave, too, in his History of the Walcheren Fever, states that 'in proportion as the autumn grew cool, these fevers (bilious remittent) abated of their ardour, and formed more easily into intermittents, though still irregular and of a bad kind. The dysentery was never general, though not uncommon, and it was observable that those who were seized with it usually escaped the fever, or if any had both, it was alternately; so that when the flux appeared, the fever ceased, and when the first was stopped, the other returned: whence it appeared that, though the two distempers were of a different form, they proceeded from a like cause.'"* P. 54.

As exposure to cold and wet, in certain climates and under certain unsalutary circumstances, is the common cause of simple dysentery, so the complication of the disease with periodic fever may very fairly be presumed to be owing to the operation of these causes in conjunction with the agency of malaria, the prolific source of remittents and intermittents. How often, nay, how generally, are troops exposed to both of these morbific influences at one and the same time! Can we therefore be "surprised that the causes of both diseases should operate on the same individual, producing a dysentery with distinct intermissions or remissions, when we see that such a combination is frequent in those countries where the diseases themselves often exist independently of each other? This subject is explained after a similar manner by Rollo, who remarks, that the dysentery is produced in place of, or at the same time with, these fevers, when the effluvium or

* "The changes from intermittent fever to dysentery were very common (in the Peninsula), and seemed to suspend the intermittent for a time; but, on the removal of the dysenteric affection, the intermittent returned; in some instances, both diseases attacked the same patient at the same time, and then the dysenteric symptoms were aggravated."—*Sir James MacGregor in Med. Chir. Transactions.* Vol. VI.

marsh miasma is joined in its action by the exterior application of cold and moisture, to which those affected with dysentery have been always previously exposed."

Without positively asserting that marsh effluvia alone are not competent to induce dysentery, Dr. Harty very reasonably argues that "the influence of wet and cold in its production is more clearly, satisfactorily, and universally demonstrable than that of marsh effluvia, and that it is consequently more legitimate to conclude that, as dysentery can occur in every possible relation to remittent fever, either as absent or present with it, as preceding it or consequent upon it, or as remaining after the fever has ceased, or *vice versâ*, so it cannot be likely that both are always produced by one and the same cause, more especially as those exposed to marsh effluvia are at the same time generally exposed to wet and cold."

As long as the fever, which happens to be associated with dysentery is of a purely remittent or intermittent type, our author maintains, and with much show of truth, that the disease is not propagable from one individual to another. Most of the authors already quoted, more especially those whose experience has been chiefly in tropical countries, have unhesitatingly adopted this opinion; and some of them, confining their thoughts entirely to what they have witnessed themselves, have gone so far as to deny that dysentery ever exhibits an infectious quality. That facts, however, do not warrant so sweeping and indiscriminate an assertion must be admitted by every one, we should think, who candidly examines the evidence adduced by our author in the fourth, and second section of the fifth, chapter of his work. The former is devoted to the consideration of the *Combination of Dysentery with Typhus*. We shall now give a summary of its leading contents.

Etmuller* alludes in a very expressive manner to the distinction of dysentery into two forms or species, the mild and the malignant. "The former," says he, "is usually unaccompanied with fever, is not infectious, and occurs only sporadically; whereas the latter is very generally associated with a malignant, and sometimes a petechial and pestilential, fever, prevailing as an epidemic over a larger district, and multiplying itself by a virulent infection."† He adds: "The tongue is whitish, and covered

---

* *Opera Omnia.* Gen. 1697.

† The reader might infer, from such a passage as the above, that whenever dysentery prevails in an *epidemic* form, it is believed to be infectious. But this is not necessarily so; the disease when associated with remittent fever may prevail epidemically, but is not infectious, as long as the fever retains the genuine periodic type. We are the more anxious that the reader attend to this point, as much of the controversy and dispute respecting the infectiousness of epidemic dysentery has arisen from a neglect of it. Such a passage as the following, taken from the extremely valuable Report of the French Academy on the Plague, might easily lead into the error in question. Alluding to the very important difference between Sporadic and Epidemic Plague, the one being admitted by almost every medical man in Egypt to be not communicable from one person to another, whereas there is difference of opinion as respects the other, the intelligent Reporter, M. Prus, goes on to say:—"There is nothing in this opinion, so generally recognized, not only at Alexandria and Cairo, but also at Smyrna and Constantinople, to surprise the medical man, who knows that *sporadic* dysentery is not communicable, while *epidemic* dysentery is often so in a high degree."

with a thick slime; or, if the fever be high, it is parched and black; there is always an extreme prostration of strength. The disease spreads like the plague, is infectious, and is then propagated under the form of a malignant fever." Zimmermann states that "the circumstance, which impresses on a dysenteric complaint the peculiar mark of malignity," is this, that, "with the causes common to the disorder at all times, others are joined which corrupt the humours very quickly. The malignant dysentery, therefore, is that in which, either from external causes, or from a putrid fomes within the body, a malignant fever supervenes; the pathognomonic signs of this species being formed by the symptoms of a malignant fever mixed with the usual symptoms of a dysentery." "It also," says he in a subsequent passage, "very often terminates in, or is a concomitant symptom of, putrid and malignant fevers; but, when a malignant fever supervenes on a dysentery before subsisting in the body, this is quite a different case, and constitutes a peculiar species of dysentery.

Dr. Rogers, in his Essay on the Epidemic Diseases of Cork (1733), furnishes satisfactory evidence of the intimate connexion that subsisted between the endemic typhus of that city and a *malignant* form of dysentery that prevailed there for several years. He remarks that the two diseases kept pace with each other, that they seemed to partake of the same common cause, and were best treated by much the same line of treatment; *viz.* the use of the most generous, warm, and active cordials; all antiphlogistic and lowering remedies were found to be utterly inadmissible.

The testimony of Sir John Pringle, also, is very strong upon the question under consideration. After stating that dysentery always becomes much aggravated towards Autumn, and is then apt to "become epidemic and infectious" (from which expression it may be fairly inferred that, previously, it was sporadic and non-infectious,) he gives the following instance of the pernicious effects of crowding dysenteric patients together:

"The village of Feckenheim was taken for an hospital, into which about 1500 sick were sent from the camp, and of that number the greatest part ill of dysentery, by which means the air became so vitiated, that not only the rest of the patients, but the apothecaries, nurses, and other attendants, with most of the inhabitants of the village, were also infected. To this acceded a still more formidable disease, the hospital or jail fever, an inseparable attendant of foul air from crowds and animal corruption. These two combined, caused a great mortality; while, on the other hand, such as were seized with the dysentery, *and not removed from the camp*, though wanting many conveniences, kept free from this malignant fever, and commonly did well." "At Feckenheim few escaped; for how mild or bad soever the flux was, this fever (the malignant) almost surely supervened: the petechial spots, blotches, parotids, frequent mortifications, contagiousness, and the great mortality, set forth its pestilential nature."—P. 86.

Other passages might be quoted from Pringle's work in confirmation of the views here stated. For example, in one place he says:

"The most fatal sort of fever, which so often attends the dysentery of the army, though not *essential* to it, is the *hospital* or *jail distemper*, which, at all times infects foul and crowded wards, but never so much as when they contain men labouring under a putrid disease." In another place he takes notice of Degner's epidemic, and remarks:—"As to the violence of the symptoms mentioned by that author, I own it exceeds anything I have observed upon the first seizure, but when a number of men, *even with the most favourable cases*, have

been crowded into the hospitals of the army, the dysentery has *then* appeared with all the virulence that it did at Nimeguen. Pringle further adds, that 'when mortification takes place, the distemper is most contagious, whether in producing a simple dysentery, or one combined with *the common hospital fever;*' and, as he says elsewhere, 'It is to be apprehended that when a single person is taken ill of any putrid disease (such as dysentery), &c., and lies in a small and close apartment, he may fall into this malignant fever.'" P. 87.

Equally striking is the evidence furnished by Grimm, in the account which he has given of three "epidemic and eventually infectious" (epidemici et *tandem* contagiosi) diseases which prevailed for upwards of three years, about the middle of last century, in one district of the Duchy of Saxe Weimar. The *first* of these diseases was a malignant Fever, which began in December, 1758, and raged throughout the Spring of 1759; it then abated somewhat of its violence; but still, during the remainder of the year, all the prevailing diseases in the place exhibited a marked tendency to assuming a type of malignancy. The fever revived in the Winter and Spring months of 1760; in the August following, it was succeeded by another and more destructive pestilence, the dysentery. This continued its ravages to the end of November, when it again passed into the malignant fever; and so striking was the connexion between the two, that, as Grimm has observed, "the same fever might well be called exanthematic at one time, and dysenteric at another, according to the season of the year in which it prevailed." "I have seen," adds he, "petechial and miliary eruptions appear on dysenteric patients; while, on the other hand, the cases of fever were often accompanied with a most offensive diarrhœa like a dysentery." It only required a change in the weather or season to cause a transmutation of the one disease into the other. In one passage, Grimm speaks of the dysentery being "associated with its own malignant fever;" and, in another, he seems to regard the former as merely a symptom of the latter; for he says that "the primary disease is the fever, arising from a peculiar vitiation of the humours, and affecting the bowels more than the other viscera of the abdomen." No candid person, one should think, after reading Grimm's description of the epidemic which he saw, can hesitate for a moment in regarding it is a compound of dysentery and malignant typhus. If such, then, were the case, can we be surprised at its exhibiting a virulent infectiousness? a property or element, which, to use the author's significant expression, was "both an effect and a cause of the disease."—This remark, it will be observed, is pregnant with much meaning.

The epidemic described by Roederer, affords equally conclusive evidence on the point we are considering. In July, of 1760, there was much intermittent fever (often of a malignant type) in the town (Goettingen) and its neighborhood; this was followed in the next month by dysentery, which prevailed to about the beginning of Winter, when it gradually lapsed into the epidemic described by him under the appellation of *morbus mucosus;*—from the immense quantity of mucus that was found on dissection within the bowels, more especially the smaller ones. This acute mucous fever sometimes assumed a tertian type; but not unfrequently, more especially in the camp hospitals, it put on the characters of malignant bilious or putrid typhus. In March, 1761, it became a true petechial

fever, accompanied with either furious delirium or coma, and constituting what Roederer calls his "febris mucosa acuta maligna." He points out the intimate connexion between this disease and the dysentery which immediately preceded it; indeed, he does not hesitate to regard both as springing from the intermittent fevers which had gone before; these producing the dysentery, and the dysentery producing the mucous disease. The two last diseases resembled each other not only in their symptoms, but also in respect of many of the cadaveric changes which they left behind; viz. inflammation of the villous coat of the bowels, gangrenous eschars on the inner surface of the large ones, livid spots on the liver, induration of the pancreas, and infarction (congestion) of the lungs being mentioned as appearances common to both. Besides the fever and dysentery that swept away so many of the wretched inhabitants of the town which was then occupied by a hostile garrison, itself invested by an enemy in the field, another, not uncommon, offspring of an atmosphere charged with morbific effluvia from a multitude of patients crowded together and suffering at the same time from insufficient food, viz. Hospital Gangrene, made its appearance among the wounded, and added to the horrors of the place.

Tissot, whom we have already quoted, says, in one passage: "If the corruption of humours, which creates malignant fevers, be united with the causes which produce dysentery, the dysentery resulting therefrom will be malignant;" and Geach, in his observations on the epidemic dysentery at Plymouth, in 1781, tells us that, in some instances, "the patients died in a day or two after the seizure, and had *vibices* large and very black, followed with such a degree of putrefaction and stench, as to deprive almost instantaneously two female attendants of their senses, who very soon after became also dysenteric, ran through the stages of the disorder, till at length one of them died, and the other with difficulty recovered."

On the subject of typhoid dysentery, Frank has well remarked:—"Whoever will place before his eyes a faithful description of typhus, and add thereto the phenomena of dysentery in the way of symptoms, will not require a more lengthened description of this dreadful disease (asthenic malignant dysentery)." He then enumerates some of the symptoms, such as "the sudden and extreme prostration of the vital powers, small pulse, tremor of the limbs, twitching of the tendons, spasms, drowsiness, delirium, with, not unfrequently, petechiæ and livid spots upon the skin. This species of the disease generally prevails in camps, besieged cities, on board ships, &c. It often produces terrible ravages, and is unquestionably infectious."

The details furnished by Cheyne of the dysentery which prevailed in Ireland, concurrently with the epidemic fever of 1817 and 1818, are highly instructive on many points connected with this combination of the disease. Dr. C. considered that both diseases originated from the same morbific causes.

Before concluding this part of his subject, Dr. Harty alludes to the remarkable decrease in the mortality in London from dysentery (under which name used to be included not only the "bloody flux," but also the "griping of the guts," which we read of in the writings of the old phy-

sicians) that has taken place during the last two centuries. We gathe from Dr. Heberden that, in the seventeenth century, the annual numbe of deaths from this disease alone in the metropolis appear never to have been less than 1000, and in some years to have exceeded 4000; whereas, in the middle of the eighteenth, it was little more than 100, and at its close not more than 20. The following passage is replete with so much interest that we are glad of the opportunity of bringing it under the reader's notice.

"These facts relative to the gradual decline of dysentery, or rather of it mortality, in London, are not to be paralleled in the history of any other disease; the plague itself furnishes no parallel case, because that disease either raged violently or disappeared altogether: not so the dysentery, which has declined not so much, perhaps, in the numbers attacked, as in its positive mortality. The cause of so great an alteration in the health of the people of England, Heberden attributes to the improvements that have gradually taken place in London and all the great towns, and in the manner of living throughout the whole kingdom, particularly with respect to cleanliness and ventilation. The great influence of the assigned causes I by no means dispute; on the contrary, I am perfectly satisfied of their efficacy in the production of these happy results: in admitting this, however, I consider them to have been not the *immediate* but merely the *remote* agents in effecting such a change in the mortality of dysentery. The influence of these causes was exerted through the medium of another disease, which, if the preceding views be correct, is the great source of *danger* as well as *contagion* in dysentery; their operation was primarily exerted in mitigating and preventing contagious fever; by that means acting secondarily on dysentery, which, when separated from this its dangerous associate, is no longer an object of terror to the patient or his attendants. This opinion is supported by various facts relative to the plague, to be found in the same author. It would appear that the disease, so called, was seldom absent from London previously to the great fire of 1666, after which event it never more visited the metropolis; it appeared also that dysentery, antecedently to that period, was in general both malignant and destructive, and that shortly after there was a wonderful decrease in its frequency and severity; and though dysentery still prevailed from that time to 1692, a space of twenty-five years, it had much decreased in mortality. Morton states it to have been epidemic from 1666 to 1672, and at first exceedingly fatal (300 or 400 dying every week during its *acme*), though less so towards the end of that period; from Sydenham's description, dysentery appears, except occasionally, to have been devoid of contagion; and Willis's History of the Dysenteria Cruenta of the year 1670, coincides generally with this statement.

"Now I consider it almost demonstrated by Heberden, that the plague of London was nothing more than the malignant contagious fever, exalted by various auxiliary aids to such a pitch of destructive violence as well to merit that name; and that the identity of the two diseases is satisfactorily supported by authority and facts. The singularly rapid decline in the mortality of dysentery when the plague ceased to visit London, or rather when fevers lost much of their malignity, cannot but lead us to conclude that the immediate cause of this change was owing to the diminished influence and dominion of malignant fever in London. We may indeed take it as an admitted fact, that the malignity of dysentery had ceased with the disappearance of the plague. What so common among medical writers, down to the eighteenth century, as accounts of malignant and contagious dysentery? Do not their writings abound with concurrent histories of the plague or of very malignant fevers? Diemerbroeck, for example, mentions, amongst the precursors of the plague which ravaged Nimeguen in 1636: 'Morbi epidemii mali moris, ut erant variolæ, morbilli, *dysenteriæ* valdle

malignæ et contagioeæ, imprimis febres putridæ, malignissimæ et purpuratæ, plurimisque lethales.' "* P. 101.

Having now demonstrated, beyond all possibility of dispute, the existence of a typhoid form or species of dysentery, it only remains to be stated that this, the worst and most destructive variety of the disease, is not, like the remittent or intermittent form, limited to the Summer and Autumnal months, but may continue its ravages throughout the Winter; "being dependent, not so much on the seasons as on the febrile contagion, and continuing so long as the causes of the fever continue to operate." "This fever," continues Dr. Harty, "may be either originally a malignant typhus, or it may have been a remittent of bad character, converted by the peculiar circumstances of the sick into one of a contagious continued type." The reader will excuse us if we call his attention to the position here laid down: viz. that the accompanying fever of epidemic dysentery may be of a periodic type at first, and subsequently—from the operation of various insalubrious circumstances—assume that of genuine typhus. By bearing this fact in mind, he will be able to reconcile much of the conflicting and seemingly contradictory evidence that has been adduced on the question of the infectiousness of particular epidemics.

Surely then, from all the facts and testimonies that have been adduced, there is good reason to believe that the division of dysentery into three species, according as the intestinal affection is unattended with idiopathic fever, or as the accompanying fever is of a periodic or of a continued type, is one that is amply borne out by an impartial examination of the history of the disease, as given by the most experienced writers upon the subject. Hoffman, more than a century ago, had pointed out this three-fold division, as is evident from the following statement at the close of his description of an epidemic dysentery in 1726. "Three classes or grades of the disease might be observed. The *first* was very mild, attended with only flying rigors and heats, and did not always necessitate the patient to keep his bed; the *second*, somewhat malignant, was accompanied with fever, this being either of a quartan or of a semi-tertian type; and the *third*, of a genuinely malignant character, was associated with an acute, and, as it were, inflammatory fever; the symptoms being of the most violent description: this form was highly infectious, and proved rapidly fatal in a very large majority of cases" Of this last-mentioned form he afterwards

* It would seem, from the following passage in Diemerbroeck's treatise *de Peste*, that he took nearly the same view, as to the nature of this disease, with that which our author has expressed in the passage now quoted. From this account it appears that, "after an intensely hot dry Summer, a pestilential fever broke out at Nimeguen and other places in Gueldres, causing great mortality. In Autumn, the heat and drought still being excessive, various febrile diseases, such as small-pox, measles, and dysentery, all of a malignant and putrid character, made their appearance in different parts; but chief and foremost was the pestilential fever which, daily becoming more widely diffused, gradually assumed a more aggravated character, until at length *in apertissimam pestem transiret.*" The intimate connexion (says Dr. Harty) of the three diseases—fever, dysentery, and plague—almost simultaneously raging, is here well and clearly marked.

says that the accompanying fever, "more dangerous than the dysentery itself, was of a malignant petechial description."

The testimony of Sir James Macgregor confirms the strict accuracy of Hoffman's division; for, in the account which he has given of the dysentery which prevailed among the British troops during the Peninsular campaigns, he distinctly asserts that, in some cases the disease, from its commencement, appeared to be unattended with fever, and, in a short time, ran into the chronic stage. In very many cases, the disease was connected with intermittent fevers, and these indeed were frequently observed to terminate in dysentery. "The type of fever, however," he continues, "accompanying dysentery was very much modified by that of the prevailing epidemic. In the hospitals in the Alentijo and Estremadura, in 1812, intermittent fever prevailed or accompanied dysentery, and remittent fever, when the army advanced so rapidly and remained some time stationary in the two Castiles, in July, August, and September. Every case of dysentery which appeared in the battalions of the Guards in 1812 and 1813 was accompanied by the *typhus gravior*, and very generally had a fatal termination, as did many at Ciudad Rodrigo, &c., where the same form of fever was prevalent." At this place, the situation of which is unhealthy, aggravated by the interment of 20,000 bodies in the course of a few months, the mortality was great.

Granting now that the position with which we started—viz. the threefold division of dysentery, according as it is unattended with idiopathic fever, or is associated with periodic or with continued fever—has been satisfactorily made out, it will not be necessary for us to occupy any space with proving that it is the last-mentioned form or species of the disease which is liable to exhibit infectious properties. A few short extracts from two or three of the leading authorities will be all that we can afford room for. Zimmermann has, in one place, remarked that "it appeared that our dysentery in general became contagious purely through nastiness and the crowding many people together in a small space, but was by no means *so itself;* for, though many were attacked with it at once, this seems to proceed from a more universal and widely-different cause, which operated at once upon every one."

In another passage he observes:

"'The benignant species of dysentery becomes contagious, malignant, and extremely dangerous, when many sick people are crowded together in a small space, or when peculiar external or internal causes produce malignity in particular persons:' and he adds, that he does not see 'that camp dysenteries are in themselves more malignant than those that happen in cities, although in the army and military hospitals they become excessively malignant and contagious from several circumstances: the same, however, takes place in cities, when a great quantity of people, attacked with this disease, are crowded together in a small space, or where the other different causes subsist of a peculiar or general malignity.' And in p. 151 he says: '*When hospitals are filled with dysenteric people, some of the assistants are attacked only with dysentery, and others with the jail or hospital fever, that ends in bloody and gangrenous stools.*' 'From all these observations, made partly by me, and partly by other physicians, I conclude,' says Zimmermann, 'that the dysentery is very often only *accidentally* contagious,' but that it also frequently becomes essentially so just before the death of the patient."—P. 134.

Hillary, after describing the endemic dysentery of Barbadoes, and attributing it to the atmospheric vicissitudes of that climate in particular months of the year, goes on to say:

"It is also probable that it may be sometimes produced by *infectious miasmata*, exhaled from diseased bodies, and floating in the air, which are received into the mouth when we breathe, stick there to the saliva, and are carried with it down into the stomach and intestines, where they produce all the above-mentioned symptoms, when they meet with a constitution fitted by the above mentioned causes to receive those infectious effluvia, and to produce the disease." "And thus the disease becomes both epidemical and contagious, though it was not the latter at its first invasion, or seizing the first patient. This I have often observed, especially when *great numbers* have labored under it at the *same time*, as often happens among the *negroes*."—P. 116.

Diemerbroeck, who unequivocally maintains that the dysentery, which prevailed in Nimeguen at the same time with the pestilential fever, of which he has given so accurate a description, was malignant and infectious, subsequently mentions another epidemic of the same disease, whose spread was (he says) owing not to infection, but to an unhealthy character of the season, in conjunction with the bad description of food that the lower classes lived upon. Frank very distinctly attributes the infectiousness (when present) of dysentery to the nature of the fever with which it happens to be associated: in other cases, the disease is, in his opinion, not communicable, from one person to another. The late Dr. Fergusson,* whose experience of the disease in different climates was very extensive, says:—"Dysentery, I believe, I can declare to be in no case contagious. I never saw anything like it except in Holland, after the weather became cold, when it was seen as a local symptom, or irregular form of typhus fever; but more could not then be said than that it was about to be swallowed up in the prevailing epidemic, and *formed a combination* rather than a distinct type of disease." The same sentiment is reiterated in the article on dysentery in his recently published posthumous volume, entitled "Notes and Recollections of a Professional Life." Another of our late army physicians, Dr. Somers, has expressed an entirely similar opinion. He denies a specific contagion to the disease, and states that he "never witnessed the production of contagion by *pure unmixed* dysentery." He had, however, met with some cases "in which low fever and chronic dysentery were co-existent: in those, unquestionably, there was contagious disease; but, this evidently arose from the superinduction of typhus upon dysentery."

These passages will doubtless suffice to convince every candid reader that dysentery is liable, under certain circumstances which have been specified, to become infectious, or transmissible from one individual to another. It will be seen from the following extract that, the views maintained by Dr. Copland on this important subject, are not exactly those for which our author contends:

"Dr. C. endeavours indirectly to negative the propositions I have been contending for, that the combination of dysentery with typhus is the special cause productive of contagion in the former disease:

* *Medico-Chirurgical Transactions*, Vol. 2.

'Many writers,' he says, 'conceive that the asthenic varieties are complications of simple dysentery with different varieties of fever, and that when they are infectious it is not the dysentery but the fever which possesses the property. *Some* authors suppose that the typhoid variety especially is a complication of this description. But if such,' he adds, 'be the case, wherefore should the disorder which is communicated be always dysentery and not fever? Moreover, this form of dysentery is often present where a case of typhus cannot be found.' He then summarily disposes of the whole question by asserting 'the fact as incontrovertible, that the asthenic forms are direct and necessary, and uniform results of certain diversified but concurrent causes, and not contingent associations of two diseases capable of separate existences.' The fact thus laid down as 'incontrovertible' being not merely a *petitio principi*, but the very assertion of the questions at issue, I shall pass it by, and proceed to examine his two arguments; and *first* as to the inference he would have drawn from the questions above quoted: 'Wherefore should the disorder which is communicated be *always dysentery* and *not* fever?' The question is doubly erroneous, inasmuch as the disease sometimes communicated is the fever and not the dysentery; as, for example, when, according to Zimmermann, hospitals are filled with dysenteric people, some of the assistants were attacked only with the dysentery, and others with the jail or hospital fever: and that 'this fever may attack people in health without being attended with the dysentery, though it arise from the putrid and confined vapours of that distemper.'* Again, the dysentery that is communicated is not dysentery merely, but dysentery in combination with the fever. *This* assertion of an 'incontrovertible fact' is amply borne out by the multifarious authorities already quoted, such as Pringle, Hoffman, Zimmermann, Vignes, Grimm, Frank, &c. His *second* argument consists in the statement that 'this [the typhoid] form of dysentery is often present where a case of typhus cannot be found.' Now for this statement no authority is given, such as might be investigated; but is not Dr. C. well aware that, under favourable circumstances (such as those recapitulated by Frank and others), there is no disease more readily superinduced or spontaneously generated than typhus, a position fully supported by the several authorities already quoted in illustration of this very typhoid variety, and which I have myself further illustrated in my work on the Epidemic Contagious Fever of Ireland (see p. 161, *et seq.*) All these authorities distinctly show that the circumstances under which contagious dysentery rages, are the same with those which give origin or aggravation to the prevalence of typhus." P. 140.

We have left ourselves no room for any observations on the Treatment of Dysentery, although the consideration of this part of the subject is necessarily fraught with most interest. But as no sound therapeutic principles can be laid down in reference to any disease, unless we have clear notions as to the real nature of the enemy that we have to contend with, the preceding pages will not be without some interest to the practical physician, if they serve in any degree to teach him how to discriminate, with greater accuracy than before, the kind of dysentery which he may be called upon hereafter to treat. At all events, they will enable him to account for many of the conflicting statements to be found in the

---

* "Nous ferons remarquer," says Vignes, "que l'infection d'une maladie ne produit pas toujours la meme espece d'affection: l'infection de la dysenterie peut, par exemple, occasionner une indisposition bilieuse ou une fievre adynamique, &c.; de meme qu'on peut etre affecté de celle-ci (la dysenterie) par toute autre exhalaison deletere que celle des dysenteriques."

writings of different authors, upon some of the most important points in the general history of the disease. They will show him the reason why a rigid antiphlogistic treatment may be required at one time or in one set of cases; why at another time, and in another locality, no cure can be effected without the aid of bark; and why, in a third set of cases, the chief reliance must be placed upon the adoption of such hygienic measures as will secure cleanliness, pure air, a regular supply of light, nourishing food, and, it may be, the contemporaneous use of wine and other cordials. For particulars upon these matters, we must refer the reader to Dr. Harty's work, which, we hesitate not to say, should be carefully perused by every member of the profession without exception, from the experienced veteran, to him who is just entering upon the duties of active practice. To the young physician it furnishes an admirable pattern of what a medical book ought to be; not the hasty offspring of an impatient ambition, eager for notoriety, it matters little how obtained; but the well-considered production of a studious and philosophic mind, whose chief aim has been to benefit medicine, and promote the welfare of mankind.

On Wounds and Injuries of the Abdomen and the Pelvis. Being the Second Part of the Lectures on some of the more important Points in Surgery. By *G. J. Guthrie* F.R.S., &c. 8vo. pp. 72. Churchill, 1847.

In fact, this second portion of the work we had a short time since occasion to notice so favourably, has never appeared in the form of "Lectures," or enjoyed the very questionable advantage of being interred amidst the masses of unreadable matter, which fill up the overgrown columns of our weekly cotemporaries. We much regret, however, to find that the non-delivery of the lectures was occasioned by the occurrence of a domestic calamity, which temporarily unhinged the robust mental constitution of our veteran surgeon. Happily he was possessed of ample materials for the illustration of the subject he had contemplated lecturing upon, and the arranging these for publication served at once to beguile away his Winter evenings—(how much is expressed in these few words, those who have suffered from bereavements alone can tell)—and to present another useful work to the medical public. Like its predecessor, it chiefly consists of succinct accounts of cases which came under the author's observation during his campaigns, with subjoined commentaries and practical deductions He has, however, availed himself likewise of the labours of other writers upon military surgery, so that his work presents us with a brief epitome of what is known upon the subject. We prefer presenting the general conclusions, with which he terminates it, to our readers' notice, before offering any comments upon any of them. They are as follow:—

"1. Severe blows on the abdomen give rise to the absorption of the muscular structures, and the formation, in many instances, of ventral hernia; which may in some measure be prevented, during the treatment, by quietude, by the local

abstraction of blood, and by the early use of retaining bandages. 2. Abscesses in the muscular walls of the abdomen, from whatever cause they arise, should be opened early; for, although the peritoneum is essentially strong by its outer surface, it is but a thin membrane, and should be aided surgically as much as possible. 3. Severe blows, attended by general concussion, frequently give rise to rupture of the solid viscera, such as the liver and spleen, causing death by hæmorrhage. When the hollow viscera are ruptured, such as the intestines or the bladder, death arises from inflammation. 4. Incised wounds of the wall of the abdomen, of any extent, rarely unite so perfectly, (except perhaps in the linea alba) as not to give rise to ventral protrusions of a greater or less extent. 5. As the muscular parts rarely unite in the first instance, after being divided, sutures should never be introduced into these structures. 6. Muscular parts are to be brought into apposition, and so retained principally by position, aided by a continuous suture through the integuments only, together with long strips of adhesive plaster, moderate compression, and sometimes a retaining bandage. 7. Sutures should never be inserted through the whole wall of the abdomen, and their use in muscular parts, under any circumstances, is forbidden; unless the wound, from its very great extent, cannot be otherwise sufficiently approximated to restrain the protrusion of the contents of the cavity, the occurrence of which case may be doubted. 8. Purgatives should be eschewed in the early part of the treatment of penetrating wounds of the abdomen. Enemata are to be preferred. 9. The omentum, when protruded, is to be returned, by enlarging the wound, through its aponeurotic parts, if necessary, but not through the peritoneum, in preference to allowing it to remain protruded, or to be cut off. 10. A punctured intestine requires no immediate treatment. An intestine, when incised to an extent exceeding the third of an inch, should be sewn up by the continuous suture. 11. The position of the patient should be inclined towards the wounded side, to allow of the omentum or intestine being closely applied to the cut edges of the peritoneum. Absolute rest, without the slightest motion, should be observed. Food and drink should be restricted, when not entirely forbidden. 12. If the belly swells, and the propriety of allowing extravasated or effused matters to be evacuated, seems to be manifest, the continuous suture or stitches should be cut across to a certain extent, for the purpose of giving this relief. 13. If the punctured or incised wound is small, and the extravasation or effusion within the cavity seems to be great, the wound should be carefully enlarged, and the offending matter evacuated. 14. A wound should not be closed until it has ceased to bleed, or until the bleeding vessel has been secured, if it be possible to do it. When it is not possible so to do, the wound should be closed, and the result awaited. 15. A gun-shot wound penetrating the cavity, can never unite, and must suppurate. If a wounded intestine can be seen or felt, its torn edges may be cut off, and the clean surfaces united by suture. If the wound can neither be seen nor felt, it will be sufficient for the moment, to provide for the free discharge of any extravasated or effused matters, which may require removal. 16. A dilatation or enlargement of a wound in the abdomen, should never take place, unless in connexion with something within the cavity rendering it necessary. 17. When balls lodge in the bones of the pelvis, they should be carefully sought for and removed, if it can be done with safety and propriety. 18. In a wound of the bladder, an elastic gum catheter should be kept in it, until the wound is presumed to be healed—unless its presence should prove injurious from excess of irritation, not removable by allowing the urine to pass through it by drops as it is brought into the bladder. 19. In all cases in which a catheter cannot be introduced, in consequence of the back part of the urethra, or the neck of the bladder being injured, an opening for the discharge of the urine should be made in the perineum, 20. The treatment of all these injuries must be eminently antiphlogistic, principally depending upon general and local bloodletting, absolute rest, the greatest possible abstinence from food, and in some

cases from drink, the frequent administration of enemata, and the early exhibition of mercury and opium, in the different ways usually recommended, with reference to the part injured." P. 72.

Ventral Hernia.—This is the never-failing consequence of a musket-ball penetrating the walls of the abdomen, and it is also usually attendant upon the division of the parietes by a sharp cutting instrument. Moreover, the same result may follow a severe *bruise* without any rupture of muscular fibres being perceptible. Of this Mr. Guthrie relates two cases. In the one, a large flat piece of shell struck the left iliac region, producing a severe and painful bruise, the whole of that side of the belly becoming first black and then discoloured. The patient recovered with little or no treatment; but, upon examining the part some months after, Mr. Guthrie found that the whole of the muscular portion of the wall corresponding to the seat of injury, had been removed by absorption, the tendinous portions having also become very thin, protruded on any effort being made, so as to constitute a circular, broad-based ventral hernia, requiring the application of a bandage. In the other case, the belly was struck by the swinging round of the spanker-boom of a small vessel in the Tagus, and the same absorption of muscle and formation of hernia resulted. Mr. Guthrie suggests that, perhaps a more active treatment of the accident at first, and the earlier application of a bandage, might possibly have prevented this consequence.

The numerous operations of late performed for the removal of ovarian tumors, have shown that, when an incision, even of great extent, has been made along the course of the *linea alba*, the union of this part may be sufficiently firm to resist any hernial protrusion. Mr. Walne sent a woman for Mr. Guthrie's inspection, in whom he had made an incision 14 inches long, and yet at the end of as many months, all was firm. In other cases, where adhesion had been prevented by the ligature from the peduncle of the tumor passing out between the edges of the wound, Mr. W. has found protrusion. So exact may the union of the divided peritoneum become, that a minute inspection after death has been required, to detect the site of the incision. Dr. F. Bird states that, of 12 cases in which he had opened the abdomen, recently examined by him, the incision in five did not exceed three inches; in five it reached from 4½ to 5 inches; in one to 8 inches; and in one to rather less than 12. With the exception of one case, the incisions were made along the *linea alba*, without involving the muscular structure. Soon after the wounds healed in general, a very marked contraction of the abdominal walls took place; so that an incision of five inches mostly became replaced by a cicatrix of scarcely more than an inch. Whenever such contraction occurred, no tendency to hernia manifested itself; while in two cases, in which the incisions were from 8 to 12 inches long, little contraction followed, and protrusion of the cicatrix, or a hernia, ensued. In one case, in which a small incision was made transversely into the outer edge of the rectus and internal portion of the obliquus externus, hernia quickly followed. In all the cases, the same mode of closing the wound was observed, viz. by the introduction of two interrupted sutures in every inch of incision, the skin alone being included.

"If it should be proved that incisions may be made with more confidence of success than hitherto has been supposed, and as these sections may lead us to believe, it will only confirm what I have constantly repeated from year to year in my lectures, that penetrating wounds of the abdomen, without injury of the viscera, when properly treated, are not so dangerous as they were generally supposed to be. In the sections made by other surgeons for the removal of diseased ovaries, ligatures have been sometimes used for the purpose of bringing the divided muscular and tendinous parts together, and although I forbid their employment as a general rule, I do not wish to imply that in very extensive transverse wounds they can be entirely dispensed with, if only to prevent the immediate protrusions which the suture through the skin might not be equal to resist in every case." P. 10.

Employment of Sutures.—The latter portion of the above extract leads us to Mr. Guthrie's views upon the mode in which sutures are most advantageously employed. His multiplied experience during the war, confirmed by what he observed afterwards in cases of ligature of the iliacs, having convinced him that the walls of the abdomen, formed of mingled muscular and tendinous structures, cannot be brought to unite permanently, save by the intervention of cellular texture, he disapproves of the endeavour to keep these parts in contact by means of sutures, which are of no permanent service, and may create much present irritation. Chelius, Graefe, Weber, and South, seem however to approve of this stitching the muscles together, although Chelius gives also directions for their immediate removal, if dangerous symptoms, such as inflammation, vomiting, hiccough, &c. supervene; but Mr. Guthrie believes that such accidents are much better obviated by avoiding the practice, which English surgeons have long discarded, as relates to other portions of the body. He brings the edges of the wound together by means of a small needle and fine silk, passing this only through the skin and contiguous cellular tissue, and employing the "continuous suture without puckering, precisely in the manner a tailor would fine-draw a hole in a coat, or a lady a cut in a cambric handkerchief." This gives a certain degree of support to the parts beneath, which is also to be further afforded by strips of plaister extending for some distance around the body, a bandage being rarely of use, while, if it causes pressure, it will be mischievous. Great attention should be paid to the position of the patient, inclining the body towards the wound, so as to keep the intestinal or omental peritoneum, if possible, in contact with the divided portion, until it has adhered to this. In such position he must be rigorously maintained without any change, until all hope of adhesion occuring has passed away.

Management of the Protruded Parts.—When omentum is the protruded part, the rule usually laid down in surgical works is to return it into the cavity of the abdomen again, and endeavour to retain it there by the application of sutures as near the peritoneum as possible. To this Mr. Guthrie demurs, believing that the best practice consists in retaining this substance between the cut edges of the peritoneum, without any strangulation or even compression however, its adhesion there forming the best barrier against the extension of inflammation. He believes, too, that the advice given to enlarge the opening into the cavity when this is insufficient

for the return of the omentum is bad, inasmuch as the obstacle to its reduction is not formed by the peritoneum but by the abdominal aponeuroses, the slightest division of which will almost always allow of the easy return of the omentum. Small wounds in the peritoneum, when covered over with healthy parts, are not usually followed by bad consequences, but the most disastrous ones may attend a large one; so that the greatest care should be taken not to augment the size of one already existing. If the omentum is adherent, inflamed, torn, jagged, or in a state of suppuration or gangrene, it should not be returned, nor should it be surrounded by a ligature, or cut off; but left to itself, and treated in the most simple manner. It will gradually retract and become withdrawn into the cavity of the abdomen. Observation of several cases of neglected penetrating abdominal wounds during the Peninsular war, convinced Mr. Guthrie of the propriety of thus leaving the omentum between the edges of the wounded peritoneum, closing the integuments over it by the continuous suture. Larrey recommends leaving the omentum which cannot be returned *in situ*, and relates cases showing the power which nature exerts in drawing it gradually into the abdomen. Mr. Guthrie has also seen such, but he has also seen fatal consequences follow both the leaving the part outside and the cutting it off. If, indeed, it be very much bruised or injured it may be cut off, but, as the general practice, he prefers returning it as far as the edge of the peritoneum, and bringing the skin over this.

The *intestine*, in any condition short of that of mortification, should be returned to the abdomen, but the directions given for this purpose by Chelius are objected to by Mr. Guthrie. These are, that the opening of the peritoneum should be enlarged when the reduction is difficult, the finger introduced to feel that the gut has not passed between the muscular interspaces, and that the patient should be so placed as to prevent the intestines pressing against the wound. The two first of these are unnecessary and mischievous, and the last erroneous, since the very best thing that could happen to consolidate the union would be to bring the intestine in contact with the divided peritoneum. When the protruded intestine is wounded, if it be a mere puncture or a very small cut, the bowel may be returned just the same, the pressure of the containing parts preventing effusion, the development of inflammation being closely watched. Wounds, however, sometimes of apparently the slightest appearance, give rise to terrible consequences, while others proceed favourably, in spite of every aggravating circumstance.

Wounds of the Intestine.—In his second and third "Lectures," Mr. Guthrie furnishes us with an elaborate view of this subject, derived from his own observations, and those of preceding writers. After describing the experiments of Travers and Gross, he observes upon these latter:—

"The important conclusions to be deduced from his experiments, 1. That wounds not exceeding four lines in length, no matter what their direction may be, are not so apt, as might be supposed, if left to themselves, to be succeeded by extravasation of the contents of the intestinal tube; and that, in the majority of cases, nature, properly aided by art, is fully competent to effect reparation. 2. That wounds of the bowels, to the extent of six lines, whether transverse, oblique, or longitudinal, are almost always, if not invariably, followed by the es-

cape of the contents of the bowel, and the consequent development of fatal peritonitis. It may, therefore, be concluded, from these experiments made on animals, as far as they can be relied upon in reference to man, that every wound of the bowel, of such extent as shall not admit of its being temporarily filled up by the protrusion and eversion of its internal or mucous coat, ought, if possible, to receive assistance from art, which can only be given with advantage in the first instance." P. 18.

That this last conclusion, founded as it may easily be upon experience in the human subject, is a correct one, may at once be admitted; and, indeed, experiments upon the lower animals might induce a fallacious security, since nature seems in so many instances to have endowed them with the power of resisting an extent of lesion to which man succumbs; and as, in them, the third tunic of the intestines, (the fibrous lamella of Cruveilhier, and the membrana nervosa of Haller), the most important of the tunics, that in which hypertrophy has its seat and in which adhesion after division readily takes place, varies much in strength and elasticity in different animals. Mr. Quekett has shown that it is most developed in reptiles, and more so in the carnivora than in man and the herbivora.

After describing the various ingenious modes by which surgeons have endeavoured to maintain the divided edges of the intestines in juxta position, Mr. Guthrie quotes the following observations from Dr. Gross' "Critical Inquiry into the Nature and Treatment of Wounds of the Intestines"

"Of the four methods, that of introducing the suture through the cellular fibrous lamella is the least objectionable, as it enables us to bring the serous surfaces into more accurate apposition. When the needle is conveyed through all the tunics, there must necessarily be some degree of puckering, whereby the mucous lining will be forced between the lips of the wound, if not beyond the level of the peritoneal membrane. By such an arrangement, the adhesive process would be retarded, and if the stitches were to lose their hold, or if the bowel should not become glued to the neighboring parts, fæcal effusion might occur, followed by its whole train of evil consequences.

"In making the continued suture, I would, therefore, recommend, that the needle be carried through the cellular fibrous lamella, or between the muscular and mucous membrane, and not across all the tunics, as is generally advised by authors. The lips of the wound should be held parallel with each other, and the stitches, drawn with considerable firmness, should not be more than one-twelfth or one-eighth of an inch apart. The needle is to be introduced a short distance, say half-a-line, from the peritoneal edge of the opening, and brought out at the corresponding point at the opposite side. The first stitch should be one line from one angle of the wound, and the last about the same distance from the other, care being taken to secure each with a double knot, and to cut off the extremities of the suture close to the surface of the tube. The instrument which I prefer, and which I employed in nearly all my experiments, is a long slender sewing needle, armed with a waxed, but strong and delicate silk thread. The operation should be performed as expeditiously as is consistent with safety, and the bowel handled in the gentlest manner possible." P. 26.

These conclusions have only their foundation upon experiments upon animals, and Mr. Guthrie at present demurs to their practical adoption.

"The continuous sutures of Bertrandi, of Travers, of Dupuytren, of Lembert, of Jobert, of Gross, and of Nuncianti, appear to resemble each other so much as to be essentially alike; the object of all being to close the bowel effectually. The

later writers have attended more than their predecessors, to turning the cut edges of the intestine inwards, so as to bring the serous surfaces external to the cut edges in contact, when the ligatures are duly tightened—an important addition to the operative process. It remains to be proved from observations and from trials made on man, whether the mode of proceeding which refrains from passing the needle and thread through the inner or mucous membrane, or that which effects this object, is the best. I am of opinion that the continued suture through all the coats of the intestine is the best; and shall continue to think so until it shall be proved that the ligature passes into the bowel in both methods; a result which is doubted, when the inner membrane remains intact." P. 26.

A punctured wound of the intestine, or one not more than a third of an inch in length does not call for interference; and even when the external wound is large we must not make it still larger for the purpose of investigating the condition of the intestines, unless any portion of these protrude, their contents are discharged, or hæmorrhage occurs. If, however, in the course of two or three hours' time we find the belly swelling from effusion or extravasation of blood, prompt interference gives our patient the only chance. The external wound must then be enlarged, the effused matter sponged up, and the bowel or artery secured by suture. So, too, we may have to re-open and even enlarge a wound which has already been closed by suture over a wounded intestine, providing swelling and ungovernable inflammation indicate this procedure. If, when we are enabled to examine the wound of the intestine, we find this to be very small, but not filled up by the mucous membrane of the gut, "a tenaculum may be pushed through both cut edges, and a small silk ligature passed around, below the tenaculum, so as to include the opening in a circle, a mode of proceeding I have adopted from analogy, with success, in the external jugular vein, without impairing its continuity." Or one, two, or more continuous stitches may be made with a very fine needle and silk, cut off in either case close to the bowel. If the person survive, the threads or sutures will be carried into the canal. When the intestine is largely injured, longitudinally or transversely, or is more or less completely divided, the continuous suture, as already stated, is to be preferred to all others. When, after wounds in the abdomen, considerable hæmorrhage occurs, the wounded vessel must be sought for. "If the blood comes from one of the mesenteric arteries, or the epigastric, the wound is to be enlarged, until the bleeding artery is exposed, when ligatures are to be placed on its divided ends, if they both bleed. I have seen this vessel tied several times with success."

There is a much greater degree of danger attendant on wounds of the small intestines, than of the large ones; that is, recoveries more frequently follow wounds of the colon in its various parts, and of the rectum, than of the jejunum or ileum.

In his third Lecture, Mr. Guthrie relates several examples of recovery after the most serious injuries by musket-balls penetrating the abdomen; and alluding to the dictum of the late Dr. Thomson, founded on what he had seen after Waterloo, that the less the operations of nature are interfered with the better the chance of recovery, properly observes that this is correct only in the absence of clear indications for interference. "When

they are present, the do-nothing system is generally followed by death. A well-regulated interference is likely to be more successful."

Effusion of blood into the Abdomen.—Petit the younger first pointed out that extravasations of blood were not diffused over and between the entire surfaces of the intestines, provided the person outlived the period of extravasation; and that general effusion only took place shortly before or immediately after death. When blood is effused in only moderate quantities, and is circumscribed by the compression of the abdominal parietes, it may be evacuated by the wound if this be sufficiently open, and the patient may recover if the accompanying inflammation is not propagated along the peritoneum, or becomes subdued. If its quantity be very small it may become absorbed; but where this is not the case it may then become a source of irritation. Petit and Larrey showed that it is then soon surrounded by fibrin, and thus cut off from the general cavity—a pouch or *foyer*, restrained within certain bounds, being thus formed. It now, however, ceases to be bland and harmless, and may, like any other foreign body, excite inflammation and suppuration. This occurs at a later period only, at the 10th, 12th, or a later day, after perhaps the original inflammatory symptoms have all subsided. With the general symptoms, pain and swelling near the wound manifest themselves, and sometimes fluctuation may be at once distinguished. If an incision be deemed advisable, it will be better first to pass down an exploring needle or a very fine trochar, and if this indicates that a bloody, purulent, or other fluid, is distending the abdomen, the original wound should be enlarged, or a more dependent opening made at once.

The formation of these pouches, reservoirs or *foyers* are, however, so rare in the natives of our northern climate, that Mr. Guthrie looks upon them as very exceptional occurrences.

Artificial Anus.—Mr. Guthrie enumerates some of the various procedures which have been devised for the relief of this disgusting infirmity, but adds nothing from the stores of his own experience. In truth, as he remarks, this portion of surgery has been almost exclusively cultivated on the continent, where the infirmity is far more prevalent than in this country. Whether the reason of this he assigns, namely, that the "hardy natives of Britain and Ireland seldom outlive the processes ending in mortification," is the correct one, we know not; but their seldomer outliving these, appears to us, at the least, a singular proof of their greater hardiness.

Wounds of the other Viscera.—We can only cursorily glance at Mr. Guthrie's numerous observations upon these. Wounds of the *Liver* are very serious, though not necessarily fatal; a few persons recovering altogether, some imperfectly, and more dying either during the immediate or the secondary inflammatory action which is set up. Pain in the right shoulder and cramps of the muscles of the arms are common symptoms, joined to those of ordinary peritonitis. Several cases of recovery after severe wounds, giving rise to the discharge of blood and bilious coloured fluids, are related; but all the cases in which the *gall-bladder* was wounded proved fatal. Wounds of the *Stomach* are generally fatal, except when

they are confined to its upper and anterior part, in which case the patient may survive. If the wound is small, it must be treated by the continuous suture, just as that of the intestines; and if the external aperture through which its contents are effused is too small to admit of the wound in the organ being sown up, it should be enlarged. This is, however, only a suggestion for the prevention of otherwise a certain death, Mr. Guthrie never having put it into force yet. In cases of wounds of this organ, the greatest caution in allowing food, especially of a solid character, must be long observed. In allusion to a case of idiopathically inflamed stomach, not marked by pathognomic symptoms, and in which suppuration had converted the coats of the organ into the appearance of a honey-comb, Mr. Guthrie makes the following general observation.

"If there be a symptom more generally observable than another in all cases of dangerous wounds of the abdomen, it is *anxiety*—not only of the mind, as shown by the countenance in a very expressive manner, but of the body, as demonstrated by the greatest uneasiness. In some cases it is a more certain sign than the pulse, of great mischief; in others, more distinctive than pain, which is sometimes referred to a part unaffected; and is in all indicative of the necessity of a corresponding attention on the part of the practitioner; for, while it is present, although other symptoms be mild, the patient is in imminent danger." P. 55.

Cases of fistulous openings into the stomach are by no means rare. Dr. Watson has collected, in the *American Journal of Medical Sciences*, for 1844, many instances of such, and of incisions made into the organ accidentally or for the removal of knives, &c. "Ploucquet, in his *Bibliotheca*, has enumerated nearly a hundred writers under the head '*Pantaphagus*,' who have related such cases, and as many more under the heads 'Vulnus' and 'Ventriculus,' and Hevin, in the 1st vol. of the *Memoires de l'Academie de Chirurgie*, has related some of the most interesting cases of those who have swallowed knives, &c. up to this time." Incised wounds of the stomach were rarely met with during the Peninsular war. Gunshot wounds were not unfrequent, and so fatal that, while Baron Percy estimates the successful cases as 4 or 5 in 20, Mr. Guthrie only calculates one-half of these.

Wounds or rupture of the *Spleen* are usually fatal, from the abundant hæmorrhage they give rise to. Mr. Guthrie, during the Peninsular war, heard of no case, in which the spleen protruded through a wound, which recovered; although instances are related by authors in which this organ was successfully removed after injury. Wounds from bayonets, swords, &c., are not always fatal, since cicatrices are sometimes found in the organ corresponding to the external wound. Wounds of the *Kidney* are not so fatal as those of the spleen, although the complications they eventually give rise to render them scarcely less dangerous. Owing to the lingering character of some of the results, these are often unknown, the patient being lost sight of. The opinion once entertained, that wounds of the *bladder* are necessarily fatal, is now known to be erroneous; and when this organ is wounded below, or where it is not covered by peritoneum, patients recover often almost unaided. Dr. Thomson saw fourteen cases recovering at Brussels after Waterloo, in which the bladder had been penetrated by musket-balls. Mr. Guthrie has never seen a case

recovery after the urine has found its way into the general cavity of the abdomen, the patient generally dying in from three to six days.

Treatment of Injuries of the Abdomen.—Mr. Guthrie protests against the practice once in vogue of bleeding a patient immediately after the injury, thus delaying the necessary reaction, and preventing the agglutinative process which may prevent inflammation extending to the peritoneum. The abstraction of blood must entirely be guided by the existence of inflammation; and although, on the occurrence of this, very large quantities may have to be taken away, he is certain that he has seen cases in which depletion has been injurious from excess, the marks of intense inflammation after death having been wanting. However, in various parts of his work, he shows that he is an advocate for a very free use of the lancet, the employment of entire abstinence, and the other usual means of subduing violent inflammatory action, which is the character traumatic peritonitis, occurring in robust subjects, usually puts on. Indeed the idea, during our perusal of the cases, has several times occurred to us, that the subsequent reparative power was, in all probability, impaired in many of these, by the energy of the prior proceedings, and that the substitution of large doses of opium, after at least the first bleeding, and the permission of a somewhat better diet, would have better vanquished the enemy. However, the powers of opium in analogous cases is of comparative recent discovery, and the battle-field, with all its dreadful privations, is not the place we look for other than surgical achievements. The axiom that "no purgative medicine whatever should be given to a person with a penetrating wound of the abdomen," is strong and justly insisted upon.

We have perused Mr. Guthrie's work with great pleasure, and strongly recommend it to the notice of our readers, distinguished, as it is, by the same independent spirit of observation, and the ability and originality which characterize his other productions.

THOUGHTS ON THE NATURE AND TREATMENT OF SEVERAL SEVERE DISEASES OF THE HUMAN BODY. By *Edward J. Seymour*, M.D., F.R.S., &c. &c. Vol. I. Octavo, pp. 260. London: Longman & Co. 1847.

THE contents of this volume consist of four papers on the interesting subjects of—Diseases of the Stomach—Gout—Mental Derangement—and Sciatica. We shall at once proceed to examine the observations of our author upon each in succession, selecting those parts that are of most value, and pointing out, as we go along, what seem to be the chief merits and defects of the work.

The first chapter might have been more appropriately entitled, "Thoughts or observations on some of the more common symptoms of Diseases of the Stomach;" this being the real nature and scope of its contents. The first symptom that is considered is *pain*, and uneasiness in the epigastric re-

gion. It is technically known by the names of *cardialgia*, *gastrodynia*, *gastralgia*, &c. The cause is very different in different cases. Very often it is dependent upon an undue amount of acid secretion in the stomach. In such cases, there is not unfrequently, at the same time, great irritability of the heart, giving rise to many symptoms simulating those of organic disease of this organ. Inordinate pulsation in the epigastric region is a common accompaniment of this state. All the symptoms, however, give way to the use of antacids with sedatives, or of mild stomachic medicines. Dr. Seymour gives the preference to a powder composed of *rhubarb*, *calumba*, *cinnamon*, and carbonate of *soda*—taken before dinner or at bedtime, for a fortnight at least.

When the cardialgia is not accompanied with heartburn or vomiting, "it may be believed to arise from the stomach being thrown into irregular contraction." The best remedy in such a case is a combination of the trisnitrate of *bismuth* and *magnesia*. The *hydrocyanic acid*, also, may often be used with much advantage. If the bowels are torpid, mild aperients will be required every second night or so.

"Where the pain has resisted this treatment, and the emaciation is very great, so as to give alarm lest there should exist organic disease, as of fungus or cancer, the best remedy is a grain of *opium* thrice daily, the bowels being kept open by means of injections; the food being animal principally, and in *small quantity*." P. 7.

The case of the poor Italian, quoted in illustration of this practice, is set down as an example "of over-secretion of acid in the stomach," cured by *opium*. It might be so; but the report of the case does not bear this opinion out.

Pain of the stomach immediately after taking food is not unfrequently indicative of gouty derangement of the system. Even then, Dr. Seymour appears to think highly of the use of opium (a grain three times a day), the bowels being kept open by enemata. From its well-known effects of usually increasing the formation of urea and uric acid, it will be always right to associate its use with that of alkaline medicines.

Pyrosis or waterbrash, when dependent upon a mere functional derangement of the stomach, is best treated with the compound *kino* powder, gr. v. ter die; the diet to be light and nourishing, and weak brandy and water allowed as a beverage. This symptom, however, is not unfrequently an accompaniment of medullary carcinoma of the stomach—a form of cancer, which, alas! often attains to a most serious extent, before the presence of organic mischief has been even so much as suspected. "It is, therefore," Dr. Seymour observes, "incumbent on the physician, when there is waterbrash, and especially if the patient be upwards of thirty, to inquire particularly as to the state of the epigastrium."

Another cause of severe pain at the pit of the stomach is the presence of a gall-stone, either in the gall-bladder, or in its passage along one of the bile-ducts. As a matter of course, if the free excretion of the bile is interrupted, symptoms of jaundice supervene; but, as long as there is no decided obstruction, the fæces and urine may retain their healthy colour, and the skin not exhibit any icteric tinge.

In another set of cases, which generally occur in those—women more especially—who are "broken by servitude or distressed by wayward affec-

tions," the pain may be indicative of Ulceration of the Stomach going on to perforation. This lesion often gives rise to very profuse hæmatemesis. The following is the description which our author gives of the usual course of such a case:

"The chronic ulcer of the stomach is not to be distinguished by any known signs, unless vomiting of blood, which has followed long-continued symptoms of pain and distress in the stomach, has taken place. Up to this time, it is easier to say what the disease of the stomach is not, than what it is.

"The patient has long felt pain after eating, without the symptoms of the presence of acid which I have before described. At one time one species of aliment, at other times another, seems to aggravate the symptoms. Solid meat is generally felt to be most distressing: and the state of emptiness is the state accompanied with the least pain, as in a case related by Mr. Travers in the Transactions of the Medical and Chirurgical Society. Suddenly the symptom of pain becomes accompanied by constant vomiting and a painful burning of the epigastrium, with extreme pain in the opposite vertebræ; the pulse becomes very small and weak; and cold extremities and shrinking features denote the approach of death, which occurs in a few hours after the aggravation of the symptoms: and on examining the body, perforation of the stomach.—simple nearly circular ulceration, without any thickening of the edges, without any inflammation of the surrounding mucous membrane.—is seen. In such cases the patient has not lost flesh, as in the case of cancerous or fungoid disease. No examination, however careful, can distinguish, before death, either hardness or fullness: nor has the appearance of the tongue shown by its bright redness that serious irritation is established in the stomach: neither does the external surface of the body present the straw-coloured appearance, arising not only from the absence of red blood, but as if the vessels were filled with size." P. 18.

Life is sometimes prolonged for weeks, and even months, after perforation of the stomach has actually taken place; adhesions having already been formed, so as to prevent the ready extravasation of its contents into the abdominal cavity. Dr. S. relates two cases: in one, the patient survived twelve days, and in the other five months. Both were young females.

When, from the preceding pain and subsequent vomiting of blood, there is reason to suspect the presence of simple ulceration of the stomach, Dr. Seymour strongly recommends the internal use of the oil of *turpentine*—3ij., with as much castor oil, being taken in the form of an emulsion every, or every second, morning.

"I have never," says he, "seen a case which did not recover for the time being from this practice. In three cases where the patients had suffered from this disease, and the hæmorrhage had entirely ceased under the use of the oil of turpentine, healed ulcers, into which small branches of arteries could be traced, were observed on the surface. I believe, in all, on the posterior surface of the stomach: but in a great many more cases, where the patients are still living after a lapse of several years, and where of course the case cannot be proved, the disease gave way to the use of the oil of turpentine. Not only were large quantities of blood vomited in such cases, but large quantities passed half digested from the bowels, under the form of thick matter, resembling soot in appearance. I do not remember to have witnessed a single case in which this treatment has failed: and, notwithstanding that the medicine is very nauseous, it rarely happens that it is rejected by vomiting.

"I am not, however, to be understood to say that hæmatemesis always arises from ulceration of the stomach: that it does so in some instances has been proved; in others it may arise from simple exudation, or from vicarious menstruation; occasion-

ally from diseased liver; and occasionally from fungoid or cancerous disease of the stomach." P. 27.

If the turpentine excite much nausea, or otherwise disagree, the diluted *sulphuric acid* or the *acetate of lead* may be given; but neither of these medicines is equal in efficacy to the turpentine. The *liquor stypticus* of Ruspini has been used with decided advantage—3 ij. every four or six hours.

At this part of his subject, our author flies off to the consideration of that most hackneyed subject—"Diet in general." There is nothing, however, in his remarks that deserve notice, although he has favoured his young friends in the profession with a recipe for making beef-tea, which he purchased from a first-rate French cook! We pass on, therefore, to the notice of another common symptom in stomach-complaints, viz. *Vomiting*. Every medical man knows that this is a frequent, and sometimes not only a distressing but even an alarming, accompaniment of early pregnancy. It is then often best relieved by one or two small bloodlettings. At other times, the use of *opium*, alone or in an effervescing draught, will be found most effectual. A mustard poultice, or a blister, to the epigastrium will often succeed. Confining the patient to a diet of equal parts of milk and lime-water has answered, after other remedies had failed. The vomiting of hysteria has sometimes yielded most speedily to *creosote* ℥j.—ij. ter die. Minute doses of *strychnine* have been much recommended by some writers in obstinate hysterical and parturient vomiting.

Dr. Seymour makes the remark that "vomiting in phthisis, occurring almost always after coughing, if it arise early in the disease, is the proof of a severe and rapid form of it; if late, it betokens that large collections of matter are locked up in the lungs, that is, have not yet found an outlet through the larger branches of the bronchi."

He briefly relates one case, and refers to several others, in which an accurate diagnosis was formed from attention to the symptom now mentioned. Dr. S. might surely have illustrated this point—one, according to him, of much practical importance—with greater minuteness and circumstantiality of detail. Equally unsatisfactory are his remarks on the iliac passion, of which he has seen three cases, in all of which recovery took place. Strange to say, he does not as much as allude to the use of enemata for the purpose of inducing "a strong (peristaltic) impression downwards,"* in the obstinate vomiting, which is the worst symptom in this distressing disease, as well as in carcinoma of the stomach; although he thinks it worth while to tell us that the late Dr. Cholmely was in the habit of using crude mercury (in doses of 3 iv.) for the purpose, and that "in a very obstinate case of vomiting, also from organic disease, great relief was obtained *temporarily* from a scruple of calomel prescribed by Sir. H. Halford."

---

* We have repeatedly found the most obstinate vomiting give way to the use of a purgative injection, leaving the stomach entirely quiet, without either food or medicine being given for six, twelve, or twenty-four hours. The application, at the same time, of a mustard poultice to the epigastric region will often very materially assist in checking the inverted peristaltic action.

Tubercular Disease of the Peritoneum is generally attended with very troublesome vomiting; and the appearance of the vomited matter is in many cases so peculiar, that it affords, in our author's opinion, a diagnostic sign of the disease. It is of "a green so dark that it is only to be compared with that which the sea acquires at great depths—a blue as intense nearly as that of indigo; deep green when regarded in one way, blue in the other." The disease may exist without this symptom; but "when this symptom is present, it is diagnostic of the disease." We must not omit to mention that Dr. Seymour maintains that tubercular disease of the peritoneum is of much more frequent occurrence than mesenteric disease. Nay, he goes so far as to say, that "mesenteric disease is very rare, and the disease in question by no means so; in fact, in hospital and private practice, where I have had occasion to verify the disease after death, this occurs, at the least, in proportion to mesenteric disease, as five to one; and, I believe, in a still higher ratio."

The experience of most physicians will certainly not agree with that of our author as to the great rarity of mesenteric disease in children.

Vomiting, often of the most intractable description, is a conspicuous symptom of the passage of a Calculus along the Ureter. It is, of course, very important to distinguish the true cause, from the first.

"The distinction is to be found in the fixed pain in the direction of the ureter, most frequently about midway between the umbilicus and the spine of the ilium, but still more in the slow soft pulse, notwithstanding the intense pain which accompanies the disease. In inflammation, the universal pain over the abdomen, the patient carefully keeping the trunk of the body at rest, the pulse being small, hard, and very quiet, the alteration of the features, well distinguish it from the acute fixed pain, the constant vomiting, the throwing about of the limbs, the numbness in the thigh of one side, the patient seeking the half-recumbent posture, which appears to relieve the pain, which constitute the form and the picture of renal colic. The sickness, however, in females especially, is often frightful. I have seen it, almost without ceasing, continue for eight days consecutively, but there was no fever, no constipation, no wasting of the body; and a loaded state of the urine, or the passage of a small calculus, soon put a stop to the symptoms." P. 55.

Our author next alludes to the vomiting which attends Cholera, the Asiatic as well as the common form. He briefly says with respect to the latter, that, "almost always, the mixture of three grains of calomel with a grain of opium, will arrest the disease;" and straightway he tells us a story about a young gentleman curing his friend at Padua, by this prescription! So infallible are its virtues in the estimation of our author, that he thinks it worth while to put upon record this interesting piece of information:—

"For many years I have been in the habit of giving this prescription to friends of mine, or members of my family, when on foreign tours, and very often they have had occasion for its use: never has an evil resulted, nor has it failed of its efficacy in a single instance. To my knowledge, in the course of the last twenty years, the number who have benefitted by it is very great." P. 59.

After this, who will be surprised to hear of "Dr. Seymour's Pills" for the use of travellers?

The last ten pages of this paper are occupied with rambling remarks on *gastrite*, and softening of the mucous membrane of the stomach; but there is nothing in them of the slightest value.

The next subject that Dr. Seymour discusses is Gout. No fewer than 80 pages are devoted to its consideration. Dr. S. acknowledges that there are "hundreds of books" on the subject; but still he thinks that something remains undone.

"I do not know," says he, "that I can say anything new, but the arrangement of our present knowledge on the subject, our treatment and the reasons for that treatment, are not satisfactorily stated, in my own view of the case, concisely and for practical use. It is to supply this (temporary if it be) want, that, having experience on this subject, I undertake the task of making the following observations." P. 72.

Let us now see what is the nature of these observations. After a brief historical sketch of the disease as known to the Greeks and Arabians, the question "what is gout?" is propounded for solution. The old physicians, it is well known, "recognized a specific morbid fluid mixed with the blood, which was thrown off upon the joints, thus purifying the blood, and producing in its elimination what we now call an inflammatory condition of the joints and tendons." Dr. Seymour's account of the modern theories of the disease is certainly not very lucid. He thus describes the opinions of English and French physicians in the present day.

"1st. The opinions popularly held in this country attribute the gout to vicious secretions of the stomach and liver; after some time, these derangements are followed by inflammation in the feet or hands, such inflammation being a natural termination or consequence of this disorder of the viscera, and of their suppressed or vitiated secretions. This is most generally the only explanation to be derived from those who have been taught in early life, that all diseases of the human body depend on disease or disorder of the secretions of the liver or of the bowels.

"2nd. The opinion most prevalent in France is, that the cause of gout is the indulgence in too succulent and nutritious a diet.* All the textures of the body become gorged, and more nourishment is afforded them than can be removed by excess of excretion. Two modes of excretion, the urinary discharge and the cutaneous perspiration, keep up for some time the equilibrium between the ingesta and egesta, but sooner or later it happens that these outlets are not sufficient, or one of them is completely obstructed by some temporary cause, and thus the over-nutritious particles, which ought to be carried out of the body, are transported to the fibrous articular structures. The nutrition is greatly increased in these structures, otherwise almost insensible, endowing them in some measure with a new species of vitality. They are transformed from parts formed for motion and passive resistance to sensible and irritable parts, no longer able to fulfil these functions, and disposed to become inflamed spontaneously, or from an ordinary exciting cause. Again, these nutritious materials in excess are deposited on the surface of the articular structures, and form the concretions composed in the greatest part of animal substance, as they for the most part consist of azote and animal matter.

"3rd. A still more modern theory supposes that the acid matter which is

---

* "The over animalization of the blood, consequent on an over nutritious diet redundance of thick and viscid bile, explains the frequency of biliary calculi and gout. The secretion of bile affords one exit for the overnourished fluids of the body; still more the urine, and the highly loaded urine in gouty persons, sufficiently attests the state of the blood, which is far more than able to supply the necessary secretions in their normal state, and the nutrition of the body."

distinguished in the urine and in the perspiration, and in the concretions in the joints, is formed in the blood, and very recently the changes in the blood produced by electricity are believed to form, in the living body, this acid so uniformly present in gouty excretions." P. 77.

We really were not aware that British physicians generally attributed the disease merely to "vicious secretions of the stomach and liver," nor the French to mere plethora—not *congestion*, as Dr. S. inaccurately terms it. This is an off-hand way of writing that is surely very objectionable: nor can we admire the accuracy of an author, who tells us that, gouty concretions "consist, for the most part, of azote and animal matter." Dr. Seymour's explanation of the hereditariness of gouty complaints is given in these words:

"The blood is more highly animalized, and filled with noxious particles, which are thrown off by inflammation of the extremities, termed gout, when no longer to be borne; and this state and condition, when children are procreated, may be transmitted to them, as scrofulous virus and syphilitic virus notoriously are." P. 78.

But is there not this very marked difference?—that, whereas the signs of what our author calls the scrofulous *virus*, as well as of syphilis, are manifest in a child from its birth, those of gout seldom show themselves before the period of adolescence? Dr. Seymour is scarcely correct even with respect to the ordinary exciting cause or causes of the disease in question. Is it really the case that those, whose blood is most highly animalized by "eating succulent food, with habits of a sedentary kind," are most subject to it? Such persons may become excessively full, bloated, and plethoric; but, if they have not indulged in the use of fermented or vinous liquors, they will very rarely indeed become gouty. In short, it is less what is eaten than what is drunk that predisposes to the disease. *Spirit* "drunkards, simply speaking, are not gouty:" Every one knows that. But can the same thing be said of those who are, or have been, in the habit of drinking their strong ales, and stronger wines, or both of them, as is often the case, together? No wonder that the only gouty patients to be seen in the hospitals, are "decayed butlers and housekeepers." Dr. Seymour tells us that he once admitted four cases of gout into St. George's Hospital on the same day; and he goes on to say that he bid one of his pupils "ask those patients, quietly, how much meat they have been in the habit of eating daily?" The answer was, in one case, three times; in all the rest, twice daily, and largely. 'And how often had they left the house?' 'Not for weeks together!'"

True; but had not these butlers and housekeepers, besides their fat feeding and their lazy habits, been tippling daily with their wine and ale? The last element is quite as necessary as the two former for the production of the malady. It would be unprofitable to follow Dr. Seymour in his account of the mode of attack, and of the symptoms of gout, as there is nothing in it but what is perfectly well known to every reader. One observation only calls for notice. It is this:

"Where patients die, worn out as it is called with gout before arriving at a great age, I have generally seen enlargement of the heart (hypertrophy), and this was signified in the latter months of life by anasarca." P. 87.

The hypertrophy in such cases is almost invariably associated with dilatation of the heart at the same time. Anasarca can scarcely be called an indication of simple hypertrophy.

With respect to the treatment of Gout, Dr. Seymour has thought it worth while to enter into a minute account of a variety of the remedies that are, and have been, most in vogue. Many pages are devoted to the history of the *Eau medicinale*, and of *colchicum* and its preparations. As to the probable modus operandi of the latter, all that we are told is to the effect that "Sir Everard Home wrote a paper in the Philosophical Transactions for 1816, to prove that the efficacy of colchicum, resulted from its being communicated through the blood." What may be the exact meaning of this remark, it is not quite easy to determine. At all events, it might surely have been expected that some reference would be made to the not unimportant fact of the quantity of the urea, and probably also of the uric acid, being found to be decidedly increased during the exhibition of the remedy. Again, was it worth while to enter into such particulars about the "Portland powder," the "Chelsea Pensioner gout powder," &c., &c.? Formulas for both are given. And here we cannot but remark that most professional readers must be offended by the gossiping tone, that pervades much of our author's writing. For example, we have scraps from Horace Walpole's letters about his bootikins, Sir W. Temple's account of the *moxa*, anecdotes about the use of *burdock* tea, and so forth; and, when mention is made of the effects of the Weisbaden and Carlsbad waters in the convalescent stage of the disease, we are told that

"Their great heat, combined with the large quantity of alkali contained in them, makes them well adapted to such cases; and twenty-five years ago, in Italy, I frequently met with men of rank from the North of Europe suffering from calculous diseases in a greater or less degree, who made it a rule to visit Carlsbad annually for the sake of their health, and passed during their stay small calculi of lithic acid, while correcting the disposition to this secretion; and I well remember a Russian Nobleman, of rank so high as to have been Ambassador-Extraordinary to this court, who assured me he had gone several successive years to Carlsbad, and showed me a large number of calculi of lithic acid of the size of small melon seeds, which he had passed each year while drinking the waters." P. 117.

The subject of Hydropathy comes in for notice, and is ushered in with the announcement that "M. Priessnitz has become the head of a new sect of medical philosophers (!)," and a recommendation of *Dr.* Herbert Mayo's book upon the subject. This gentleman, who has witnessed this practice, says, as far as regards our countrymen, that the taking large quantities fasting (say a pint or two) has disagreed with many. He thinks, however, that by substituting large quantities of water during the day, and especially at meals, in the place of wine, or stimuli, not only is the health greatly improved, but that the gout may be *cured*. He expresses his conviction of this fact. But the system has been established too short a time for certainty. What is the period of ten years or fifteen years in the life of man sufficient to be *secure* from an attack of gout, or to establish a certainty? and yet this period of time has scarcely elapsed since the promulgation of the new treatment."

The chapter closes with some remarks on the forms of Cutaneous Dis-

ease which are frequently met with in gouty habits. Psoriasis is that which is most commonly met with. In many cases it is associated, especially in the extremities, with an oozing of pure firm fluid from the affected surface; so that the eruption may be truly said to be an impetiginous psoriasis; constituting, as all know, a very distressing and unmanageable complaint.

"Local applications are injurious; the mildest only can be endured. The sprinkling the part with hair powder, and the application of the Unguent. Acet. Plumbi, can best be borne.* But internally, the remedy from which I have seen the most satisfactory effect is from the use of *pitch* in large doses.

"I will not diverge here to remind the reader of the great efficacy attached to tar water as a stomachic in the last century, nor recall to his mind that *some advantage* may be derived from a remedy so long used and so much praised: but I can assert from much personal experience that the use of this remedy is most effectual—that, both in the hospital and out of it, I have seen cases of the most decided cutaneous mischief cured by the use of tar. It is more effectual when the disease appears to be connected with disorders of the stomach, and the more as it approaches in appearance externally to the scaly squamous eruption. Unfortunately for its popularity, it must be taken for a long time together, and in large quantities: on the other hand, this is the only inconvenience which results from its employment.

"In no instance has it ever produced, in my experience, when taken for several months, the smallest bad effects. On the contrary, the general health improves: if any obvious effect be produced, excepting the cure, it occasions a slightly relaxed state of the bowels, or some perspiration.

"The following is the formula which I use:

℞ Picis Liquidæ ℈ij.
Pulv. Glycirrhizæ ℈j. M.

Ft. pilulæ xvi. æquales quarum capiat duas vel tres ter in die." P. 145.

In some cases, the *arsenical* solution internally, has been used with much advantage; chiefly when the eruption is squamous rather than pustular. The decoction of the *dulcamara* has been generally considered a good vehicle for it.

The Vesicular or rather Bullar form of eruption, known by the name of Pompholyx, is occasionally met with in gouty patients, and sometimes proves very troublesome.

"The body, but especially the arms and legs, are covered with vesicles or bullæ, from the size of a pin's head to that of a large pea, and even of a larger size. There is no fever, these vesicles break and discharge, leaving deep scales. The patient thinks he is getting better, when all at once, in a single night, another crop of bullæ make their appearance. In two cases, after such an attack, the patient was free from gout, in one instance five, in the other six years.

"Arsenic alone has no effect on this disease, nor has mercury; external applications, from the extent of the surface, are manifestly inapplicable. But what these severe alterative remedies will not do alone, they effect when taken during the same course. Thus, I have seen the most obstinate cases speedily give way under five minims of Liquor Arsenicalis, twice in the day, in distilled water, and from three to five grains of the blue-pill at bed time, with or without a pur-

* The decoction of poppies, to which a small portion of Goulard extract is added, and applied tepid, will often be found more soothing than any other application. The addition of the Hydrocyanic acid also is, in many cases, very useful.

gative as the bowels may require: or the blue-pill may be given on alternate nights.

"I have seen cases which had been very troublesome for several months, cured in little more than a week by this practice, although the remedies separately had been already tried without effect." P. 147.

In the treatment of this, as well as of every other, form of cutaneous disease, more especially if occurring in a gouty habit of body, the physician will do well to have his attention directed to the state of the urine. When this is in a healthy condition, recourse may be had to the use of the *hydriodate of potash* with excellent effects in a great variety of skin complaints. It is most advantageously given with *sarsaparilla.*

From Gout we shall pass on to Sciatica, the subject of the 4th chapter, leaving that on Mental Derangement to be last considered. This is altogether better, seeing that, in not a few cases, there is some alliance bewteen these two kinds of bodily suffering. The definition of Sciatica by our author—a pain greater or less affecting the great sciatic nerve, *or even when effecting large nerves of the body*—is certainly quite comprehensive enough. He describes three forms, or rather degrees, of the malady.

"In many cases the patient only complains of a dull heavy pain at the back part of the thigh, most severe in damp weather, severe after walking, and especially on ascending a stair, often increased after a full meal, and principally after the limb has cramped by sitting. This is the milder form of disease.

"In the second form the low grumbling pain in the course of the nerve is scarcely ever absent, but in addition violent accessions come on, with comparatively long intervals of ease. In the paroxysm the pain is described as of a pistol-shot discharged down the course of the nerve, and the suffering is extreme. This is the second degree of the disease.

"Lastly, the patient is quite unable to move the limb; when moved, the pain becomes excruciating to so great a degree, that the patient will not willingly permit a friend to approach his bed, or walk quickly on the floor, lest the movement of surrounding objects, or of the bed, should renew the pain.

"Nor is this all: generally at night, most frequently about midnight, violent pains come on in the nerve; the limb, formerly motionless, now shakes so that even force can hardly restrain it; the muscles and tendons are affected by the cramp, and the unhappy patient lies screaming often for several hours in torture. Towards morning the pain becomes diminished, and the patient falls exhausted into slumbers, without which it appears to be difficult to conceive how life could be prolonged; for such paroxysms sometimes recur nightly for weeks." P. 226.

As a matter of course, the first and most important part of the physician's duty is to try to form an acurate diagnosis of the true nature and cause of the malady he seeks to relieve. He must therefore seek to discover whether it is simply and purely a local affection of the sciatic nerve, or whether the pain be but sympathetic of disease elsewhere. There are two diseases for which it is very apt to be mistaken: 1, disease of the hip-joint; and 2, diseased or disordered condition of the kidney, especially that state in which there is an excess of uric acid in the urine. Whenever sciatica occurs in a person who has suffered from the gout, we have reason to suspect that it is connected with this truly systemic disorder;

and it should be treated accordingly. "The various preparations of *colchicum* will be of great importance; but in the cases which I have witnessed, (and they were very severe), *quinæ sulphas* in large doses given in the day time, with thirty drops of the wine of colchicum in a draught at night, were most effectual; but neither separately produced permanent relief."

Some very obstinate cases of sciatica appear to be connected with a syphilitic taint; hence the continued use of *sarsaparilla*, *iodine*, &c., has proved successful, when all other remedies have failed.

When the disease is ascertained to be quite unconnected with any constitutional depravity, such as gout and syphilis, or with any disorder of remote organs, such as of the stomach, kidneys, &c., and when, therefore, there is just reason to believe that it is purely local, the use of topical remedies will form an important part of the treatment. Cotunnius was of opinion that the principal cause of the malady was an effusion of fluid into the sheath of the nerve; and accordingly his favourite remedies were leeches, cupping and blistering, with the view of causing the absorption of this supposed fluid. Dr. Seymour appears to have found more decided advantage from the use of *acupuncture* than from any other remedy in severe sciatica, not referable to constitutional disorders. Its modus operandi is hinted at in these words: "If the opinion of Cotunnius as to the pathology be correct, the benefit arises from setting free the fluid contained in the sheath of the nerve. I am inclined to be of this opinion." The remedy truly is better than the interpretation thereof.

Of the class of sedatives, *belladonna*, *opium*, and *aconite* have, on the whole, been found most useful. The first in the form of extract, has been used in many cases with decided advantage, especially when the disease appears in young females, and is connected with an hysterical state of the constitution. The dose is from one-sixth to one-third of a grain; its effects upon the system must be watched. "It undoubtedly," says Dr. S., "exercises great influence over nervous pain. I have seen it control local spasms of the face especially, and spasmodic loss of voice, in a very remarkable manner; it produces usually, at the same time, symptoms of great depression of vital power. I have seen, more than once, the terrible disease 'tic douloureux' suspended for ten years by its employment." For external use, the extract should be mixed with equal parts, or thereabouts, of soap plaister or mercurial ointment, and applied along the seat of the pain. It should not be used in the endermic method, in consequence of the poisonous effects it might produce by being absorbed.

Dr. Seymour has no great confidence in *aconite*; its effects are unequal, and cannot be depended upon. He has reason to believe that it is apt, like *digitalis* and some other medicines, to accumulate in the system, and give rise to alarming nervous symptoms. The case, however, advanced by him in proof of this is not satisfactory; seeing that the cerebral affection might have resulted from the stoppage of the chronic diarrhœa, for which it had been prescribed.

The use of the alkaloid, *aconitine*, has been highly praised by some practitioners of late years. "It has been said," says our author, "that half-a-drachm of this medicine (gr. xv. Adipis Ppæ. ℥j. Ol Olivæ ℥ss.)

rubbed through (along?) the course of this nerve, has been found productive of immediate relief,—a great happiness in extreme cases."

Dr. Pereira recommends for external application a tincture of aconite, prepared by macerating a pound of the root in a gallon and a half of rectified spirit.

It is scarcely necessary to say that, in all cases, when the pain is very severe, returning in paroxysms lasting from one to several hours of almost intolerable suffering, nothing can take the place of *opium* in some form or another. The acetate or muriate of Morphine is perhaps the best form in which it can be administered. The dose required to afford relief is sometimes enormous. Dr. S. mentions a case where as much as six grains of the acetate were obliged to be given during the attack, before any advantage was obtained.

All that we hear respecting *iodine* and its preparations in the treatment of sciatica is to this effect:—"The well-known effects of this medicine in secondary symptoms would induce its employment where the sciatica appears to arise from a syphilitic taint, but in cases where this painful affection arises as symptomatic of an over-excited state of the nervous system I feel certain that it is injurious. In one case, undoubtedly, the employment of it was followed by great increase in the severity of the pain."

It is more especially in rheumatic and syphilitic cases that the hydriodate of potash has been used with the most advantage. We have also seen it produce excellent results in neuralgic affections occurring in cachectic states of the system, indicated by the outbreak of impetiginous and ecthymatous eruptions on the skin. It should then be combined with sarsaparilla or colchicum, or with both.

When sciatica is associated with those marked states of the stomach and bowels, which are accompanied with much flatulence, there is no better remedy than the oil of turpentine. A drachm of it may be given in the form of an emulsion, prepared with the yelk of an egg, and some aromatic water, for several mornings in succession. It may also be administered as an enema in many cases with marked relief, both to the flatulence and the pain.

In his concluding observations, Dr. Seymour alludes to severe sciatica (sometimes one of the brachial nerves is the seat of the pain) being a premonitory, or at least a precursory, sign of Cerebral Disease. There is one symptom which he has reason to believe is usually present in such cases, and the existence of which, therefore, it may be of importance to attend to, as aiding the diagnosis: this is "vomiting after taking any solid food, either provoked voluntarily, or occurring with very little effort." As a general remark, whenever spontaneous vomiting has existed for a length of time, resisting all the usual methods of relief, and when there is no reason to believe any organic mischief of the stomach or of the chylopoietic viscera, the physician should suspect the presence of diseased action in the encephalon. In cases of this sort, the establishment of an issue in the nape of the neck, and the cautious administration of sedatives, regularly and perseveringly employed, may be the means of delaying the progress of the cerebral mischief.

CHAP. IV. *On Mental Derangement, especially that form called Melancholia, or the alternation of melancholia with violence, termed Lypomania or Suicidal Madness, especially in reference to the treatment of this form of disease by the employment of Opiates, or of that class of medicines called Sedatives.*

The following sentence from Esquirol is affixed to this chapter as a motto or epigraph:—"Every organic lesion, that has ever been found in the bodies of the insane, has been observed in those of persons who had never been affected with chronic delirium: in many cases no abnormal change has been discoverable, although the insanity had existed for a number of years. Pathological Anatomy has shown to us every part of the encephalon altered, suppurated, destroyed, without any chronic injury of the understanding."

The great object of Dr. Seymour in this chapter is to recommend and enforce the great utility of opiates and other sedative and hypnotic medicines in certain forms of mental alienation, where there is reason to believe that no organic lesion of the encephalon is present. The existence of such a lesion may be suspected, whenever there is loss of power in any of the limbs, liability to epileptic fits, cephalalgia accompanied by dimness of sight and loss of memory, &c. In all cases of this sort, the prognosis must be most unfavourable.

Passing over the introductory remarks,* which are, it must be confessed, most loosely and carelessly written, we proceed at once to the consideration of the very important practical point that our author seeks to illustrate. His attention was first drawn to the benefit of the use of opiates, continued for a length of time, in certain cases of insanity, about eighteen years ago, by Messrs. Berverly and Philips, the medical superintendents of Dr. Warburton's very extensive lunatic establishment at Bethnal Green. Their statement was to this effect:

"We have found the acetate of morphia useful both in the excited and the low form of insanity. We have also found it useful in cases of fixed delusions, but not of any great standing, and more useful in the low than the excited form of the disease. Of five cases of melancholy, three got well; the remaining two are certainly improving under the use of this medicine. Of five cases of excitement, two were discharged cured; one remains much improved; two received no benefit. It is necessary to observe, that we have used this medicine in several cases without taking notes, and the result was similar to the two cases mentioned, that is, without benefit. It appeared to us, that morphia did not produce the same good effect in excited as in other cases, unless there was an occasional interval of reason. In the cases mentioned, we have commenced with a fourth, and have not found it necessary to exceed half a grain; at present we have a patient taking half a grain dose every night, with decided advantage, and we

* Many of the observations at the close too of this paper are scarcely intelligible, from sheer carelessness in the manner of their expression. For example:—"The state of the memory, the principal function of the brain, which depends on the perfection of the organ, is to be carefully considered. In old people the deficiency appears traceable to ossification of the arteries, and the first failure often precedes it." Are we to understand that the decay of the memory, "the principal function of the brain," is owing to incipient ossification of the cerebral arteries? Where is the proof of this assertion?

think the case very interesting, and proving the extraordinary effect of the medicine in cases of melancholy.

"A woman of the age of thirty-six, the mother of four children, was attacked with depression of spirits while pregnant with her last child. She did not feel the attack before she quickened, but immediately after: she had a strong desire to destroy herself and children. This continued during pregnancy. After she was delivered she became worse, and attempted to commit suicide several times; and described her feelings; which is not common in such cases. She continued in this state, not fit to be trusted without a strict watch. She was sent here about two years ago; and what is extraordinary in her case is, that about noon all the feelings of the desire of self-destruction left her. This occurred within the last three months; from which time they have remained the whole of the day. Various means were tried, without effect. Our first idea, from the regularity of the attack, was to treat her disease as an intermittent; which failed. About a fortnight ago we gave her the morphia, beginning with the fourth of a grain, and gradually increasing it to half a grain. After taking the second dose, one-third of a grain, she slept all night; in the morning she was cheerful, without feeling the propensity to destroy herfelf. The third day she had a return, which lasted until noon; the dose was then increased to half a grain. The fourth morning she had not any return, and continued well until the fifth day after the half-grain dose was given, when she had a return from five o'clock in the morning until nine; a paroxysm three hours shorter than any of the preceding. She is now free from any desire to destroy herself." P. 155.

Since then, Dr. Seymour has employed this practice in very numerous cases, and with most pleasing effects. "Upwards of 70 cases (of Melancholia and Hypochrondriasis) have recovered; and I consider no case to be called a recovery, unless two years at least of unabated health have elapsed since the treatment concluded. In nearly 20 cases, the treatment has failed, or only given temporary relief." The preparation employed was the *acetate* of *morphia;* the dose being, in mild cases, a quarter of a grain in the form of solution, and, in severe ones, half a grain, increased speedily to one grain, every night at bed-time. The practice must be steadily continued, without the intermission of a single night, *for several weeks in mild cases, and for three months at least in very severe ones.* Sometimes indeed, at first, sleep is not induced; but, almost always, rest and quietude are at once obtained. Slight nausea and disturbance of the head may be felt for the first few mornings; after a short time, however, these unpleasant symptoms cease, the patient sleeps at night, and is free from pain and uneasiness during the day. In some cases, two, three, or four weeks will pass before any marked or decided benefit to the disturbed state of the mind or feelings is experienced. Still, the dose should not be increased beyond what has been already stated. Dr. S. assures us that, even when the treatment has not succeeded in curing the disease, he has never witnessed any ill effects from the use of the Morphia, provided it has been given in the manner which he directs: viz. not to increase the dose beyond that recommended, and to keep the bowels daily open. Occasionally, the application of cold (ice in a bladder) to the head has been employed at the same time; this remedy is chiefly useful when the melancholy and mental depression alternate with paroxysms of violence. The regular employment of the *tepid bath*, every or every second day, is a most serviceable adjuvant in very many cases; particularly in females, and more especially when the disease has arisen in the puerperal state. "The only

remedy," says our author, "which could, some thirty years ago, be recommended as being of certain advantage at the Quaker's Retreat at York was the warm bath in melancholia; and the same was stated to me at Charenton in the year 1819.*

There is no form of Insanity more amenable to the treatment, that has now been recommended, than that which arises immediately after Parturition. We give the report of one case:

"About nine years ago I was sent for to Islington to see a lady, who had been confined the day before. This lady had had, a short time before her expected time, a fright from an alarm of fire, and her labour, although some days after the fright, was believed to be on the whole premature; the child was living. The patient's state was dreadful; her screams and cries were under the apprehension that she was condemned, and in hell fire. The lochia had ceased to flow; the patient, young and always delicate, seemed to have almost superhuman force; but her expression and violence of fear were scarcely to be endured. The pulse was 130, or even quicker. I recommended a grain of the acetate of morphia in solution, and a warm bath, every evening; her bowels to be kept open. I did not visit her again for two or three days, and I well remember the astonishment with which the anxious friends assured me she was so much better.

"The practice was continued, and I only saw her once afterwards; the further attendance of a physician being quite unnecessary." P. 174.

When the insanity has arisen during the early months of pregnancy, it is usually much more troublesome and unmanageable. "Its character is generally that of self-accusation, alarm or horror for the husband or children; scenes of future punishment often present themselves, and a peculiar dread of taking food for fear of adding to the evil by increasing the health, and thus perpetuating the wickedness of which the patient considers herself to be guilty, is constantly present." There is often a very strong disposition in this unhappy state to commit suicide; and it may require the greatest and most watchful care to prevent this dreadful occurrence. It is not unfrequently supposed that the mental derangement will cease after the labour; but this, in Dr. Seymour's experience, has not been the case; nay, rather, "the birth of the child has been the signal for an increase of the disorder, and unless cured—and it may be cured by the means I mention—no alleviation has occurred until after another child has been born." Is this last condition, pray, necessary for the recovery of the patient?

There is much truth in Dr. Seymour's remarks as to the utter inutility—in many cases the positive mischief—of travelling and frequent change of scene upon patients afflicted with this kind of insanity:

"In every case that this course has been pursued, it has invariably been hurtful in my experience. Nothing is so common as to say—'Poor thing! give her or him a little variety; rouse them, change the scene.' In simple hysterical cases this may do, but in cases of real aberration of intellect in melancholy, two circumstances render it wrong.

"First, presenting to a mind impaired a succession of objects too quickly.

"Secondly, that the mind pre-occupied considers all this as an abomination: 'What do they drag me here for? I who am so wretched: ah! it is all very well; what a mockery in my state!'

---

* "*Se melancholiam incipientem solo aquæ dulcis balneo frequenter curasse.*"—Galen de Locis Affect. c. 7.

"Thus it is, and thus it has been forced on my mind, in a great number of examples, both of men and women, that change of place and variety is eminently injurious as a means of cure, until the melancholy hallucinations are completely at an end.

"This is in great contrast with the good which does arise from such a plan in the deep and real affliction which results from loss of friends, simply severe bodily illness, or any great moral distress. In these, if the unhappy individual can be persuaded to travel, new life comes from the exertion.

"The contemplation or the thought of what great changes occur on the face of the earth—how many thousands have equally great or worse sorrows—the very distraction of occupation—works wonders.

"In the one example the mind constantly rebels against the change of scene; in the other, the mind borne down with affliction, still in right-minded people lends it assistance to recover.

"To the really melancholic and hypochondriac it does most serious harm. I speak decidedly, for I have seen many cases retarded in their cure by the prevalent and popular opinion that variety and change of scene will benefit cases of mental aberration, either in the incipient or confirmed stage." P. 180.

The recovery is always much promoted by encouraging the patient to engage in some regular pleasing occupation. For women, embroidering is one of the very best.

In most of the cases of which we have just been speaking, the catamenial discharge is irregular or altogether deficient. When the mental malady is cured by the use of opiates, &c., the secretion often returns as one of the results of restored health; but this is not always the case. By far the most efficient remedy for the re-establishment of this important function is, in our author's opinion, the direct application of leeches to the cervix uteri, two or three days before the expected period.

"It appears to me," says Dr. Seymour, "to be very desirable to impress upon the profession the great advantage of this practice; it is equally useful in the hysterical mania of young women, which is met with not unfrequently, and here the application of the remedy is still more difficult, and the careful manner of its application still more necessary. I have seen the most serious and alarming illness disappear under the regular adoption of this remedy, and I have every reason to believe that I have seen consequences of the most distressing nature, namely, the establishment of mental derangement, averted by it." P. 184.

Very true; but then the same result may often be obtained by simpler and less objectionable measures. The very case, quoted by our author as an example of its efficacy, shows this; for the patient had recovered on a former occasion without its adoption. How comes it, too, that no chalybeate was ordered in a case of "languid amenorrhœa?" Under such circumstances, the employment of Electricity—shocks passed through the pelvis—is often of the greatest utility.

In the morose and fretful crabbed melancholy or hypochondriasis of old age, which, alas! not unfrequently terminates in suicide, the treatment by sedatives, as already explained, will often be found to be most useful. It may be necessary to have recourse to bleeding or cupping, and the use of saline aperients, if there be any symptoms of cerebral congestion; but it will be almost always useful to give from $\frac{1}{4}$ to $\frac{1}{2}$ of a grain of the morphia every night, at bed-time. There is unquestionably far too much dread of opium in any form, among medical men generally, in cases where they suspect disordered cerebral circulation. As a matter of course, its ex-

hibition requires caution and watchfulness; but this is no more than is necessary in respect of every potent remedy;—potent either for evil or for good, according to the judgment with which it is employed. The case of the poet Crabbe, as related in his life, suggests some useful reflections;

"My father," (says Mr. Crabbe's son,) "now in his forty-sixth year, was much more stout and healthy than when I first remember him. Soon after that early period he became subject to vertigo, which he thought *indicative of a tendency to apoplexy*, and was occasionally *bled rather profusely*, which only increased the symptoms. When he preached his first sermon at Muston, in the year 1789, my mother foreboded, as she afterwards told us, that he would preach very few more; but it was on one of his early journeys into Suffolk, in passing through Ipswich, that he had the most alarming attack. Having left my mother at the Sun, he walked into town alone, and suddenly staggered in the streets, and fell. He was lifted up by the passengers, and over heard some one say significantly, 'Let the gentleman alone, he will be be better by and by;' for his fall was attributed to the bottle. He was assisted to his room, and the late Dr. Clubbe was sent for, who, after a little examination, saw through the case with great judgment. 'There is nothing the matter with your head,' he observed, 'nor any apopletic tendency; let the digestive organs bear the whole blame! you must take *opiates*.' From that time his health began to amend rapidly, and his constitution was renovated: a rare effect of opium, for that drug almost always inflicts some partial injury, even when it is necessary,* but to him it was only salutary, and to a constant but slightly increasing dose of it may be attributed his long and generally healthy life." P. 163.

Dr. Seymour, we must not omit to remark, has been led by the results of his experience to the conclusion, that one of the most frequent causes of failure in the cure of uncomplicated—*i. e.* unattended with any organic lesion of the encephalon—Melancholy and Hypochondriasis, is the intimate connexion between diseases of the mind and diseases of the lungs, especially Consumption. Esquirol has remarked that "les melancholiques succombent presque toujours à des maladies chroniques, particulierement aux affections de poitrine;" and he states that of 176 patients, who died affected by melancholy, 62 were carried off by phthisis. Of the 20 cases, alluded to above, in which the sedative treatment failed of producing decided benefit, a large proportion, 12, proved fatal from this disease. It becomes, therefore, the duty of the physician to have his attention drawn, every now and then, to the state of the thoracic organs in melancholic patients, whose cases are found to resist every method of treatment that can be devised.

Dr Seymour promises us one or two more volumes of the same general character as the present. They will doubtless be acceptable to the profession; for all his writings show him to be the shrewd and experienced physician. But we must call upon him to bestow more labour upon their composition, and aim at giving them something better than a merely ephemeral reputation. We cannot admit the plea of "unceasing professional occu-

* "This is the opinion of Mr. Crabbe, notwithstanding his father's recovery, not of Dr. Clubbe; and this, in spite of recovery, is the ordinary state of alarm in the minds of people ignorant of medicine—the same as to colchicum, and to many of our most important means of cure.

"I have known at least twenty cases similar to the one related by Mr. Crabbe."

pation and much anxiety" as an apology for the hasty and superficial character of the present volume. He has only to look at the writings of his eminent friend and late colleague at St. George's Hospital, whose time and thoughts must have been quite as much taken up as his own, and he will at once perceive that the excuse put forward is more convenient than well-founded.

TRANSACTIONS OF THE PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION. Instituted 1832. Vol. XV. New Series, Vol. III. 8vo. pp. 440, Lithographs. Churchill, 1847.

THE present volume of the "Transactions" contains some interesting papers, the substance of which we proceed to lay before our readers.

I. AN EXPERIMENTAL INQUIRY INTO THE EFFECTS OF HYDROCYANIC ACID, PRODUCED UPON ANIMAL LIFE. By *Thomas Nunneley*, Esq., F.R.C.S.E.

There is no circumstance more extraordinary or more distressing than the present apathy which prevails in the public mind respecting accidental or wilful poisoning, and, although medical writers seem doomed to waste their breath in vainly endeavouring to call attention to this strange anomaly, we cannot reconcile to our conscience the omission of any opportunity of so doing. That homicidal as well as suicidal poisoning are upon the increase in this country, and that greatly so beyond what our augmented powers of detection will explain, no one who is at all conversant with what is going on in the criminal courts can be unaware: and that many of such crimes might be prevented if difficulties were thrown in the way of obtaining the instrument of destruction may at least be assumed until these have been contrived and found wanting. At present, an indirect encouragement to the perpetration of crime is distinctly held out in more ways than one. The murderer having determined upon the commission of the deed, and revolved in his mind the different modes of accomplishing it, finds that poison is pre-eminently his implement. In the first place, it is procurable, however deadly be its nature, from any chemist or country huckster, without any inconvenient questions being asked, or, if asked, readily evaded. Next, it may be so easily administered to the victim without exciting his suspicion or in directing this to an innocent quarter. Again, the symptoms its ingestion produces are so similar to, and so liable to be mistaken for, those induced by disease. It is true that the progress of modern chemical science has opposed a vast check to the hopes of escape derivable from this consideration; but while this is still so frequently at fault in the detection of poison in those cases which are fairly brought under its cognizance; and while, under the present defective means of carrying on such investigations before the Coroner's tribunals, so many others never come under its supervision at all, we cannot consent to deny there is still some validity in the hopes thus engendered. But, lastly, the crimi-

nal may have been proved to have administered the poison which has undoubtedly caused the death, and yet the experience of several recent trials assures us that he may count upon the maudlin sympathy of his jury, who, because some one or more of their number find their opinions upon the propriety of the punishment of death clashing with the moral and civic obligations of the oath they have taken, by a curious psychical operation, sacrifice the peremptory requirements of the one to the speculative fancies of the other, and turn a villain loose upon society to perpetrate new iniquities, and to add another example of the impunity to the most deadly crimes afforded by the uncertain and capricious condition of our criminal law.

We are, however, unconsciously wandering from our proper province, our concern being here with the first only of these inducements. And in respect to this, is it too much to suppose that if the murderer, oftentimes impelled by passion, or the half-wavering suicide, were compelled, by the difficulty he experienced in obtaining the venomous substance, to pause in his designs, his doing so would not frequently end in his abandoning them? That it would sometimes, there can be no doubt, and this is sufficient to call for the creation of the impediments in question. And the most provoking circumstance about this matter is, that such could be contrived, without any detriment to society whatever in any other respect, and yet we go on year after year, no one seeming to think it worth his while to take the initiative. It seems to us that the following provisions might be easily carried out, and would prove effectual to the end proposed. 1. That no person should be allowed to deal in drugs or dispense medicines who had not given proof on examination of his acquaintance with the properties of the substances he intermeddles with. In the rural districts, a few common drugs, such as senna, salts, &c., might be sold, as so many of a dangerous description are at present, by chandlers and the like. 2. That even to the qualified druggist should not be permitted the unrestricted sale of poisonous substances. Some of these (as prussic acid for example) should, in fact, never be sold, except to medical men in the ordinary way of business, and if required by the public, should only be obtainable as part and parcel of a prescription. The suspicion of evil design or the fear of accident justify such restriction upon the use of poisons which are potent in their nature and only medicinal in their proper application Others, again, of a very destructive character, (as arsenic, corrosive sublimate, oxalic acid, &c.,) some of which are the more dangerous as possessing but little taste, are sometimes required for useful purposes, as the killing vermin, cleansing substances, &c., &c. But the intention of so employing them should be rendered at least probable by their purchase being made by persons competent to give an account of it, and *in the presence of a witness*. This last precaution is indispensible, for that derived from taking down name and address, &c., is a mere farce. Even when the demand of the poison is thus legitimatized, accidents should be guarded against by mixing it with other substances, where practicable, such as fat in the case of arsenic for the destruction of animals, and by selling it only in very minute quantities, just sufficient to accomplish the end in view. As to persons who apply for poisonous drugs (as laudanum, &c.) under the plea of their habitually taking them, they should be expected to make

this out satisfactorily by medical or other testimony. 3. All substances of a poisonous character should not only be carefully labelled as such, but dispensed in papers, boxes, and bottles of a distinctive colour. That these and perhaps additional precautions would save many, very many lives, and would, after a while, be attended with little inconvenience, we feel certain; and at all events we hope the present state of affairs, by which every needy little shopkeeper is allowed thus to trifle with human life, will ere long be remedied.

The frequency of poisoning by *Hydrocyanic Acid* is a startling fact, seeing the little excuse there is for allowing any of the public to have this deadly substance in their possession. That a chemist or medical man should now and then commit suicide by its agency we can understand; but nothing but the most culpable negligence could ever allow it to become the instrument of murder or suicide in the hands of others. That it has often become so, every one knows; and it is on this account, and because great discrepancy of opinion prevails among professional men respecting the effects which it exerts upon animal life, that Mr. Nunneley has been induced to institute a series of experiments in elucidation of these, in continuation of others which he formerly performed. They are very numerous, amounting in all to between one and two hundred, performed on dogs, cats, rabbits, frogs, fish, insects, &c.; and, from their number, the careful mode in which they were prosecuted, and the details accompanying their narration, constitute the most valuable series of facts of the description we are acquainted with. It is well known that the deductions drawn from a few experiments upon animals made at an early stage of the history of poisoning by this substance were prematurely generalized, subsequent observation having negatived some of them and shown that others were not applicable to man. It became of importance, therefore, if information was to be obtained from this source, that the field in which it was sought should be sufficiently extensive. And certainly comparative experiments upon animals are justified here, if they are at all, inasmuch as the effects produced upon them by the substance are so similar to those observed in man, that many valuable indications concerning the symptomatology or the treatment of this description of poisoning may be derived.

Mr. Nunneley's experiments have shown that *cold-blooded animals* do not enjoy the immunity from the effects of this acid supposed by some; although, to produce these to the same extent as in other animals, a larger quantity, and especially a longer time, are required—several minutes often elapsing before any symptom manifests itself. In one case a frog, which afterwards died, leapt the length of a foot half an hour after the acid had been given. Even among warm-blooded animals, in which no corresponding difference in the condition of the circulation prevails, some are more easily affected than others. "Thus the rabbit and the mouse (perhaps all rodents) are more susceptible to its action than is the cat, and the young animal of the same species than the old."

From Mr. Nunneley's observations upon the *condition of the blood* in animals who have been poisoned by the acid, we extract the following:—

"The appearance of the blood to the naked eye is often materially changed, not only in colour, but it looks muddy and broken down, while its important property of coagulation is so interfered with that it seems to be the generally

received opinion that, after death from hydrocyanic acid, the blood is almost invariably found in a fluid state: yet this is by no means the case; and unless other observers should be more successful than myself, the examination of the blood will throw very little light upon the subject. If, however, any inference is to be drawn, it will tend to confirm the opinion that the action of the acid is upon the central nervous mass, the blood being only secondarily involved. Though the blood to the naked eye is often materially changed in appearance, yet when examined under the microscope no decided difference is seen between the globules of blood drawn from an animal before the administration of the acid and that taken immediately after death has ensued from it, whether the acid has been given by the stomach or through the lungs by inhalation. * * * * * * * * * It might be supposed that the action of the acid was limited to the fibrine, yet it is extremely doubtful if there be any decided change in even the fibrine, for the foregoing observations by no means confirm the idea of the fluidity of the blood after death from the acid; on the contrary, in by far the great majority of the cases the blood was found to coagulate, though by no means invariably so; possibly coagulation is somewhat delayed, and certainly in many cases the coagulum is found less dense than in blood abstracted from the body. But it must be borne in mind that, after death from almost any cause, the coagulation of the blood in the vessels is hardly ever complete, and often very imperfect as compared with that, which possibly has been abstracted from the same body only a few hours before death. * * * * So, also, though the colour of the blood is very frequently darker than natural, it is not always, as several of the experiments show. Thus, in No. 112, where out of four toads which were destroyed in a similar manner, in two the blood was florid, while in one only was it very dark; and in the cats, Nos. 89 and 98, the blood was of the brightest, most florid hue I ever saw any arterial blood, and the blood which flowed from the jugular vein of the dogs Nos. 80 and 81, was certainly not darker than usual." P. 68.

The author delivers a useful caution to experimenters in these days of hasty microscopic deduction, viz. to examine the healthy blood of the same or of corresponding animals. Having found the globules of the blood of a kitten who had been poisoned to be for the most part of a hexahedral or octahedral form, he had nearly attributed this to the effect of the acid, when, upon examining the healthy blood of other kittens, exactly the same shaped globules were observed, these being at once converted into circular ones by the addition of a minute portion of water or of the acid.

In an Appendix, Mr. Nunneley gives an account of some experiments he subsequently made by injecting the acid into the external jugular veins. Of these he observes:

"These experiments sufficiently prove, I think, that hydrocyanic acid does not produce any other effect when injected into the blood than when administered in any other way, the only difference being a somewhat speedier effect, and also, I think, somewhat more decided, so that probably a rather smaller quantity would destroy life by injection than when applied upon a membrane; though, when all circumstances are taken into consideration, I doubt whether absolutely the same quantity coming into *actual* contact, over an equally large surface, upon a mucous membrane, whether alimentary or respiratory, or even the conjunctiva, would not produce, if not as speedy, at least as decisive effects as when injected into the blood. They also confirm what has been stated as to the acid not acting upon the structure of the blood, for here not any difference could be seen in the characters of the blood; its colour and coagulation were as usual." P. 370.

In a medico-legal point of view it is of importance to remember that

Mr. Nunneley's experiments show that the acid acts with nearly if not quite as much rapidity and certainty when applied to any of the mucous membranes, as the vagina, rectum, or conjunctiva, as when swallowed. Other experiments prove, as indeed accidents had already done, that the inhalation of air impregnated with it is another certain and rapid mode of poisoning by it; "one which it would be very easy to employ, but most difficult after a few hours to detect, as the odour, being so diffusible, is very soon dissipated." Its application within the meatus auditorius and to the external surface of the body has produced but little effect.

The following are Mr. Nunneley's opinions upon the mode in which this poison influences the *action* of the *heart*.

"I do not agree in the opinion which has been expressed by some (Dr. Lonsdale, Ed. Journal, Vol. 51), that the heart only ceases to beat because of the suspension of respiration, and its consequently only containing dark blood. It rather appears to me that the cause of death is altogether different from what occurs in asphyxia from the occlusion of the air-passages, the heart being primarily, and to at least an equal extent, affected as respiration. This is not merely a theoretical question, but a most important practical one, as it must materially influence the treatment. If the heart's action only ceases on account of the want of respiration, and the consequent engorgement of its cavities with dark blood, venesection would be one of the remedies to which we should first have recourse, and from which we should expect the most decided benefit; while, on the contrary, if the state of things be as I suppose—not only that the blood itself is changed by the introduction of the poison, but that the heart is primarily affected, our reasons for having recourse to it would be much less powerful; neither should we expect the same amount of benefit from its employment." P. 72.

After stating that, although the experiments showed little benefit was derivable from venesection, that where congestion occurs it may be usefully, if moderately employed, he continues—

"Venesection may, therefore, in some few cases, in which the dose of the acid has been small, be proper and advisable enough, when practised with the understanding of why we employ it, and too much blood be not abstracted;* but as there is great loss of power as well as spasm at the time, and where recovery takes place considerable weakness and depression are for a while manifested, the taking away a large quantity of blood, in my opinion, is likely to be rather injurious than beneficial. The condition of the heart, at least at first, is one of contraction, not of dilatation. There is a persistence of contraction, not a diminution of contractibility, so long as the spasm continues, and of the left cavities, for long after. The cavities, it is true, may possibly occasionally afterwards become engorged, but this is only when the heart, like the general muscular system, becomes paralysed, and certainly is not supported by the condition of things revealed by the post-mortem examinations above recorded." P. 73.

The author concluded at first, with other observers, that the evidence derivable from post-mortem examination in this description of poisoning is merely negative, so contradictory have the reports furnished been; but,

* "Of this, in most cases, there is little danger, as the blood will not flow Indeed, I am not sure the distress in respiration is not in a great measure caused by the want of blood passing through the lungs. It appears very probable that but very little blood is passed forward by the heart."

when he examined the body immediately after death, he found the explanation of the different statements made in reference to the state of the heart and vessels. When death is long delayed, or the dose of the acid very small, the blood is usually dark, and all the cavities of the heart may contain more or less of it, especially the right, which is often much distended. If the death had been sudden, the left side of the heart, and especially its ventricle, was almost always found perfectly empty and rigidly contracted, the right side being at the same time, though not always, much distended. In nearly every instance the aorta and its large branches were, like the left ventricle, found empty. As showing that this different condition of the two sides of the heart is not dependent upon obstruction of the lungs, the detailed post-mortem examinations are referred to.

"This condition of the heart and great vessels sufficiently explains, what has given rise to much surprise, why it is that with the venous system so much congested so little blood should flow on a vein being opened. It is obvious that it cannot. The circulation is positively suspended; though there be motion of the heart, the blood is not propelled; there is no *vis â tergo*. For, as the left ventricle will not open to recieve the blood from behind, so it cannot give the onward impetus; hence, should the blood in some degree flow, or afterwards continue to drain away, this will merely depend upon the circumstance of the aperture being in the most depending position, when, if the blood continues fluid, some of it will find an exit, as any other fluid does from an opening below its level, a mode of escape altogether different from that of ordinary venesection." P. 77.

The author's subsequent observations, made while injecting the acid into the veins, also confirmed this view. In a minute, or a minute and a half, blood almost ceased to flow from even such large vessels as the external and internal jugulars.

Very erroneous ideas at one time prevailed as to the *rapidity* with which the effects of this poison are produced, and the possibility of certain voluntary acts being performed subsequent to taking it. Many authentic cases now prove that its action is by no means so instantaneous as once supposed. Mr. Nunneley believes some of his experiments tend to show that consciousness may, when the dose is not very large, be retained, after all voluntary power is extinct, and but little sensibility remains. This would indicate the spinal marrow as being more obnoxious to its influence than other portions of the nervous system, an inference likewise derivable from the violent spasmodic action alternating with paralysis which is produced. However this may be, the experiments confirm the statements now usually received of the length of time during which both consciousness and volition may be retained; for, although in a few instances the action of the poison was too rapid to admit of a manifestation of these, in most of the dogs and other warm-blooded animals about *twenty seconds* elapsed ere any symptoms were manifested, and in several a much longer period. There does not seem to be any *fixed quantity* of the acid which will invariably destroy life, much depending upon individual and varying circumstances. The more vigorous the animal, *cæteris paribus*, the larger is the quantity required. A full stomach lessens the effect, and age exerts a material influence. Mr. N.'s experiments seem to show that an animal is easily brought under the influence of the poison, just in pro-

portion as it is young; within however certain limits, for if it be *very young*, a larger dose is required than is necessary to poison one of the same species a little older. "This is so curious a fact, that were there not sufficient evidence to support it, we should feel much inclined to doubt it. Is it to be regarded as another proof of the approximation of the young of the higher species to the adult of the lower?" The degree of *concentration* of the acid employed exerted no very material influence—a moderate degree of dilution, in some cases, seeming to have augmented its effect. Neither did death follow with rapidity according to the *quantity* taken, supposing the minimum dose to suffice for speedy death. Thus, supposing 40 m. of Scheele's acid kills a dog in four minutes, it does not follow that 80 m. or more would do so in half the time. "Hence, when called to a person poisoned, we cannot, merely from the length of time he has survived, or the violence of the symptoms, determine anything with certainty as to the degree of concentration or dilution of the acid, nor, except within wide limits, much as to the absolute quantity taken."

Too much reliance was at first placed in our criminal courts upon the occurrence of the *death-shriek*, as symptomatic of this description of poisoning in man. Mr. Nunneley's experiments show that it is as often absent as present even in dogs, and in not more than one-third of the number is it very loud. When it does occur, however, it is entirely and distressingly peculiar. "It is different from anything I have heard in any other condition of dogs or other creatures, and is, I think, when present, characteristic of the poison."

Treatment of Poisoning.—Mr. Nunneley availed himself of the opportunities which his numerous experiments presented him with, of examining this important part of the subject; but with very defective results, as respects the discovery of an antidote, or even the treatment of the effects of the poison. Chlorine, alkalis, and the preparations of iron, were found unavailing for the first of these objects. For counteracting the effects, electricity was of no avail, if it did not aggravate these. Cold affusion, which has been so much lauded as a means of arousing the energies of the system, is thought of little avail by Mr. N. in any but the slighter cases of poisoning, and in these only when used with discrimination, and not too prolonged. He observed that shaking the animal briskly seemed to exert a marked beneficial effect in several cases. In bleeding, we have already seen, he has but little faith. Emetics, although immediately given, were of no avail whatever, when the dose of the acid had been large; but when this has been but small, or taken into a stomach already containing other ingesta, they may be useful. Owing to the favourable reports which have been published concerning the effects of ammonia, Mr. N. put these very freely to the test of experience. This convinced him that its efficacy has been exaggerated. Applied externally it was not of the least avail. "I think all that can be fairly adduced in favour of ammonia is, that as it may be beneficial we ought to have recourse to it, not with the expectation of deriving benefit from it in severe cases, but that in those cases where the dose has been so small as to render it uncertain which way the balance will turn, it may assist it to incline on the side of life."

II. Observations on the Operation of Ovariotomy. By *George Southam*, Esq.

Mr. Southam here presents us with the details of a case in which this operation was unsuccessful, adding to these some interesting remarks. He truly observes that, it is a duty of one who has recorded successful cases to give an account of those in which he fails, a duty, however, we fear, especially as regards the operation in question, very imperfectly fulfilled. "The suppression of the particulars" by such practitioners, he observes, "can only be regarded as a proof that the operators thought they would reflect no credit on their judgment, either in a medical or surgical point of view. These ought not, therefore, to be adduced as objections, the fault being with the operator, rather than the operation." This conclusion is scarcely correct. In ordinary operations we do not find the same refusal to publish unsuccessful cases; and we cannot but think that fear lest the medical public would regard the undertaking this one, in the cases in question, as utterly unjustifiable, has prevented disclosure so morally obligatory and vitally important for estimating the true position of ovariotomy as a legitimate operation, unsettled as this is at present amidst the crude statistical statements which have been adduced. Death from other operations does not, at least only in a very insignificant proportion of cases, occur in consequence of the case having been wrongly diagnosed, or the operation abandoned without being completed—circumstances avowedly of frequent occurrence in respect to ovariotomy. We see no reason to alter the opinions expressed in a former number of this Review,* and we entirely doubt the accuracy of Mr. Southam's statement that, "by a majority of those who have made the diseases of females their more especial study, ovariotomy is now considered as perfectly justifiable." We believe exactly the contrary is the fact, and that few of our eminent obstetricians have so expressed themselves; and even if they had, we must appeal to a more competent tribunal, and one more accustomed to estimate the difficulties and calculate the chances of surgical operations in general; and we are aware of the names of scarcely any of our operating surgeons who have as yet admitted ovariotomy as a justifiable undertaking, except in very rare and exceptional cases.

We need state only a few of the particulars of the case. The patient was 26 years of age and had had five children. The tumor was first perceived ten months prior to the operation, had rapidly increased in spite of bandaging, frictions, and was complicated with ascites. Its diagnosis from uterine disease was a matter of some difficulty,—Dr. Simpson's sound obscuring, rather than aiding this. As a difference of opinion prevailed concerning the origin of the tumour, Mr. Southam made an incision but three inches long, mid-way between the umbilicus and pubes, until he had ascertained that it was unconnected with the uterus when this was prolonged above the umbilicus, and the tumor, which was found connected to the broad ligament and posteriorly to the omentum, removed without much difficulty. The operation was completed within twenty-five minutes, the

* Med.-Chir. Review, N. S., Vol. I., pp. 26 and 42.

patient being repeatedly faint, although little blood was lost. The woman lived until the sixth day after the operation, sinking apparently from want of constitutional power. The post-mortem examination exhibited slight traces of recent peritonitis; and the pedicle of the tumor, which had been encircled with the vessels in the ligature, was in a state of complete sphacelus. "The risk from the operation would, no doubt," Mr. Southam observes, "be much diminished if the vessels could be alone secured, which I believe is practicable in a majority of cases, as there are seldom more than three that require tying."

The question of the *malignancy* of ovarian tumours forms an important element in considering the propriety of their removal. It can be best decided by watching and recording the condition of patients who have undergone the operation of extirpation; for we quite agree with Mr. Southam, that the microscope is at present a very unsafe guide in this matter. Of this opinion are several of our most eminent microscopic observers, among whom is Mr. Goodsir, who, reporting to the author upon a specimen of the tumour submitted to him, says:

"I put very little value on the microscope in detecting the nature of tumours of any kind. It is by what we call in natural history the *habit*, or *general appearance*, or *bearing*, of *plant*, *animal*, or *morbid growth*, that the *species* or *nature* in doubtful cases, is to be determined. In disease this tact in discrimination is to be acquired not by poring through a microscope, but by experience in the *diagnosis* and *handling* of tumours in the living, and the examination, by the naked eye, of their general appearance in the dead. The microscope can only *verify the previous determination*." P. 103.

We cannot admit that this grave operation was justifiably performed upon a woman of enfeebled frame, the subject of ascites; but Mr. Southam's narration of its unsuccessful issue is entitled to much praise

III. Observations on the Pathology of Abscess of the Heart. By *T. H. Stallard*, Esq.

Mr. Stallard details an interesting case of this. A shoemaker, æt. 60, and previously very well, was seized, 22nd March, while at work, with coma, cyanosis, and great prostration—his pulse (60) being full, soft, and feeble, and the respiration slow and gentle. Stimuli were given him, and next day he rallied somewhat, the pulse becoming stronger, and the respiration a little quicker. On the 25th he was thought to be asleep, but was found to be dead.

"On opening the left ventricle, which was accomplished by a V-shaped incision, an abscess was observed. It was situated at the apex of the ventricle, and of a very irregular shape, being most pointed towards the apex of the heart, from the surface of which it was separated by two or three lines of healthy structure; above, it projected considerably into the cavity of the ventricle, with which it communicated by a small fissure; the interposed septum was about one line thick, and appeared to consist of thickened endocardium. The cavity of the abscess contained a bloody, purulent-looking fluid; its lining membrane was of a light red tint, and presented a granular appearance. Around the abscess the muscular tissue was darker than natural, and in the outer wall of the same ventricle were observed several fissures, containing a dark-coloured fibrinous mate-

rial, and some of these, though not all, communicated with the cavity of the ventricle. The coronary arteries were much ossified." P. 107.

Mr. Stallard argues the improbability of this abscess having arisen from acute or chronic carditis, since all symptoms of this were absent, and no signs of the always accompanying endocarditis or pericarditis were found after death. The dark fibrinous matter found in the substance of the ventricle had all the appearance of coagulated blood. The presence of blood in this situation may be explained, 1, by the fluid being forced between the *columnæ carneæ*, so as slightly to rupture the endocardium. 2. Congestion of the coronary vessels may terminate in rupture. 3. A partial rupture of the heart's muscular fibres may occur—and this the author regards *as a frequent cause of angina pectoris*. Blood effused from any of these causes may be slowly re-absorbed, remain for a period unchanged; or, if in sufficient quantity to interfere with the action of the organ, the constitutional powers being at the same time feeble, it may undergo the changes so ably pointed out by Mr. Gulliver,* and become softened into a puroid fluid, which, however, resembling pus in appearance, is easily to be distinguished from it by the aid of chemistry and the microscope.

"I have thus endeavoured to show that the case I have related is one dependent upon the effusion of blood into the substance of the heart, that the abscess resulted from softening of the coagulum so effused, and that this having possibly existed for some considerable time, at length escaped into the ventricular cavity, and caused slow death by poisoning the circulating fluid. I shall now proceed to confirm this opinion by one or two recorded cases, and whilst my observations will tend to show that carditis, or myo-carditis, is more rare than is generally supposed, I by no means assert that it never takes place. Andral cites several cases to show that redness of the muscular substance, observed after death, had been caused by an attack of carditis, and this is also the opinion of Dr. Elliotson; but it is remarkable that, in all these cases, severe symptoms of cardiac disease preceded death, whereas none, or few, were present in cases specially related as instances of pure carditis." P. 111.

An abridged account is given of two cases of abscess supposed to arise from carditis, related by Mr. Salter in the *Medico-Chirurgical Transactions*† (Vol. 22), and by Mr. Chance in a recent number of the *Lancet*.‡ Mr. Stallard believes the symptoms and post-mortem appearances in these are best explained by the above hypothesis; and observes that the point is an important one, as a correct plan of treatment can only be founded upon a proper comprehension of the true pathology of the disease. In the one case antiphlogistic means would be called for, even though the symptoms indicated prostration, while in adopting Mr. Stallard's views, stimuli are indicated upon the following grounds:—

"1. To stimulate the vital energies, in order to prevent the effused blood becoming softened and decomposed, and thus to favour its absorption or organization. 2. If the clot shall have already softened and entered the blood, to obviate or at least stave off the effects of poisoned blood, which alcohol has a great power of effecting. 3. Should the fluid have escaped into the pericardium, to stimulate the system to the production of pericarditis; which, however undesirable under ordinary circumstances, is in this instance curative, since it closes the opening

---

* See Med.-Chir Transac. Vol. 22, p. 151, or Med.-Chir. Rev. No. 63, p. 47.
† See Med.-Chir. Rev. No. 63, p. 51. ‡ No. 1185.

of the cavity, and strengthens the parietes of the heart at a point peculiarly requiring support." P. 116.

Mr. Stallard has, in these observations, drawn attention to a very interesting subject, well worthy of the renewed attention of pathologists.*

VI. An Essay, Literary and Practical, on Inversio-Uteri. Part 2. By *John Green Crosse.*

This is a continuation of the elaborate monograph we have already given some account of,† and is marked by the same laborious research which characterized its predecessor. It is, however, more interesting and original than that, and will be perused with advantage. Mr. Crosse describes several stages or degrees of inversion, viz., simple *depression* of a portion of the fundus, which may give rise to a severe or even fatal hæmorrhage. It soon passes, however, into the state of *introversion*, in which the fundus and a portion of the body of the organ is "received into the remainder of the body and cervix, the convexity of the fundus being palpable at the os tincæ." *Perversion* is said to exist when more or less of the inverted portion passes through the os tincæ. In *total inversion* the cervix, as well as the rest of the uterus, is inverted. The former part of the Essay comprised a description of the varieties of the disease and their distinctive symptoms, and the present one commences with an account of the author's opinion of the alleged

"*Infrequency of Inversion.*"—According to statistical evidence, this disease is of very rare occurrence. Thus, of Mauriceau's 850 cases selected for publication, only six were examples of this. Dr. Collins met with no example in 16,654 deliveries at the Dublin Lying-in Hospital, while Dr. M'Clintock, speaking of that hospital, states that no case was met with amidst *seventy-one thousand* deliveries. A lying-in hospital is not, indeed, the place we ought to expect to meet with an occurrence which generally arises from mismanagement. However this may be, the disease is by no means so rare as the above statement would show, many authors, whose works are cited by Mr. Crosse, relating several cases. Altogether he has been enabled to collect 400 cases, of which 350 occurred after parturition, 40 from polypus, and 10 from various other causes. The belief in the great rarity of the disease is calculated to favour the overlooking of its minor degrees. These have been observed by several of Mr. Crosse's medical friends since he has so particularly directed attention to the subject; but he believes that uterine hæmorrhage still frequently occurs without partial inversion being suspected as its cause. "No other case," he observes, "has, in relation to the infrequency of its occurrence, been so often brought into a court of justice as *inversio uteri*;" and in truth the instances, some of which of recent occurrence he cites, in which not merely ignorant midwives, but practitioners supposed to be qualified, have mistaken, mutilated, or removed this organ, are frightfully numerous.

* See Med.-Chir. Rev., No, 35, p. 174, and N. S., No. 9, p. 40.
† Med.-Chir. Rev., N. S., Vol. 2, p. 467.

The perpetrators of such outrages, who have sometimes been screened by well-meaning medical men, should be placed beyond the pale of professional sympathy, as, although our law courts seem little competent to deal with such delinquents as they deserve, the exposure of their misdeeds, and the consequent loss of reputation this entails, is a debt we owe to the public; for although we should be careful not to render patent the errors our brother-practitioners may fall into in the exercise of a fallible judgment, we should refuse to place in this category those results of ignorance so mischievous and glaring as to render the pursuit of the profession by their authors a matter dangerous to society. There is a limit beyond which our duties as members of society are paramount to all considerations of medical etiquette.

Passing over the sections upon the symptoms and pathology of the disease, we come to that which treats of—

The Causes and Mechanism of Inversion.—Causes of a contradictory and insufficient character have been assigned as competent to the production of Inversion. *Predisposing Causes* may exist long prior to the occurrence of the accident, and among these may be classed whatever tends to enfeeble the system, inversion, in the majority of instances, occurring in women of delicate frame and relaxed fibre, with such a loose state of the pelvic soft parts as usually yields to a quick or precipitous delivery. The erect posture is favourable to its production, many instances having occurred in which the woman was suddenly delivered before she could assume a recumbent one. *Over-distension* of the uterus, as by the *liquor amnii*, has been supposed to dispose the organ to this accident. Mr. Crosse's investigations do not corroborate this view; for, in such case, we should expect the accident to more frequently follow the birth of twins, while of the 400 cases he has collected, only four were examples of this. Moreover the accident is of far more frequent occurrence in the young primipara than in women who have already borne children. He believes that some of the worst cases have depended upon great *thinness* or defective density of the uterine parietes; but he regards a derangement of the normal vital action of the organ, rather than its different mechanical properties, as the usual predisposing cause—a *partial inertia* or *a want of contraction* inducing the *depression* or first stage of the displacement.

Relaxation of the round ligaments has been classed among the predisposing causes, and they are at all events passive and incapable of affording resistance. The *previous occurrence* of the *disease* seems to have taken place in very few cases. *Hæmorrhage*, generally dependent as it is upon partial inertia of the uterus, may act as a predisposing cause by increasing that inertia; but, if the blood has accumulated in the cavity, its sudden escape may be attended with inversion. The *attachment of the placenta to some part of the fundus uteri* seems to be almost an essential condition for the production of the disease, and may be regarded as forming a connecting link between the predisposing and determining causes. But neither this nor any other of the predisposing causes have much effect, unless several of them be conjoined.

The *immediate or determining causes* of inversion may be classed under three heads. "1, Such as are situated in the uterine tissue itself; 2,

such as act upon the outer surface of the organ, pressing or driving it down; and 3, such as act upon its internal surface, drawing or pulling it down." Though not competent to *commence the displacement causing depression*, the uninverted portion of the *uterus* will act upon the portion already incipiently inverted from any cause, as upon an extraneous mass, and complete the inversion.

The *expulsive abdominal nisus* of the patient may much aid in the production of depression, and afterwards asist in every stage until complete perversion; and it is indeed sometimes the most active power in the production of the worst cases of rapid inversion. In many recorded cases we find the inversion to have been latent or unsuspected for hours, or a day or two, until some expiratory effort has increased it to perversion and prolapse. In the third class of causes, *violent traction through the cord* has often induced the accident; but in an enfeebled constitution, and when the predisposing causes are numerous or considerable, the slightest traction by a careful practitioner may suffice to induce the first degree of the affection. The weight and even the bulk of the placenta, by distending the lower part of the uterus and exciting its expulsive action, may thus contribute to the completion of the inversion. A *short funis*, or one that is coiled around the body of the child, may render depression unavoidable when the placenta is attached to the fundus. The *too rapid removal of the body of the fœtus* from the uterus, after the birth of the head, will, "upon the principle of a tendency to a vacuum (?), draw down the relaxed fundus." *Morbid adhesion of the placenta* by the encouragement it affords to traction at the cord, may also cause inversion. Not only must the predisposing and efficient causes unite for the production of this displacement, but there is usually a plurality of each contributing to this.

Mr. Crosse has an excellent chapter upon the *Diagnosis* of the disease and treats it more minutely than any other writer we are acquainted with. We have only room for one or two of his general remarks.

"In no instance has the importance of a correct diagnosis been more strikingly illustrated than in uterine inversion, which has been mistaken more often, in proportion to its frequency, than any other malady of its class, and, perhaps of *any class*. These mistakes have usually been attended with the most fatal effects, for many are the examples, attested by the best authorities, where it is observed that, had the nature of the case been timely known, it would have been rightly treated, and life easily saved.

"In commencing this section by considering recent inversion, I find reason to remark that errors in its diagnosis have frequently been but hasty and inconsiderate impressions, which a moment's reflection would remove; or they have arisen from the want of previous experience, and the absence of all suspicion, owing to the real or supposed rarity of the displacement. * * * * The short and dogmatical rules of some of our most esteemed writers, would indicate the diagnosis to be very easy; the authentic records of our art seem to show the contrary, even as regards recent inversion post partum, which in its earlier stages has been mistaken for the head of another fœtus, another placenta, a mole, an excrescence, a polypus, a tumour, and a clot of blood; and, in its more advanced stages, for not only some of these, but for still more unaccountable diseases and displacements. It is very difficult to explain the source of all these erroneous impressions, which not only midwives, but even well-educated and practiced surgeons have acknowledged, until we consider that the emergency is great and sudden,—some opinion or other must be rapidly formed,

and those ideas with which the mind is more familiar first present themselves. The pressing urgency of the circumstances is even greater than the surgeon estimates them to be, for uterine inversion, at the close of the delivery, is a case that leaves little time for consideration, none for preparation, ere a decision must be arrived at, or the risk be incurred of the patient's expiring of her undetected, or misunderstood, and consequently maltreated, disorder." P. 317.

Prognosis.—No disease of its class is more fatal than this, unless effective aid be promptly rendered. The mortality from it has been variously estimated; but Mr. Crosse, from the most extended investigation yet brought to bear upon the subject, is in a condition to affirm that "above one-third of all the cases, under whatever circumstances, or in whatever degree they occur, prove fatal very soon, or within one month after the displacement is produced." Of 109 fatal cases collected from various sources, in 72 death took place in a few hours, generally within an hour and a half, and often nearly immediately. Eight cases proved fatal in from one to seven days, and six in from one to four weeks. "It deserves to be forcibly noted that, although 86 cases were thus brought to a close within *a month*, this period being passed, only one of the remaining 23 proved fatal before arriving at the chronic stage," the remaining 22 terminated at different dates extending to three or four years, and in a rare case or two, to 5 or even 20 years. The less the degree of inversion when we are called to the case, and the slower it has advanced, the smaller the amount of danger. The organ may perhaps be so relaxed as to admit of ready reduction, though entirely inverted; but this may arise from the patient being already *in articulo*. A rapid spontaneous inversion may be more dangerous than one inverted by traction of the cord, as the patient in the latter case may possess a strong constitution, enabling the disease to go on to its chronic stage. In the stage of depression the consequent hæmorrhage, if the displacement be not rectified, may alone kill the patient, but when the organ is extruded, we have the influence of "shock" superadded to this. Mr Crosse believes that total inversion can only be rectified by almost immediate attempts, but the patient has a better chance of recovery by this remaining unattempted than from its accomplishment through violent means.

We regret that our limited space has prevented our furnishing a more complete account of Mr. Crosse's interesting Essay, and hope that he will not long delay the publication of the concluding portion, embracing the treatment of this formidable accident and its consequences. The same reason likewise precludes our noticing a paper of Mr. Barker, describing the Removal of a *Large Secondary Prostatic Calculus* by Perineal Incision; the *Retrospective Address* of Dr. Ranking; and a short Essay by Dr. Paxton upon "*Pathological Memorials*," in which he ably advocates the desirableness of making pathological drawing part of medical studies.

Upon the whole the volume is an excellent one, and well illustrated by numerous lithographs.

I. The Construction and Government of Lunatic Asylums and Hospitals for the Insane. By *John Conolly*, M. D. 12mo. pp. 183. Churchill, 1847.

II. Journal of Insanity, Vol. III. Utica, U. S. 1846.

III. Third Annual Report of the Managers of the State Lunatic Asylum. By *Amariah Brigham*, M. D. Albany, U. S. 1846.

IV. Twenty-Second Report of the Officers of the Retreat for the Insane at Hartford, Conn., U. S. 1846.

V. Report of the Medical Officers of the Lunatic Asylum for the County of Lancaster. 1846.

Although we have recently* treated somewhat at large upon the subject of Insanity, yet so important is the topic, that we are glad to avail ourselves of every additional opportunity of adverting to it, whether to chronicle progress or to indicate the obstacles to this. All that we have seen or read since they were written but confirms the correctness of the opinion we advanced in the articles referred to, that the government of Lunatic Asylums in this country is founded upon an essentially faulty basis, and that, until this error is remedied, the management of the Insane will not continue to be such as is most conducive to their interests. At present this is a matter of the merest chance, dependent upon the amount of intelligence, good sense and forbearance possessed by so miscellaneous a class of persons, as far as mental attributes and special information are concerned, as the visiting justices and governors of these establishments. From Dr. Conolly's work we incidentally learn the mischievous powers possessed and employed by them at Hanwell, and certain it is that it required an amazing union of humanity, patience, discretion, and firmness upon the part of this philanthropist to carry his great experiment to a successful issue. Few men are equally endowed, and thus we see, as a natural effect of the interference and irritating annoyances, they are subject to a constant change of the resident medical officers in our various asylums. So peculiar is the condition of the lunatic, that we do not hesitate to repeat our opinion, that his real well-being, whether rich or poor, can only be provided for effectually by his becoming an object of direct state-interference or management: but, supposing that the prejudices against any such centralizing procedure would prevent its adoption, we may at least imitate the Americans, who have the good sense to leave the entire management of their asylums, at least of all that relates to the patients, exclusively in the hands of a well-selected and well-paid resident medical superintendent. Until an officer of this kind is allowed absolute control over it, no establishment can be considered in a satisfactory condition.

* Vide, Med.-Chir. Rev. N. S., Vol. IV. pp. 56 and 394.

He is the central point around which all must hinge and turn. Take every imaginable pains in his selection, but having chosen him, remunerate him handsomely, and clothe him with the necessarily despotic authority. The present humiliating position of most of our resident officers, as contrasted with that of their brethren on the other side of the Atlantic, is as distressing as it is mischievous.

Dr. Conolly's little work consists chiefly in a reprint of some Lectures which appeared in the Lancet last year, as supplementary to his valuable clinical course which we had hoped would also have been re-published. It is full of matter of the highest importance, especially at this epoch, when so many new establishments for the reception of the insane are upon the eve of erection. We may notice some of the chief topics he adverts to; but we may here observe, that we do not participate in the fears which he entertains that the projected separation of the chronic from the more recent cases will be attended with the diminution of the comforts or attentions at present enjoyed by the former; at all events, we are certain that it need not be so. While we acknowledge that each county is bound to provide suitable establishments for all its lunatics, it seems only reasonable that such provision should be made with all the view to curability and economy that the comfort of the patients admits of.

Site and Construction of Asylums.—Dr. Conolly properly insists that, any plan for the construction or alteration of a lunatic asylum, should be submitted to the consideration of a physician well acquainted with the characters, habits, and wants of the insane prior to its final adoption, he frequently being best able to point out the defects, or to suggest improvements. Heretofore, asylums had too much the appearance of prisons, and in point of fact, both descriptions of edifice were constructed for merely the safe detention of their inmates. A great improvement in this respect has taken place in the case of recently-erected asylums, and we hope ere long none of the establishments so destined will retain the repulsive and forbidding appearance which some yet have. Dr. Conolly lays much stress upon a gentle eminence being chosen for the site; and all those who have seen the beautiful situation of the Lincoln Asylum, and are aware of the generally healthy state of its inmates and the happiness which the extended prospect imparts to them, will be disposed to agree with him. He thinks the building should not be constructed for more than 360 or 400 patients, great care being taken to enclose sufficient space within the walls to furnish ample gardens, airing courts, and to admit of the due classification and employment of the insane, and of the enlargement of the building, if necessary, without violating the original plan. In all asylums the proportion of separate sleeping-rooms is too small, and Dr. Conolly always recommends that at least two-thirds of the patients should be so accommodated. With large overcrowded dormitories a sweet state of the air cannot be maintained. Whatever system of ventilation and warming be adopted, Dr. Conolly lays great stress upon the necessity of having windows large enough to admit an abundant supply of fresh air in temperate weather, and open fire-places, with all their cheering associations in winter. He insists, too, upon having the galleries well lighted, for not only is it an error to suppose that the insane are benefited by being kept

in a state of darkness or semi-darkness; but they suffer grievously when condemned to this from motives of economy. "In visiting patients in private houses," he observes, "I generally find the rooms made totally dark, but filled with anxious relatives, attendants, and half the servants of the house. All these things are unfriendly to the patient's tranquillity, and produce suspicion, fear, and increased violence." The maintenance of the most perfect cleanlinesss, both of the persons and the chambers of the insane, seems so obviously of the first importance, that one is surprised to find it is one of the essentials only quite recently and not yet universally recognized. It is one, indeed, impossible to put into force by other means than a well-organized superintendence.

Exercise and Recreations.—We sincerely hope that, in any new establishment that may be erected, ample provision will be made for out-door exercise, in which most of our public asylums, and nearly all the private ones, are terribly deficient. The sight of hundreds of restless lunatics cooped up within a miserable space of ground, encompassed by high walls, is one of the most mournful of spectacles, and the wonder is that reason is ever restored under such discouraging circumstances. Even in our best asylums, the curative, ameliorating, and composing effects of exercise are not developed, by reason of the rules which prevent the patients passing beyond the walls. Large, indeed, must be the space enclosed if traversing it again and again for months and years do not prove insupportably monotonous. In America, all the patients who can be depended upon are permitted, with the best effects, to take walks and excursions into the adjoining country, and why the same practice should not be followed here it would be difficult to say. The various out-door amusements should be encouraged, as far more useful than those of a more sedentary character, and as acceptable to the attendants, who stand as much in need of relaxation, as well as to the patients. Within-doors, the means of amusement are now freely resorted to in most asylums; and, indeed, since attention has been drawn to the necessity of providing a variety of such as a substitute for restraint, some have been introduced into some of the foreign asylums, such as dramatic representations, debating societies, &c., of questionable utility. Upon the defective condition of the majority of private asylums in regard to exercise, Dr. Conolly observes as follows:—

"It is astonishing to find that one of the particulars of treatment in which the rich patient is sometimes more unfavourably situated than the poor, is that of having an opportunity of enjoying free and frequent exercise. The untrodden lawn, the dusty and desolate courts, the paved yards, the wretched sheds, the lonely out-houses, together with the closed doors and windows of a private asylum, often give to such places an external character, which makes the visitor regard them with dread, and the passer-by speak of them in whispers. The idea of their entrance is connected with that of the fatal gate, which whoso entered left all hope behind. If a patient is visited in such a house, the unlocking of doors, the threading of passages, and the ascent of gloomy stairs, with the close atmosphere of apartments, filled with patients sitting by the walls, oppressed with indolence and monotony, are all features too familiar to those who knew such houses before better improvements penetrated into them. And it is only in a few of the best private asylums that we even now find cheerful sitting-rooms, opening into gardens, into which patients may walk when they please; and the

benefit of this improvement is yet too generally limited to the most rational of the patients, and not extended to the irritable and troublesome, who ought also to be able to go out of their sitting-rooms, although into gardens more secluded and secure." P. 59.

Dr. Conolly believes that attention to the *dress* of patients is too much neglected, and all those who have seen the humiliated mien of some lunatics when presented to friends or strangers in unsuitable attire, will allow that this may act very detrimentally upon them.

"Many private asylums are open to the charge of great neglect as respects the dress of the classes far above pauperism. Tattered and thread-bare coats, very shabby hats, trowsers not always free from an offensive smell; and equally slovenly dresses on the female side of the asylum, shoes out of repair, hair in curl-papers, make the unfortunate patients objects of pity or of ridicule. They feel themselves degraded, lose their self-respect, and with it the little self-control their malady has left them. It is very true that, in some cases the patients will not dress themselves properly, that they have an affection for old and ragged garments, insist upon their being fit to go to court in, and are evidently offended if better clothes be substituted for them; but such cases only form a small proportion in any asylum; and, in many instances, habits of personal neatness may be long preserved, and in some restored, after being long lost." P. 63.

Employment.—The value of this can scarcely be over-estimated, when discriminately furnished. "We consider labour," says Dr. Brigham in his Report, "as among the most essential of our curative means: of this we become more convinced every year." It should, however, never be regarded apart from the other portions of the treatment of the patient, and its amount therefore regulated by the opinion of the physician, not by its economical results, important though these be in large establishments. Out-of-doors employment is far more conducive to health and recovery than that of a sedentary character. Regular work should never be attempted to be enforced from an unwilling patient upon the plea of idleness. According to the last Hanwell Report, 219 out of 418 male patients were employed, and 314 out of 567 women. All the patients' clothing, except shoes and bedding, is made up in the asylum, and all the washing and mending for so large a population performed. There are a garden and farm, on which labour is performed, carpenter's, tailor's, shoemaker's tinman's, &c. shops, the expense of employing instructors being an obstacle to the multiplication of occupations. Dr. Brigham complains also of this, and that in a much smaller establishment than Hanwell.

"In winter, we find it difficult to furnish suitable and sufficient labour for all who would be benefited by it. Some engage in sawing wood, but this furnishes but little labour for 60 or 70 men. Some work in the joiner's shop, and a few are employed in other kinds of business. Still, for a considerable number we have not sufficient employment, and we apprehend this is a difficulty which all large asylums have to encounter. We have studiously examined this subject, and reflected much upon the propriety of establishing some kind of business, in which many can engage. But we find it difficult to determine what kind is best. Hitherto, we have found none better than *carving toys* and making small wooden articles. Several of our patients have become very skilful at this business. Some articles carved here are not inferior to the handsomest Swiss specimens, of which in fact ours are imitations." P. 33.

The difficulty becomes greater still in devising occupation for male patients in the upper walks of life.

Attendants.—Upon no one circumstance does the well-doing of an asylum so essentially depend as upon the possession of an efficient corps of attendants. The securing this is of the first consequence then, for without it no ameliorations can be carried out to the extent they are capable of, and no improvement can be prevented from eventual degeneration. Two chapters are therefore devoted by Dr. Conolly to this important subject, in which he points out the defects of the present modes of appointing and governing these functionaries, and suggests some of the numerous alterations these are capable of, and urgently demand. It is so evident that these persons should be the faithful and passive instruments in the hands of the physician for carrying out his designs, that it is scarcely to be believed that they are chosen without reference to his wishes or their own qualifications, called upon to perform duties of a nature and at a time that have not his sanction, and dismissed or changed with as little consideration of his feelings upon the subject or the true welfare of the patients as prevailed in their appointment! We are constantly hearing complaints of the unfit class of persons who follow the occupation of keepers or attendants of the insane, and can it be wondered at that such should be the case, when our large asylums set so vicious an example as this. The resident alone can appreciate their qualifications and their efficiency; to him alone should they owe their introduction to the establishment and their continuance in it; or at the very least he should possess a *veto* upon the one and the other. Nothing is more admirable than the spirit of benevolence which pervades the whole of Dr. Conolly's work, and this is not exhausted upon the insane themselves; for, while he demands that steps be taken to provide them with more efficient attendants, he likewise insists that these ought to be more considerately treated in regard to their distribution of employment, relaxation and recreation, instruction, &c. With the present hard work and uncertain tenure of an asylum-assistant, no person will longer engage in the occupation than he can help, those best qualified for it indeed being the first to quit it, leaving the staff overburdened with the unwilling and the incapable. The duties to be performed at Hanwell, here described in full detail, are indeed multifarious and onerous, and make so large a call upon the benevolence, the mental energy, and the bodily activity of the attendants, that we are at a loss to understand how they can be efficiently executed by persons so miscellaneously chosen and so inadequately encouraged. Dr. Brigham says that, in his asylum, great advantage has been derived from a more judicious distribution of duties among the attendants, so as occasionally to exempt them from duties which, uninterruptedly continued for many hours, must produce a tension of mind utterly destructive to the retention of its elasticity and detrimental to the preservation of the temper. These supplementary attendants are derived from a class of persons sufficiently instructed to develop the amount of mental occupation the insane are capable of and benefited by.

"Our observation for many years in various lunatic asylums, led us a long time since to regard the want of *mental occupation* as the greatest want in modern

institutions for the insane. Go into any such establishment, and you will find some few, in winter a very few, at work, some playing cards or other games; yet a still larger class will be found sitting about, listless, inactive, doing nothing, saying nothing, taking no interest in anything going on around them, gathering round the stove, looking forward to nothing but the hours for eating and retiring to sleep. For a short time each day, when the physician passes round, they will exhibit a little animation and say a few words, and then relapse into their former condition.

"These patients give but little trouble in an asylum, and are very apt to be overlooked and neglected, and, if not already demented, soon become so. They are thought not to require much attention, as they have good bodily health, and are quiet, consequently they generally receive but little notice. *But this class require great attention.* They need *mental exercise*; they should attend school, and have their minds aroused into mental activity for an hour or two every day. Soon, by this course, their memories will improve, they will become interested in singing or study, and by perseverance some will be cured, and many, very many, rendered capable of much enjoyment, and be kept from sinking into a state of hopeless dementia.

"Our teachers spend all their time with the patients, but have no labour nor any other duty to attend to, than to interest the patients, and contribute all they can by their presence and conversation to their contentment and enjoyment. Thus they join in their amusements and walks, and are their constant companions. The relief which they afford the attendants is very great, and enables us to dispense with somethat would be otherwise necessary. We are satisfied that an establishment like this can be better managed, and with equal economy, by having an arrangement by which some should dovote their time to the ordinary duties and labours of the halls, while others should have nothing to do but to accompany the patients and endeavour to instruct and amuse them. The latter having nothing to do with any coercive measures, the patients do not become prejudiced against them, and will readily hearken to their suggestions. Thus they serve as a constant guard, and by their presence and management prevent outbreaks and disorder, and make coercive measures, restraint, and seclusion rarely necessary.—*Report*, p. 37.

There seems sound wisdom in this arrangement, both as regards the welfare of the insane and the comfort of the attendants; and it would be still more applicable to the pauper lunatics of England than to those of America, inasmuch as a much smaller proportion of the former are able to read, and thus amuse themselves by the perusal of light reading, newspapers, &c., with which, too, our asylums are far more scantily provided. But the lack of well-informed attendants is most felt by the wealthy and educated lunatic, who finds himself condemned to the perpetual society of one possessed only of the acquirements of a menial, but invested with the authority of a master, and that too often a harsh one. Little matters it that a ray of intelligence may yet lurk amidst the ruins of his mental edifice, no sympathy and encouragement will enkindle it into an invigorating flame; and the mental apathy and listlessness which an utter absence of interesting conversation or other intellectual occupation engenders or augments, may confirm and render hopeless a condition which, more skilfully managed, might have had another issue. When it is considered how many persons there are of both sexes, whose mental acquirements and moral attributes would well fit them for the occupation of attending upon the insane, and the excellent remuneration the wealthy portion of so many of these unfortunates would enable them to furnish, it

is evident that causes must exist for preventing persons who might be thus so mutually serviceable coming oftener into contact. The leading one is doubtless that indicated by Dr. Conolly, namely, the unremitting attendance usually required at the hands of the attendant, no provision whatever being made for relaxation or temporary cessation of duties. Few minds possessed of the sensibility which constitutes their most valuable property, could long withstand the harassing wear and tear incident upon their incessant occupation, or alternated with intervals of mere listlessness. Occasional breaking up of the routine of duties by the enjoyment of the society of the sane, healthful recreation, or change of occupation, is essential if the mind is to be maintained in its pristine vigour. It is evident that this cannot be accomplished as long as one individual alone has the charge of the insane patient; and we think it highly detrimental to the interests and comforts of the one and the other that he should ever be permitted to have this. Moreover, a person qualified as we have supposed would properly decline performing many menial offices now expected at the hands of the attendant. He should, therefore, always be provided with a subordinate for such purposes. This would entail an expense, not to be defrayed by other than the very wealthy, and would render the maintenance of the patient otherwise than in an asylum generally impossible. And we are convinced it is always undesirable he should be otherwise placed than under the roof of such an establishment; but we believe that the directors of this would find their account in employing a higher class of attendants, remunerating them better, and adjusting their duties more judiciously.

Religious Services.—Dr. Conolly, Dr. Brigham, and others of the most enlightened medical officers have frequently borne testimony to the value of these when subordinated to the other portions of the general plan, pursued with temperance and caution, and only to the extent sanctioned by the physician. Indeed, the good results which have attended their performance have been far greater *à priori* than might have been expected, seeing that so large a portion of the patients in the asylums of this country, and especially in America, have been originally rendered insane by perverted religious tenets and fanaticism. Still, everything depends upon the prudence with which this instrument of improvement is wielded; for, if it be placed in the hands of a rash intermeddling or conceited man, great evil will result. We quote some passages from a just appreciation of a Chaplain's various duties from the pen of Rev. T. Gallaudet, Chaplain of the Retreat at Hartford, U. S.

"So long as the insane have any exercise of their reasoning faculties left, and any moral and religious susceptibilities to be appealed to, (and no inconsiderable portion of them retain more or less of these faculties and susceptibilities, and some of them in a striking degree), so long Divine Truth, with its higher motives and consolations, will be found eminently adapted to the exigencies of their unfortunate condition, and one of the most salutary and efficacious means of cure. To what extent the influence of this Truth can be beneficially employed, time and a careful experience will show. Its effects should be critically noticed, and compared at different institutions.

"In exhibiting these views, however, it ought to be stated that the Chaplain by no means regards it the part of a wise performance of his duty, in his per-

sonal intercourse with the patients, to confine himself to conversation on religious topics merely. He endeavours with the general counsels, and often with the specific suggestions of the physician, to adapt the mode of this intercourse to the peculiar exigencies of the case. He appears among the inmates as their sympathizing friend. He exchanges with them the customary civilities of social life. He listens to their conversation, and lets them see that he is interested in it. He often introduces other than grave and serious subjects, adapted to afford rational instruction or innocent enjoyment; nor can he discover that, in doing this, he is exposed to any disparagement of the proper dignity of his office, by the want of courtesy or respect of those whom he seeks to benefit. It is, indeed, by pursuing such a course, that he hopes to avail himself of the suitable opportunities when they offer, and they not unfrequently do offer, of presenting, in the most favourable manner, the simple and consoling truths of the Gospel. * * * * * * * *

"The good order, too, of such a numerous household, including the officers of the Institution, and others who are engaged in the management of its internal affairs; the conscientiousness, faithfulness, and kindness with which their various duties should be discharged; and the diffusion throughout the whole establishment of that spirit of self-denying benevolence which the Gospel teaches and inspires, are best promoted by constantly bringing before their attention, and commending to their cordial acceptance, as the rules for their conduct, the principles, the motives, and the encouragements contained in the oracles of Divine Truth."

A clerical officer actuated by these enlarged views must be an invaluable coadjutor to the physician, and is well deserving of the encomium passed upon him by Dr. Butler.

Instruction.—We can sympathize with the regret with which Dr. Conolly views the ill-judged suppression of the schools at Hanwell. Wherever they have been established in this country, in France, or in America, the testimony is universally in their favour as powerful means for ameliorating the condition of the patients, From the last Lancaster Report we extract the following:

"Exclusive of day-schools for the idiotic, an evening class for reading, writing, and arithmetic has been established in each ward, under the superintendence of the matron and chief-attendant, with such assistance as the ordinary attendant can bestow, aided in many cases by the better educated portion of the patients. A great number of the inmates take a lively interest in the proceedings, and in many, a marked improvement is observable. The day-schools are conducted on somewhat the same principle as that adopted in infant schools, and it is most gratifying to observe the favourable impression produced even on the idiotic mind by well-directed and persevering efforts, where, to the casual observer, all prospect of educational benefit, would appear to be utterly hopeless."

Dr. Brigham, whose establishment possesses the great advantage of having attendants specially devoted to the instruction and amusement of the patients, says of the schools:

"Our confidence in their utility has been increased by experience and observation. Many cases, we believe, cannot be improved, but by arousing and calling into exercise the dormant faculties of the mind. Hence we have found our schools particularly beneficial to the demented and those approaching that condition. In such, the active state of the disease, which originated the mental disturbance, has passed, and left the brain and faculties of the mind in a torpid state. In these cases, medicine is in general of no use, and as we have said, they cannot often be much improved, but by exercising the faculties of the mind.

But others are also benefited by devoting a portion of every day to mental improvement. To those who are nearly or quite well, and who remain with us for fear of relapsing at home, or for other reasons, our schools afford enjoyment, and often means of improvement, which they highly value. Those that are uneasy and nervous, that are constantly restless and disposed to find fault and to annoy the attendants, and quarrel with all about them, because they have nothing else to occupy their minds, are also much benefited by the exercises of a school. We are every day surprised at the good effect they have upon this class of patients." P. 35.

Dr. Conolly concludes his work with some most excellent observations upon the proper officering of asylums; and ably illustrates the evils of the present plan of subdividing authority among the physician, matron, and governors. His advice, based upon long experience of suffering under the inconveniences he exposes, calls for the serious attention of all those who think that future attempts at ameliorating the condition of the insane should not be liable to risk of failure from intermeddling of well-meaning but ignorant lay-men, though these be "justices of the quorum."

Appended to Dr. Brigham's Report are some observations upon the *Prevention* of *Insanity*, He truly enough says that, if the predisposing causes were more sedulously avoided, that attacks and relapses of this dreadful disease would be far less frequent; and he seems to think that the public only need advising upon the liability of its being hereditarily transmitted or induced by faulty education and habits of life detrimental to health. Were this all, certainly to no one could we point as so capable of giving such advice as the learned author of the Influence of Mental Cultivation in inducing Insanity: but, alas! those who have to do with the realities of this bustling world, too soon become acquainted with the invincible carelessness or indifference of the public in respect to all hygienic precautions, and the too frequent impossibility or nullity of such amidst the increasing turmoils, struggles, and reverses of the present state of society. That insanity is upon the increase in this country, in France, in Germany and the United States, can excite no surprise in the mind of those who are attentive spectators of what is passing around them.

With Dr. Brigham's opinion upon one other point touched upon by him we entirely agree, namely, that *medical men in general practice do wrong to neglect the study of insanity*, leaving it thus entirely in the hands of a special class of practitioners, and even rendering themselves incompetent to decide upon when the assistance of these can be most beneficially sought for. The earlier symptoms, are for this reason often overlooked or mistaken, and invaluable time lost, when by some overt act the patient makes patent to all the world that which the eye of science ought long since have provided for. Moreover, if preventive counsels are to be of any avail, they will be so by being specifically adapted to individual cases. You may furnish the public with the best popular treatises upon the subject, but they are unable or unwilling to correctly apply the principles these may contain. Our readers are aware that one of the highest claims Dr. Conolly has to the gratitude of the profession, is his earnest endeavours to diffuse information by his valuable clinical lectures at Hanwell.

We had intended citing one or two papers from the "Journal of Insanity," but have exhausted the space at our disposal.

A Treatise on Fractures in the Vicinity of Joints, and on certain Forms of Accidental and Congenital Dislocations. By *Robert William Smith*, M.D., M.R.I.A., Fellow of the Royal College of Surgeons in Ireland, Lecturer on Surgery at the Richmond Hospital School of Medicine, &c., &c. Octavo, pp. 314. Dublin: Hodges and Smith, 1847.

We know of no class of cases more perplexing to the practitioner than the injuries considered in the work before us. Often we have seen surgeons even of considerable experience at fault, or at a loss to make out the exact nature of the lesion in cases of injury connected with a joint, and though much has been written on the subject, there is no work in which it has been satisfactorily treated of. Boyes, Sir A. Cooper, and Dupuytren have indeed done a good deal in clearing away the obscurities connected with fractures in the vicinity of the articulations, but there is a remarkable discrepancy in their accounts of the symptoms of many of these injuries, which can only be accounted for by unsettled and imperfect views of their pathological characters. We think, therefore, that Mr. Smith, who is known, by his contributions to the Dublin Medical Journal, to have devoted much attention to this important subject, required no apology for submitting his views to the notice of the profession.

Chapter I. is on the Diagnosis and Pathology of Fractures of the Neck of the Femur. After describing the ordinary symptoms of this injury, Mr. Smith notices the difference of opinion which has existed, and indeed still continues, as to the amount of shortening of the limb which occurs in the two varieties of this important lesion. "Upon this question the most eminent surgeons of modern times are directly opposed to one another. Sir Astley Cooper, Amesbury, Chassaignàc, Vidal, (De Cassis,) and others, maintain that the shortening is greater in the intra capsular fracture than in the extra capsular. Upon the other hand, Desault, Boyer, Dupuytren, Cloquet, Earle, Stanley, &c., have stated, that the greatest amount of shortening accompanies the fracture external to the capsule How are such conflicting statements to be reconciled?" In discussing this question our author very properly limits his observations to the retraction of the limb, which *immediately* succeeds the receipt of the injury; for this is the period at which it is most important to form a correct diagnosis. The chief circumstance, according to which the degree of shortening varies in cases of intra capsular fractures, is, the amount of laceration suffered by the fibrous covering of the neck of the bone, which Mr. Smith rather inappropriately terms the "cervical ligament of the femur."

"If the force which acts upon the neck of the femur be inconsiderable, and as it were, exhausted, after producing the fracture, this ligament may escape uninjured, or nearly so. In such a case, the retraction of the limb will be very slight, and will be at its minimum when the fracture has traversed the bone obliquely from the inferior part of the head of the femur downwards and outwards. When the cervical ligament remains nearly entire, it not only limits the retrac-

tion of the limb, but it likewise becomes the sole medium through which a vascular communication is maintained between the fragments; it is, therefore, very important to observe the utmost caution in examining the limb; and if the existence of fracture of the neck of the femur is rendered evident by the presence of the other symptoms which characterize this lesion, we should, I think, abstain from instituting such an examination as is necessary to produce crepitus. In doubtful and obscure cases, this examination should of course be cautiously made, but I perfectly coincide with the opinion of Boyer, that, in the majority of cases of fracture of the neck of the femur within the capsule, the nature of the injury is sufficiently indicated by other symptoms." P. 8.

No doubt, as stated by Boyer, and indicated in a case of Mr. Stanley's' which is quoted by the author, much injury may result from a rough examination of the injured limb, but without either shortening or eversion, which may not be present when the fibrous covering is entire, the nature of the case would not be so clear as to justify the surgeon in dispensing with an examination of the limb.

Among the causes which limit the retraction of the limb in cases of intra capsular fractures, Mr. Smith does not omit to notice the powerful capsular ligament which so closely embraces the articulation. He very properly controverts the statement of Sir A. Cooper, that when the bone is broken within the capsule, the leg becomes from one to two inches shorter than the other, siding on this point with Mr. Earle and Boyer, who never witnessed such an occurrence. But when the fracture is external to the capsule and not impacted, there is but little to prevent the full force of muscular action upon the lower fragment of the bone, while, at the same time, the upper is depressed by the weight of the body, so that, from these two causes, a degree of shortening may be produced equal to, or even greater, than the entire length of the neck of the bone.

"Again, we meet with cases in which the shortening of the limb is not by any means decided or evident for several days after the receipt of the injury: in such instances it usually happens that the muscles have been, to a certain extent, paralyzed by contusion; but according as, under the influence of rest, &c., they regain their power of contracting, the limb becomes slowly and gradually shortened, and this independent of any process of absorption. A third class of cases occur, in which the retraction of the limb, having been at first scarcely perceptible, at the expiration of a few weeks becomes very considerable, owing to the rapid absorption of the neck of the bone. In the case of Margaret Myler the amount of shortening was at first only a quarter of an inch, but at the end of six weeks amounted to one inch and a half. Lastly, examples are sometimes seen in which the limb retains nearly its natural length for many weeks after the receipt of the injury, and then a very decided and, comparatively speaking, considerable degree of shortening occurs, not gradually, but suddenly. In these cases the diagnosis is somewhat obscure; the cause which has produced the fracture is comparatively slight, and the patient has not made any attempt to use the limb after the receipt of the injury; the eversion of the foot is by no means so well marked as when the retraction has occurred early; the patient is, it is true, unable to raise the limb *en masse*, but this may be owing to the effects of contusion upon the muscles; in fact, in cases such as those to which I allude, there may be at first neither shortening of the limb to any appreciable extent, eversion of the foot, nor any change in the position of the trochanter; and therefore, unless we can ascertain crepitus, it is most difficult to decide whether the neck of the bone be broken or not; and if we fail to elicit crepitus, the more prudent course is to withhold our opinion until time shall more fully develop

the nature of the injury. In these cases the surgeon is apt to allow greater freedom to the patient than is advisable, or, it may be that, in his anxiety to establish the nature of the injury by producing crepitation, he employs too much force in rotating the femur; in either case it is highly probable that the sudden occurrence of shortening of the limb will establish the nature of the lesion which the joint has suffered; the eversion of the foot now becomes decided, and the trochanter altered in position. These phenomena may take place long after the occurrence of the accident. In these instances it is most probable that, at the time of the receipt of the injury, the cervical ligament, having escaped laceration, prevented the retraction of the limb, but that subsequently it was torn, either in consequence of some imprudent exertion upon the part of the patient, or too eager a desire upon that of the surgeon to produce crepitus by forcible extension and rotation of the limb; the retraction then takes place as the immediate result of the laceration of this important membrane." P. 12.

Mr. Smith states, that if there be any one fact in surgical pathology more certain than another, it is this—that in cases of fracture of the neck of the femur within the capsular ligament, there is scarcely any callus ever effused; it certainly is never formed in such quantity as to be at all capable of counteracting the causes which produce shortening of the limb. We shall not follow our author in his rather tedious refutation of the views of M. Rodet on the subject of shortening after fractures, to which undue attention seems to have been bestowed by him. The position of the foot is not subject to as much variety as the shortening of the limb; in general it is turned outwards. Mr. Smith's experience would lead him to say that inversion of the foot is most frequently seen in cases of extra capsular fractures. He has seen several examples in which the foot was turned inwards, in five of which the fracture was external to the capsule. He remarks—

"There is one remarkable circumstance which appears to have escaped the observation of those who have described injuries such as those now alluded to, accompanied by inversion of the foot, and which appears to support the opinion that this symptom is to be ascribed to the relative position of the fragments, rather than to the influence of muscular action. I have observed it several times, and it is this: the deformity having been removed, and the limb restored to its natural length by extension, as soon as the extending force ceases to act, though the limb is again shortened, the foot will be found to remain everted." P. 25.

In every case of fracture of the neck of the femur, accompanied by inversion of the foot, which Mr. Smith has had an opportunity of examining after death, the inferior has been placed in front of the superior fragment. He gives the best description that we have hitherto met with of that remarkable but not uncommon species of fracture of the neck of the femur, termed the impacted fracture, in which the broken cervix is driven into the shaft of the femur between the trochanters. Our author justly observes:—

This peculiar form of fracture is the only one, the diagnosis of which is attended with difficulty, for it rarely happens that the limb is shortened to the same extent as in the ordinary examples of extracapsular fractures; in general the amount of retraction is nearly the same as in cases of fracture within the capsule. Between these two forms of injury, therefore, the shortening of the limb is not available as a means of differential diagnosis; upon further examination, however, it will generally be found that crepitus cannot be elicited upon

rotating the limb, because upon the one part the neck of the femur is so firmly wedged into the cancellated tissue of the shaft that the fractured surfaces cannot be moved upon each other; and upon the other the integrity of the strong fibrous and tendinous structure which invests the whole of the region traversed by the second fracture, which has been already alluded to as being always present, is such that the detached portion of the trochanter, whether large of otherwise, in general moves with the shaft of the bone, and it is only by submitting the patient to an examination unjustifiably severe, that we are occasionally enabled to produce this characteristic evidence of fracture. When we endeavour to extend the limb, we usually find that no force which it is safe to employ will restore it to its normal length, and in general the dislocated head of the femur will return into its socket more readily than the impacted cervix will leave the cavity between the trochanters, into which it has been driven.

"More remarkable deviations than these, from the ordinary symptoms of fracture of the neck of the femur, sometimes attend this particular lesion; and cases have occurred in which the patient has not only raised himself from the ground after the fall which caused the fracture, but has even walked a considerable distance, bearing his weight upon the injured limb." P. 29.

The cases are very liable to be mistaken for contusion of the hip. The evidence of the existence of an impacted fracture of the cervix femoris is of a negative rather than of a positive character, and is thus briefly stated. "1. Slight shortening of the limb. 2. Slight eversion of the foot. 3. Absence of crepitus. 4. Great difficulty in all cases, and in the majority of instances an impossibility, of removing the shortening of the limb by extension; and lastly, less loss of power than in other forms of fracture of the neck of the femur." The anatomical characters of this form of fracture, as well as of the fracture of the neck with the capsular ligament, are well described by Mr. Smith, and obviously from extensive observation of specimens of those injuries.

Our readers are probably aware that a peculiar form of injury of the neck of the femur, termed "partial fracture," has been described by the late Mr. Colles and also by Mr. Adams of Dublin. The former author has spoken of its occurrence in cases of fracture within the capsular ligament, and the latter has described the symptoms and the morbid appearances supposed to characterize it in extra-capsular fractures. Mr. Smith, after quoting the observations of Mr. Adams on this form of fracture, states that he has examined the specimens referred to by both these pathologists, and for reasons which are alleged he can come to no other conclusion than that the doctrine of partial fracture has not yet been proved to be correct.

In reference to that long-agitated question, the possibility of osseous union taking place in cases of fracture of the neck of the femur within the capsular ligament, Mr. Smith, after briefly stating and repeating the chief objections which have been made to the occurrence of such an event, gives a concise summary of the cases which constitute the evidence by which the possibility of osseous union taking place has in his opinion been established. Seven cases, most of them well known to surgical pathologists, are adduced. Our author thinks it highly probable that they have all been examples of impacted fractures: "certainly in all those, of which delineations have been given, there has been either penetration of one fragment by a portion of the other, or else the irregularity of the line of

fracture has been such that the displacement of the fragments has been prevented; they have been maintained in contact and at rest, and it is under such circumstances alone that we are to hope for the occurrence of bony consolidation."

Mr. Smith, after noticing the unfavourable nature of the prognosis in cases of fracture of the neck of the femur, the injury in many instances soon proving fatal, and in all the functions of the limb being for ever impaired, makes a remark which we can fully confirm, viz., that the form of fracture which is most rapidly and most frequently fatal is the extra-capsular fracture, when it is accompanied by a comminuted fracture with displacement of the trochanters. Our author next gives a brief account of the preparations which seem to warrant the conclusions he has deduced from them, accompanying the description with a concise history of the symptoms which each case presented during life. The preparations amount to sixty in number, and are nearly all illustrated by woodcuts. He subjoins a table, showing the age of the patient, the degree of shortening of the limb, and the position of the foot in these cases, as well as the length of time which elapsed between the receipt of the injury and the death of the patient. He terminates this elaborate investigation of fractures of the neck of the femur with the following conclusions:—

"1. A slight degree of shortening, removable by a moderate extension of the limb, indicates a fracture within the capsule.

"2. The amount of *immediate* shortening, when the fracture is within the capsule, varies from a quarter of an inch to one inch.

"3. The degree of shortening, when the fracture is within the capsule, varies chiefly according to the extent of laceration of the cervical ligament.

"4. It also varies according as the fracture is impacted or otherwise.

"5. In some cases of intracapsular fractures, the injury is not immediately followed by shortening of the limb.

"6. This is generally to be ascribed to the integrity of the cervical ligament.

"7. In such cases, shortening may occur suddenly, at a period more or less remote from the receipt of the injury.

"8. This sudden shortening of the limb is in general to be ascribed to the accidental laceration of the cervical ligament, previously entire, and is indicative of a fracture within the capsule.

"9. The deposition of callus around the fragments is not necessary for the union of the intracapsular fracture.

"10. When osseous consolidation occurs in the intracapsular fracture, it is effected by the direct union of the broken surfaces, which are confronted to each other.

"11. The osseous union of the intracapsular fracture is most likely to occur when the fracture is of the variety termed 'impacted.'

"12. In the intracapsular fracture, the mode of impaction is different from that which obtains in the extracapsular.

"13. The degree of shortening, when the fracture is external to the capsule, and does not remain impacted, varies from one inch to two inches and a half.

"14. When a degree of shortening occurs immediately after the receipt of the injury, we usually find a comminuted fracture external to the capsule.

"15. The intracapsular fracture is accompanied by fracture with displacement of one or both trochanters.

"16. The extracapsular *impacted* fracture is accompanied by fracture without displacement of one or both trochanters.

"In such cases, the fracture of the trochanters unites more readily than that of the neck of the bone.

"18. The degree of shortening, in the intracapsular impacted fracture, varies from a quarter of an inch to an inch and a half.

"19. The exuberant growths of bone met with in these cases have been erroneously considered to be merely for the purpose of supporting the acetabulum and the neck of the femur.

"20. The final cause of their formation is the union of the fracture through the posterior intertrochanteric space.

"21. The difficulty of producing crepitus, and of restoring the limb to its normal length, are the chief diagnostic signs of the impacted fracture.

"22. The position of the foot is influenced principally by the obliquity of the fracture, and the relative position of the fragments.

"23. Inversion of the foot may occur in any of the varieties of fracture of the neck of the femur.

"24. When the foot is inverted, we usually find that either a portion or the entire of the extremity of the lower is placed in front of the superior fragment.

"25. In cases of comminuted extracapsular fractures, with fracture and displacement of the trochanters, the foot will generally remain in whatever position it has been accidentally placed: it may be turned either inwards or outwards, or there may be inversion at one time and eversion at another.

"26. Severe contusion of the hip-joint, causing paralysis of the muscles which surround the articulation, is liable to be confounded with fracture of the neck of the femur.

"27. Severe contusion of the hip-joint may be followed, at a remote period, by shortening of the limb and eversion of the foot.

"28. The presence of chronic rheumatic arthritis may not only lead us to suppose that a fracture exists when the bone is entire, but also, when there is no doubt as to the existence of fracture, may render the diagnosis difficult, as to the seat of the injury with respect to the capsule.

"29. Severe contusion of the hip-joint, previously the seat of chronic rheumatic arthritis, and the impacted fracture of the neck of the femur, are the two cases most likely to be confounded with each other.

"30. Each particular symptom of fracture of the neck of the femur, separately considered, must be looked upon as equivocal; the union of all can alone lead to the formation of a correct opinion as to the nature and seat of the injury." P. 112.

In Chapter II. there is an account of the disease of the hip-joint termed by Mr. Adams "Chronic Rheumatic Arthritis." The symptoms of the disease, as described by Mr. Smith, are those of a chronic rheumatic affection with which we have long been familiar, though we have not hitherto connected them with the remarkable changes in the neck and head of the femur described and represented in the work before as. Since our attention has been called to these changes by the interesting observations of Mr. Adams in the Cyclopædia of Anatomy, we have been led to question whether the shortening of the neck, and the curious alteration in the form and depression of the head, occasionally met with, are altogether dependent upon a peculiar morbid change. We are satisfied that they may take place in advanced life as the result of the degeneration to which the bones are liable, without being attended with any rheumatic affection or marked symptom of disease. In this natural decay, the animal parts sometimes waste faster than the earthy, and thus the bone becomes so brittle that it is liable to fracture from the slightest violence; whilst in other cases the earthy parts are removed before the animal, so that the bone, being softened, becomes depressed and changed in shape by the

pressure to which it is subject. This latter change may, and indeed is very liable to be precipitated in chronic rheumatism of the joint,—the state of rest consequent on the painful nature of the affection, being favourable to the natural process of degeneration. The exuberant growths of bone, and eburnation of the bared head of the femur, combined with the other changes, are very probably the result of chronic rheumatism. There are some excellent woodcuts, illustrating the various alterations in the femur described in this chapter. Our own experience leads us to coincide with Mr. Smith in the following practical remarks:—

"It is an affection amenable to treatment in a very slight degree, and although its anatomical characters would lead us to suppose that it depended upon chronic inflammation affecting all the tissues entering into the composition of the joint, yet it is not found that antiphlogistic treatment produces any material alleviation of pain, nor is any permanent benefit derived from local bleeding or counter-irritation. Patients labouring under this affection not unfrequently present themselves at hospitals and dispensaries, in whom the entire of the region of the hip is covered with the marks of leeches, cupping, moxa, &c., but the disease has, nothwithstanding, steadily progressed, totally uninfluenced by such treatment. Rest, anodyne embrocations, keeping the joint protected by new flannel or carded wool from the influence of cold and damp, together with the free and long-continued use of hydriodate of potass, combined with the compound decoction of sarsaparilla, and small doses of colchicum, constitute the mode of treatment from which I have seen most benefit derived." P. 128.

The subject of Chapter III. is Fractures of the Bones of the Fore-arm in the vicinity of the Wrist-joint. In by far the most common form of fracture in this situation the lower end of the radius is broken and its lower fragment displaced backwards. This is an obscure form of injury until recently not well understood, to which the attention of the profession was called by an accurate description of it, published by Mr. Colles, in 1814, in the 10th volume of the Edinburgh Med. and Surg. Journal.* It has been particularly noticed by Dupuytren, and is also described by Chelius. Mr. Smith, after quoting Mr. Colles' account of this injury, gives some minute particulars of the symptoms which characterize it. The marked deformity at the wrist is well shown in the accompanying woodcuts. It has been supposed, from the symptoms of the injury, that the fracture is in many instances oblique. This is not the opinion of Mr. Smith. He states:

"I have lately examined upwards of twenty specimens of the injury, and have not found one in which the bone had been broken with any considerable degree of obliquity from above, either downwards and backwards, or downwards and forwards; in all of them the anterior and posterior margins of the fractured surface have been nearly upon the same level and the surface plain; there is, however, an obliquity which the fracture not unfrequently presents, and which is directed either from

* Mr. Smith finds fault with the learned author of the Surgical Dictionary for not alluding to Mr. Colles' account of this fracture, remarking that it is the duty of every person who undertakes to write upon a given subject, to make himself, as far as possible, acquainted with, and also to acknowledge, the labours of those who have preceded him in the same field of inquiry. The observation is a just one, but it is one, we think, particularly applicable to many of the writers of the Dublin School, though not to Mr. Smith.

within downwards and outwards, or from without downwards and inwards; even this obliquity, however, is always trifling, and scarcely ever sufficiently great to invalidate the truth of the general proposition (first maintained, I believe, by Monsieur Voillemier), that when the radius is broken within an inch of its lower extremity, the direction of the fracture is transverse, the direction which we would expect it to follow, from reflecting upon the manner in which the accident happens, and in which the force which breaks the bone is applied; for, when the fracture takes place from a fall upon the palm of the hand, as it usually does, the force, though principally and ultimately transmitted to the posterior surface of the lower end of the radius, yet is applied in a direction nearly parallel to the plane of the carpal surface of the bone." P. 142.

It appears that Voillemier, a writer in the Archives Générales de Medicine, regards all fractures of the lower end of the radius as examples of impacted fracture, and his views are given in the work before us at some length, and afterwards very fully discussed. This opinion is founded upon the fact that, in nearly all specimens of fracture of the lower end of the radius examined long after the occurrence of the injury, a line of compact tissue is found, continuous with the posterior wall of the shaft, and extending to a greater or less distance into the reticular structure of the lower fragment. Mr. Smith's attention having been directed to the subject by the memoir above referred to, he examined numerous sections of this fracture, and, for reasons which we have not sufficient space to quote, he considers that the doctrine "of fracture with penetration" is untenable. He believes that "the impaction is only apparent, and that the compact tissue of the shaft is not found enveloped in bone, from its having penetrated the lower fragment at the time of the occurence of the injury, but because it becomes subsequently incased in osseous matter during the process by which the bony union of the fracture is accomplished." We are disposed to agree with Mr. Smith on this subject, but we think that he has discussed the point at greater length than its importance would seem to require.

Mr. Smith describes a very rare injury, viz. "fracture of the lower extremity of the radius, with displacement of the lower fragment forwards." It generally occurs in consequence of a fall upon the back of the hand.

"It is accompanied by great deformity, the principal features of which are a dorsal and a palmar tumour, and a striking projection of the head of the ulna at the posterior and inner part of the fore-arm; the dorsal tumor occupies the entire breadth of the fore-arm, but it is most conspicuous internally, where it is constituted by the lower extremity of the ulna displaced backwards; from this point, the inferior outline of the tumour passes obliquely upwards and outwards, corresponding in the latter direction to the lower end of the superior fragment of the radius. Immediately below the dorsal swelling there is a well-marked sulcus, deepest internally below the head of the ulna, directed nearly transversely, but ascending a little as it approaches the radial border of the fore-arm.

"The palmar is less remarkable than the dorsal tumour; formed principally by the lower fragment of the radius, it is obscured by the thick mass of flexor tendons which cross the front of the carpus, but towards the ulnar border of the limb there is a considerable projection, which marks the situation of the pisiform bone, passing down to its attachment into which, can be seen the tendon of the flexor carpi ulnaris thrown forwards in strong relief. The transverse diameter of the fore-arm is not much altered, but the antero-posterior is considerably increased, and the radial border of the limb becomes concave at its lower part." P. 163.

Mr. Smith has never had an opportunity of ascertaining the anatomical characters of this fracture. He is quite satisfied, however, of the nature of the injury, and believes that it has not unfrequently been mistaken for dislocation of the carpus forwards.

"The facility, however, with which the deformity can be removed, its liability to recur when the extending force ceases to act, the production of crepitus when the limb is extended, and a motion of rotation given to the hand, and our being able to feel the irregular margin of the upper fragment of the radius posteriorly, are sufficient to enable us to distinguish this accident from luxation of the bones of the wrist forwards." 163.

Another form of injury occurring in the vicinity of the wrist is "a separation of the inferior epiphysis of the radius, with fracture of the lower extremity of the ulna." This accident is very liable to be mistaken for dislocation of the wrist backwards, but cannot be confounded with the ordinary fracture of the radius; the dorsal tumour is transverse, and the limb presents none of the peculiar deformity arising from the displacement of the lower fragment of the radius towards the side of supination which distinguishes the more common form of fracture.

"Although this injury assumes much the appearance of dislocation of the carpus backwards, it may yet be distinguished from it without any considerable difficulty, more especially should we be fortunate enough to see it, before the occurrence of tumefaction has obscured the diagnostic signs. The styloid processes of the radius and ulna can be felt, still holding their normal relations to the carpus, and, as has been remarked by Boyer, these processes move with the hand when any motion is imparted to the latter. If the distance between the superior margin of the dorsal tumour, and the extremity of the middle finger of the injured limb, be measured and compared with that between the corresponding point of the hand and the upper edge of the carpus of the sound limb, the former measurement will be found to exceed the latter by at least half-an-inch; sometimes the difference is greater; but if the case should be one of dislocation of the wrist backwards, this measurement will give the same results upon each side, and the styloid processes will be found to remain at rest when the hand is moved; if we add crepitation, the easy reduction of the deformity by extension, and its liability to recur when the extending power is removed, we are at once furnished with a group of symptoms, quite sufficient to enable us to distinguish this injury from that very rare accident, luxation of the wrist." P. 166.

With respect to the treatment of the ordinary fracture of the lower end of the radius with displacement of the lower fragment backwards, the object most difficult to be accomplished is to restore to the carpal surface of the bone its normal direction forwards, and this is not effected by the means recommended by Dupuytren. Mr. Smith has found the following mode of treatment to answer remarkably well, and to fulfill all the more important indications.

"The deformity having been, as far as possible, removed by extension and counter-extension, and the hand moderately adducted, a cushion is to be placed upon the posterior surface of the limb, of sufficient length to extend from the elbow to the fingers; the portion of this cushion which corresponds to the lower fragment of the radius and to the carpus should be thicker than any other part, and from its ulnar to its radial border, should gradually increase in thickness. A transverse section of this portion of the cushion would represent an isosceles triangle, the base of which would correspond to the radial border of the limb.

The objects proposed to be attained by constructing the pad of this form are, to press the lower fragment of the radius forwards, and to direct its external border towards the side of pronation.

"A second cushion, thicker below than above, is to be placed upon the front of the limb, but should not descend below the margin of the superior fragment, for otherwise it would, to a certain extent counteract the influence of the dorsal cushion, and would tend to maintain the displacement backwards of the inferior fragment. An anterior and a posterior splint are then applied, each of which should be at least an inch broader than the fore-arm; the posterior should extend from the elbow to the fingers, and should be curved from the wrist downwards, to receive the adducted hand; the anterior need not descend below the palm of the hand; a roller is then to be carried around the splints in the ordinary manner.

"By constructing and placing the cushions in the manner above described, the two fragments are pressed in opposite directions, and the carpal surface of the inferior is directed forwards, while the curved splint, by maintaining the hand moderately adducted, tends to restore to the articulating surface its natural direction inwards, quite as effectually as the ulnar splint of Dupuytren, and with much less uneasiness to the patient. It is scarcely necessary to mention, that the form which I have recommended can be readily given to that portion of the dorsal cushion which corresponds to the lower fragment, by the employment of graduated compresses." P. 168.

"During the application of the apparatus, as well as during the subsequent treatment, the fore-arm should be maintained in a position intermediate between pronation and supination, for it is in this position that the co-aptation of the fragments can be most easily and most perfectly accomplished, in consequence of the relaxation of the pronator and supinator muscles." P. 169.

We may here add the description of the plan of treatment in these cases recommended by Professor Fenger of Copenhagen, and recently communicated to the Royal Medical and Chirurgical Society.* He states that, as the deviation occurs in a curve, with its centre upon the fracture, it is desirable to counteract the deformity by extension acting in a direction according to the tangent of that curve. This end he thinks is best attained by acting through the medium of the hand and of the capsular ligament which is attached to the lower end of the radius. The hand is first to be brought into a position of strong flexion, and the fore-arm is then placed on an oblique plane, with the carpus highest, the hand being permitted to hang freely down the perpendicular end of the plane. The tendons of the extensor muscles are thus brought into a position which enables them to assist in keeping the reduced fragments of the bone in proper relation. Where the deformity requires it, the displaced lower fragment is to be pressed into its position by the thumb of the operator, after sufficient extension has been made, and when the hand is bent on the fore-arm. The patient is to be kept in bed, but the hand is not confined, the seat of fracture being covered only by an evaporating lotion.

Mr. Smith's views are summed up in a number of general corollaries, which we regret that we are unable to find room for. This chapter presents the clearest view of the anatomical characters, symptoms, and diagnosis of the common fracture of the lower end of the radius that we know

* The Lancet, Vol. I., 1847, p. 487.

of, and its obscure nature and the difficulty of managing it, perhaps justify the length with which it is treated.

Chapter IV. treats of "Fractures of the Humerus, in the vicinity of the Shoulder-joint." On the subject of fractures of the greater tuberosity of the humerus we find nothing particularly deserving for notice. Mr. Smith has had but one opportunity of observing the anatomical characters of the injury. The accident had occurred many years before death, and the history connected with it could not be precisely ascertained.

Our author describes two varieties of impacted fracture of the neck of the humerus. In one, the upper extremity of the lower fragment penetrates the recticular tissue of the head of the bone; "this is an extra-capsular fracture, and occupies the situation which, in the young subject, marks the junction of the epiphysis with the shaft; in the other, the superior fragment is forced downwards into the cancellated structure between the tubercles, the greater of which processes is, in almost every such instance, split off from the shaft of the humerus; in this case the fracture is intracapsular, and occurs through the anatomical neck of the bone." Both these injuries are fractures of the true anatomical neck of the humerus, which, our experience would lead us to say, are of very rare occurrence. They have been confounded with fractures through the tuberosities, or through the line of junction of the epiphysis with the shaft of the bone.

"In the latter, the deformity is considerable, in consequence of the lower fragment being drawn inwards by the muscles which constitute the folds of the axilla; but in the former there is scarcely any displacement of the inferior fragment, the influence of these muscles being counteracted by the supra-spinatus, infra-spinatus, and teres minor, attached to the greater tubercle; the bone being thus placed between two opposing forces, suffers very little displacement, and the deformity is slight in proportion; hence the diagnosis of fracture of the anatomical neck of the humerus is, comparatively speaking, obscure. The impairment of the motions of the joint and crepitus are, in fact, almost the only symptoms upon which we can depend, in forming our opinion as to the nature of the injury which the bone has sustained." P. 185.

Notwithstanding the unfavourable circumstances in which the head of the humerus is placed as regards bony union, osseous consolidation takes place, the impaction serving to maintain the parts in contact. Mr. Smith describes some interesting cases and specimens of the injuries in question.

"Separation of the superior epiphysis from the shaft of the humerus," is an accident which Mr. Smith states not unfrequently occurs in early life. Our experience would lead us to say that it was a rare form of injury. It is attended by a considerable degree of deformity, but of so striking a character, that there is no great difficulty in recognizing the true nature of the injury.

"The axis of the arm is directed from above, within, and before, downwards, outwards, and backwards; the elbow, however, projects but little from the side, and can be brought into contact with it with facility; the head of the bone can be distinctly felt in the glenoid cavity; a slight depression is seen beneath it, and it remains motionless, when the shaft of the humerus is rotated.

"The most remarkable feature, however, of this injury, is a striking and abrupt projection, situated beneath the coracoid process, and caused by the upper

extremity of the lower fragment or shaft of the bone, drawn inwards by the muscles which constitute the folds of the axilla: there is but little displacement as regards the length of the bone, for the extremity of the inferior fragment is seldom drawn so far inwards as to enable it to clear completely the surface of the superior. Were this to occur, the humerus would, of course be drawn upwards by the muscles passing from the shoulder to the arm, in a direction parallel, or nearly so, to the axis of the humerus, and a corresponding diminution in the length of the limb would result.

"This remarkable and abrupt projection does not present the sharp, irregular margin of an ordinary fracture; on the contrary, it feels rounded, and its superior surface is smooth and slightly convex. The latter can be felt as plainly as the cup-like cavity of the head of the radius, in cases of luxation of that bone backwards at the elbow joint. By pressing the upper end of the lower fragment outwards, and directing the elbow inwards, during extension and counter-extension, crepitus can be perceived, and the deformity removed without much difficulty; but the moment the parts are abandoned to the uncontrolled action of the muscles, the deformity recurs." P. 201.

Fractures of the Acromial Extremity of the Clavicle are considered in Chapter V. The chief point of interest in relation to this inquiry adduced by our author, is the occurrence of displacement of the outer fragment of the clavicle in cases of fracture between the trapezoid ligament and the acromio-clavicular articulation, which, he says, is, in general, considerable, its inner extremity being drawn inwards. This displacement is frequently carried to such an extent that the fragments form a right angle with each other; and it is principally due to the action of the clavicular portion of the trapezius muscle. In consequence of the displacement the clavicle is shortened.

In Chapter VI. Mr. Smith treats of Dislocations of the Bones of the Foot. After briefly alluding to the displacements with which surgeons are already familiar, he remarks that the instances of luxation he is about to describe are different from any that have yet been recorded. They are instances of dislocation of the metatarsus and internal cuneiform bone, upwards and backwards upon the tarsus. Its external characters are said to be very striking, and clearly to indicate the nature of the accident.

"The foot is greatly deformed, but it is at first sight obvious, that the relations which the bones composing the ankle-joint bear to each other, are undisturbed. There is, it is true, a remarkable fore-shortening of the foot, but we are not likely, on this account, to confound the accident with displacement of the tibia forwards, for there is no corresponding elongation of the heel: the foot in front of the ankle-joint, is shortened to the extent of an inch or more, but the heel preserves its natural relations to the bones of the leg.

"The foot likewise appears to be rotated upon its long axis, in such a manner that the aspect of the dorsal region is directed outwards, and that of the plantar inwards; the inner edge of the foot is elevated and the outer depressed. These alterations resemble those which are the results of the dislocation of the tibia outwards; but in the latter accident the outer edge of the foot is applied to the ground throughout its whole length, whereas, in the injury under consideration, it is only the central third of the external margin of the foot, which is presented to the ground in walking.

"The next remarkable symptom which attracts observation, is the alteration

which the form of the sole of the foot undergoes. Instead of presenting its natural concavity, the plantar region becomes convex, both in its antero-posterior and transverse diameters; and hence it is that, in standing or walking, the central third alone, or nearly so, of the outer margin of the foot, is presented to the ground, as has been already mentioned."

"The dorsal region of the foot presents a transverse prominence, situated about an inch below, and in front of the ankle-joint; commencing almost imperceptibly at the external margin of the foot, it gradually becomes more evident till it terminates in a very distinct projection at the inner edge of the tarsus. Between this prominence and the ankle-joint there is a superficial sulcus. The line at the transverse convexity in the plantar region, is opposite to the ridge upon the dorsum of the foot, but its most prominent part is at the outer edge of the tarsus." P. 226.

Such are the principal diagnostic signs of this dislocation. Mr. Smith has twice had an opportunity of ascertaining by *post-mortem* examination the precise nature of the accident, the luxation having remained unreduced in both instances. The anatomical characters presented in these cases are minutely described, but we must refer our readers to the work, in which they will also find several wood cuts, without seeing which, they would scarcely be able to comprehend Mr. Smith's description of this peculiar displacement of the tarsal bones.

"This remarkable and rare dislocation may be produced by the passage of a heavy body over the dorsum of the foot, but is much more liable to occur when a person, in falling or leaping from a considerable height, alights upon the anterior part of the foot. Under these circumstances, the limb is submitted to the operation of two forces acting in opposite directions; one, the weight of the body and impulse of the fall, tending to depress the tarsal bones, the other, the resistance of the ground, tending to displace the metatarsus upwards; the articulating surfaces thus glide past each other, and the anterior part of the foot is then drawn backwards, and the aspects of its surfaces altered by muscular action." P. 233.

In Chapter VII., on Congenital Luxations of the Wrist-joint, Mr. Smith quotes a case, recorded by Cruveilhier, as an example of luxation of the carpus forwards, but which so closely corresponds with the appearances in a case dissected by our author, and regarded as a congenital luxation, that he is compelled to arrive at the conclusion that the French pathologist is in error, and that the lesion in his case is also an original malformation.

Chapter VIII., which treats of Congenital Dislocation of the shoulder, is an extension of a memoir published by Mr. Smith, in the 15th volume of the Dublin Medical Journal.

Mr. Smith has ascertained the existence of two varieties of this malformation. "In one of these, the head of the humerus is placed beneath the coracoid process; while in the other and more rare variety, it is lodged in an abnormal socket, formed upon the dorsum of the scapula, below the outer and posterior part of the acromion. They may be termed the subcoracoid and sub-acromial congenital luxations, and may exist either upon one side only, or upon both." Of the latter species our author has seen only one example, but several instances of the former have come under

his observation within the last few years. Five cases are detailed, and representations given of certain alterations in the form of the head of the humerus and of the glenoid cavity, which were noticed in some of the cases. This is a lesion involved in some obscurity, and it is by no means clear that all the cases were really instances of congenital malformation.

Chapter IX. is on Dislocations of the Lower Jaw. Mr. Smith relates a very curious case of what he terms, we think incorrectly, congenital luxation of the inferior maxilla in a man, æt. 38, and idiot from his infancy, who died of diseased lungs.

"While engaged in examining the head, a singular deformity of the countenance, which is accurately represented in the accompanying figure, attracted my observation. The right and left sides of the face seemed as though they did not belong to the same individual, the left being in every respect larger and more fully developed. Upon this side the prominence of the malar bone and of the arch of the zygoma, the development of the masseter muscle, and the fulness of the parotid space, all in a remarkable manner contrasted with the atrophy of the opposite half of the countenance, which, in the situation of the zygoma, presented a concavity in place of a convexity, and in the parotid space a very distinct depression. The countenance was, moreover, crooked in almost every feature. Upon the right side the angle of the mouth was higher than upon the left, while upon the left the orbit was placed a little higher than upon the right; the superciliary arch was much more projecting, and the eye more prominent.

"The right side of the face appeared as it were sunk in, and the extremity of the finger could be placed between the parotideal margin of the jaw and the front of the external auditory canal."

"Upon elevating the integuments, and contrasting the dissected muscles of either side, those of the right were found to be much smaller than their fellows of the left side. The masseter, in particular, was atrophied both in length, breadth, and thickness; the temporal and pterygoid muscles also appeared small when compared with those of the opposite side, but as regarded colour and consistence, the muscular fibre did not deviate from the normal condition." P. 276.

"When the mouth was closed, and the teeth maintained in apposition, as exact as the abnormal state of the parts admitted of, the external lateral ligament of the lower jaw, instead of being directed backwards, was seen descending obliquely forwards, to be attached to a very imperfectly-developed condyle, which was not in contact with that portion of the temporal bone which, in the natural state, corresponds to the glenoid cavity, being separated from it by an interval of at least a quarter of an inch.

"There was neither an inter-articular cartilage nor cartilage of incrustation, the osseous surfaces of the joint being invested by a thick periosteum alone. There was no distinct capsular ligament." P. 277.

"The right side of the inferior maxillary bone was considerably smaller than the left, the atrophy extending forwards to within a very short distance of the symphysis, and affecting the bone as to its length, breadth, and thickness, the ramus being half an inch less in its transverse diameter, and its parotideal margin half an inch shorter than upon the opposite side.

"The lower edge of the bone presented a deep concavity at its posterior part, and the angle was remarkably prominent and curved outwards; the parotideal margin, thin, concave at its upper part, and forming nearly a right angle with the body of the bone, terminated above in a small curved process, directed nearly horizontally inwards, its superior surface being directed slightly outwards, and its inferior slightly inwards.

"This process, which in form somewhat resembled the coracoid process of

the scapula, was the only vestige of the existence of a condyle, and was destitute of cartilage.

"The external pterygoid muscle was attached to its anterior and inner part, and the external lateral ligament to its outer surface; there was a complete arrest of development in the condyle; the coronoid process was small and thin, and the sigmoid notch could scarcely be said to exist." P. 279.

The temporal malar, superior maxillary and right side of the sphenoid were also more or less atrophied. The right orbit was in consequence smaller than the left. The figure of the base of the skull shows also a diminution in the size of the arch formed by the occipital and parietal bones, though this is not mentioned by Mr. Smith. The case seems to us simply one of arrest of development of all the bones entering into the composition of the right half of the skull and face, the abnormal condition of the articulation of the lower jaw on that side being merely a consequence of the imperfect development of the bones composing it, and proper growth of the left half of the lower jaw. The case, however, is curious and interesting. Mr. Smith next describes the symptoms of the ordinary dislocation of the lower jaw, the detection of which is unattended with difficulty. When the luxation is confined to one side, it is not so easily recognised. The symptom which Mr. Hey found to be the best guide, in this case, is a small hollow which can be felt behind the condyle that is dislocated, which does not subsist on the sound side.

Mr. Smith notices the distortion of the countenance produced by chronic rheumatic arthritis of the temporo-maxillary articulation. "In the majority of instances, this remarkable disease attacks those of advanced age, and is symmetrical; but occasionally it occurs during the period of adult life. In the latter case, it is generally more rapid in its progress, is accompanied by greater pain, and is more liable to implicate the neck of the condyle and the ramus of the jaw." The nature of the distortion which attends this remarkable affection, varies according as one or both articulations are engaged in the disease.

"When it is confined to that of one side, and has existed for a considerable period, the mouth becomes crooked, the affected side of the jaw being drawn forwards and towards the opposite side, so that the teeth of the lower jaw, upon the sound side, project beyond those of the superior maxilla; but when the disease is symmetrical, the entire of the jaw is drawn forwards, and the chin projects just as it does (although from a different cause) in the edentulous subject.

"The glenoid cavity, when the disease has been of long duration, becomes increased as to its circumference, this enlargement being accomplished at the expense of the horizontal and transverse roots of the zygoma, more especially of the latter, which in all cases is to a greater or less extent absorbed.

"It is upon the destruction of this transverse root, or articular eminence, that the distortion of the countenance depends, for when the removal by absorption of this process is, to a certain extent, accomplished, the external pterygoid muscle then draws the jaws forwards and to the opposite side, where but one articulation is diseased, and the muscles of each side act in displacing it directly forwards, when the destruction of the articular eminence is symmetrical." P. 292.

We have quoted our author's account of this rheumatic affection of the jaw, as it is one which we have not hitherto recognized, and we believe will be new to most of our readers. In the last chapter of this work,

Mr. Smith furnishes some additional observations on fractures of the radius, dislocations of the bones of the foot, and congenital luxation of the wrist-joint, in support of his views on these subjects.

Our notice of Mr. Smith's work has been so full that our readers will be able to form some estimate of its value. He has endeavoured to throw light on injuries and affections of an obscure, complicated, or rare character, and to assist surgeons in discriminating more clearly than heretofore the various lesions implicating the principal articulations. In the execution of this task he has collected many important and interesting facts, which he has described clearly and discusssed without bias, and with a desire to advance our knowledge of surgery. The work is enriched with a large number of woodcuts, which were really necessary to enable the readers of it to comprehend the descriptions in the text. These figures are well executed, but there is a hardness about most of them, and a want of that finish which may be observed in the best woodcuts which illustrate the works that have recently issued from the London press.

Should this work reach a second edition, we advise Mr. Smith to give his numerous extracts from the French writers in translation, instead of in, or in addition to, the original. He may reply that this is unnecessary, as every well-educated medical man in the present day is able to read the French language; which is true enough; but we know that there are a great many members of our profession who unfortunately are not well educated, or who are unacquainted with any other modern language than their own, but who may nevertheless desire to consult Mr. Smith's book, and it is for their benefit that we make this suggestion.

LECTURES ON THE COMPARATIVE ANATOMY AND PHYSIOLOGY OF THE VERTEBRATE ANIMALS; Delivered at the Royal College of Surgeons of England, in 1844 and 1846. By *Richard Owen*, F.R.S., Hunterian Professor and Conservator of the Museum of the College. Part I.—Fishes. Illustrated by numerous Woodcuts. 8vo. Longman & Co. 1846.

THE second volume of this important work has now made its appearance, and calls for an early notice at our hands. It may be considered as by far the most important of the series, as it enunciates the new and profound views of its distinguished author on some of the highest and most abstruse theories of transcendental anatomy. It is for this reason that we now propose to take up this volume for our separate consideration, not associating it, in our notice, with the former one, nor waiting until the work shall be completed by the publication of the third and concluding part.

The promulgation of theories, and many of them highly ingenious and even exhibiting no small degree of philosophical generalization, in the structure of particular groups of animals, is not a new thing. The first, however, who carried his views of the subject into anything like a full and consistent plan, was Geoffroy St. Hilaire; whose genius, as brilliant as it

was bold, gave to his theories a character of deep and varied interest, by which the philosophical world was equally astonished and charmed. The fascination which belonged to his splendid and glorious idea (and we feel no hesitation in designating it by so strong a term,) the charm which always attaches to a single, uniform, and all-inclusive theory dazzled his first disciples, and prevented them from seeing the inconsistencies and incongruities which in truth were included in it, whilst those to whose clearer vision these faults were apparent, became the founders or the adherents of an opposing party, who rejected the principle on account of its defects, and fell into a greater error in endeavouring to escape from the less.

Passing the clever vagaries of Carus, and the hypotheses of many others of various degrees of demerit, we believe that it was left to Professor Owen, by the enunciation of a clear and tangible theory, by the discrimination between those structural elements which are essential to the relation of beings of the same type, and those which only subserve a definite and, as it were, individual purpose, to exhibit the true conditions of that beautiful and unerring law which provides for the various requirements of every subordinate group or species, by modifications of the typical structure, rather than by the loss of any of its essential elements and the substitution of others foreign to it; whilst it recognizes, in certain subordinate structures which have hitherto been confounded with the former essential and typical ones, a certain superadded and non-essential development of abnormal elements. To these two distinct classes the author gives the terms—somewhat pedantic perhaps, but sufficiently expressive—of *homological* and *teleological* structures.

Before we advance, however, to the more detailed development of this theory, we will allow the author to exhibit his general views on the structure of the great division of the animal kingdom, the Vertebrata, on a portion of which the present volume treats.

We are met, *in limine* by an expression which we confess we do not fully understand. Are we to consider it as declaring the deliberate judgment of the author on the comparative merits of the different objects of study and research there mentioned? The passage to which we allude is as follows:—

"And first, permit me to dwell a little on the inestimable privilege which we enjoy, in entering upon our professional studies by the portal of anatomy.

"How vast and diversified a field of knowledge opens out before us as we gaze from that portal! Consider what it is that forms the subject of our essential introductory study; nothing less than the organic mechanism of the last and highest created product which has been introduced into the planet; *contrast this, which both sage and poet have called 'the noblest study of mankind,' with the dry and unattractive preliminary exercises of the Lawyer or the Divine.*

Indeed! is this Professor Owen's deliberate opinion? We are old-fashioned enough to think that there may be a still nobler study to which the mind, the heir of immortality, may be devoted. We have fancied, without any derogation from the value and importance of the casket, that the gem contained within it was still more worthy of contemplation—that the value and interest of the body depend mainly on its being the seat of the immortal part of our nature, and that the divine emanation by which the corporeal form is animated and ennobled, may afford subject for a

higher and even more interesting investigation, than the material of which we are composed. But let this pass.

The following statement of the author's views on a theory, the truth of which has for some years been too generally and readily taken for granted, will be read with interest, although we could have wished it to have been more fully developed.

"In ascending to man, we trace a very extensive and varied but progressive course of development, through the great vertebrated series which commences at a very low point.

"It might, perhaps, be imagined that the lowest vertebrate forms began where the highest invertebrated form ended, and made a direct step in advance in the scale of animal organization. Such, indeed, ought necessarily to follow on the hypothesis of the development of species by progressive transmutation, and of the arrangement of animal life in a single and uninterrupted chain of being.

"But truer views of the nature and direction of Zoological affinities, and a deeper insight into the laws of Development and of Unity of Organization in the Animal Kingdom, concur to disprove the once favourite and recently-revived hypotheses. We have seen that the Invertebrata resemble each other only at the earliest and most transitory periods of their development, diverging thence, in special directions, to the manifestation of very distinct types of animal structure. So likewise we must look to the very beginning of the development of the Vertebrate animal before we shall discover that amount of concurrence which will justify us in predicating 'Unity of Organization' between it and any of the Invertebrated forms. And when, with infinite care and minutest scrutiny, availing ourselves of all the aids and appliances of optical art, we have arrived at clear and satisfactory demonstration of the greatest amount of resemblance, in constitution and properties between the Vertebrate embryo and the Invertebrate adult—it is not with any of the higher forms of Invertebrata—with neither the Cephalopod, the Arachnidan, nor the Insect—that such organic correspondence is found to exist; but it is with the lowest forms and simplest beginnings of animal life—with the infusorial monads. Only, in fact, during that period of the ovum life of the Vertebrated being, in which the mysterious properties of the impregnated germ-vesicle are diffused and distributed by fissiparous multiplication amongst countless nucleated cells—the progeny of the primary germinal vesicle and coheirs of the seminal virtue—do we find such a form and such properties of the Vertebrated animal as justify us in affirming that there is 'Unity of Organization' between it and an Invertebrate animal." P. 11.

A succinct and well-arranged account of the general structural characters of the vertebrate classes occupies the principal portion of the first lecture, which however does not exhibit any novelty in generalization or detail to warrant us in delaying our advance to the next lecture, in which the author's original views on the homologies of osteology are first enunciated. The following passage, which commences this portion of the work, exhibits the extensive range of organs which the author considers as comprised within the definition of the term skeleton.

"The branch of Anatomy which treats of the Skeleton of Vertebrate animals is designated 'Osteology,' because in anthropotomy it relates exclusively to the bones and teeth. But the skeleton, according to its etymological signification of hard and dry parts, might apply to the hair and nails, and, indeed, the entire epidermal system. When, also, in a general survey of the Vertebrata, we see the spinal column gristly in some fishes, and the tendons bony in some birds; and when we call to mind such homological relations as that of the fibro-membranous sclerotic of the human eye with the cartilaginous sclerotic of the turtle

and the osseous sclerotic of the cod-fish, it will be obvious that the present branch of anatomy ought naturally to embrace the aponeuroses, ligaments, and cartilages, since these are so many arrested stages in the histological development of the internal skeleton." P. 20.

We confess that we dissent from this extended view of the term, as being absolutely indefinite; and the examples just cited by Professor Owen only appear to us to exhibit the difficulties of a definition without solving them. The distinction, however, which he has drawn, in the former volume of his work, between the terms analogue and homologue, and the subsequent development of his theory of homological and teleoligical relations, show that the difficulty is confined to words, and that the author's views are clear, distinct, and definite.

The hypothesis of Geoffroy St. Hilaire, of the theoretical identity of the crust or exo-skeleton of the annulosa and the endo-skeleton of the vertebrata, an hypothesis which involved the author in such a labyrinth of contradictions, as required a genius like his to give it even the colour of consistency, has at present but few supporters. The application of the term skeleton, indeed, to such parts as the skin of the annelides, and the polypary of the corals was enough to startle those who had been accustomed to the older restriction of the term to the actual bony structures of the vertebrata, and the endeavour to prove the homologies (to apply a new term to an old idea) between the rings of the articulata, the scutum of the cephalopoda, and the vertebral column of the highest grand division of the animal kingdom, was sufficient to split the scientific world at once into two great parties; those who looked upon this great naturalist as an unerring guide, the Newton of transcendental anatomy, and those who considered him as a mere visionary, ingenious indeed and full of talent, but still altogether a visionary. On this subject Professor Owen has the following pertinent remarks:

"In the Invertebrata we saw that the skeleton, or parts analogous to the bones of the Vertebrata, commonly consisted of large, strong, thick, often unjointed plates, developed in or upon the skin, hardened principally by carbonate of lime, protecting the whole body, and having the muscles attached to the inner surface.

"In the Vertebrata the skeleton chiefly consists of diversely configurated, but most commonly cylindrical and articulated pieces, hardened chiefly by phosphate of lime, developed from fibrous and cartilaginous tissue in the interior of the body, of which it forms the internal framework, giving attachment to the muscles by the outer surface, and subserving their action as levers and fulcra.

"The exterior calcified shells and crusts of the Invertebrata are unvascular; they grow by the addition of layers to their circumference, or they may be cast off when too small for the growing body, and be reproduced of a more conformable size; but they have no inherent power of repair.

"The internal bones of the Vertebrata are vascular; they grow by internal molecular addition and change, and have the power of repairing fracture or other injury.

"Such are the broad and obvious distinctive characters of the skeletons of the Invertebrate and Vertebrate animals; the contrasts having relation chiefly to the difference in the development of the nervous system. Thus, when the powers of discerning and avoiding lethal or hurtful agencies are dull and contracted, the entire animal is protected by a hard insensible dermal armour, or exo-skeleton; but, as those powers become expanded and quickened, the body

is disencumbered of its coat of mail, the skeleton is put inside, and made subservient to the activities, and the skin becomes proportionally more susceptible of outward impressions of pleasure and pain.

"Some estimable anatomists, who have more especially devoted their attention to the detection of the corresponding parts in different animals, have supposed that these different functions were performed by modifications of essentially the same or homologous parts of the skeleton.

"Observing that a segment of the outer skeleton of an articulate animal, the thoracic ring of a lobster for example, formed a small canal for the nervous trunks, and a larger one for the vascular trunks and plastic organs; and that a thoracic segment of the skeleton of a Vertebrate animal also formed a small protecting canal for the spinal chord, and a larger hoop about the vascular and other viscera of that cavity,—they have concluded that both were modifications of the same elements or primary segment of the skeleton. Carus, for instance, calls both rings 'vertebræ;' and Geoffroy St. Hilaire thought it needed but to reverse the position of the Crustacean,—to turn what had been wrongly deemed the belly upwards,—in order to demonstrate the unity of organization between the Articulate and Vertebrate animal. But the position of the brain is thereby reversed, and the alimentary canal still intervenes in the Invertebrate between the aortic trunk and the neural canal.

"The outer and the inner skeletons do agree in certain relations; neither of them are primitive parts of the organism, but are modifications or metamorphoses of other pre-existing systems: both serve as fixed points of attachment to the muscles, aid their action as levers, and determine the kind of movements by particular joints: both are organs of protection and support.

"But, besides the differences of tissue, mode of growth and vital properties, already noticed, the exo- and the endo-skeletons differ in the one being developed from the skin, the other from the internal cellular and fibrous systems. The exo-skeleton defends or surrounds the periphery of the animal; the endo-skeleton the internal parts. The exo-skeleton is related to the muscles by its inner surface, the endo-skeleton by its outer surface. The exo-skeleton is the reflex of the circumambient medium and relations of the animal: the endo-skeleton is the index of its motive energies and its intelligence." P. 22.

And he very properly adds—

"Only the highest of the Mollusca possess a true homologue of the endo-skeleton, developed in relation to the defence of the nervous centres: but it is a feeble cartilaginous rudiment in the best organized Cephalopods; and, in the cuttle-fish, is far outweighed by the calcareous dorsal plate which still represents the exo-skeleton of the testaceous mollusks. Thus a cartilaginous cranial vertebra co-exists in the highest Invertebrata with a calcareous dermal skeleton; and there is no abrupt contrast in passing thence to the consideration of the skeleton in the Vertebrata." P. 23.

The existence of an exo-skeleton is recognized, indeed, by our author in the vertebrata in the scales of fishes in all their modifications, in those of serpents and lizards, in the ossified scales of the crocodile, and the horny plates of the tortoise, the "tesselated armour" of the armadillo, the imbricated pointed scales of the manis, the spines of the hedgehog, the quills of the porcupine, the feathers of the bird, and the hair of the ordinary quadruped.

In treating of the structure of true bone, the following passage contains Mr. Owen's opinions on this important point in structural anatomy.

"The blastema or primitive basis of bone is not originally cartilage, but more resembles mucus in its chemical characters: it appears at first to be a sub-

transparent glairy fluid, but contains a multitude of minute corpuscles. Its assumption of the cartilaginous character and consistency is attended with the appearance in it of numerous small, sub-elliptic, nucleated cells. As the cartilage hardens, these cells increase in number and size, and begin to accumulate, and to be arranged in linear series at the part where ossification is about to commence. These series in the cartilage of long bones are usually vertical to its ends, and in flat bones are vertical to the peripheral edge; *i. e.* they are parallel to the axis of the long bone, and are radiated in the flat one, but not with mathematical exactness.

"The nucleated cells are the instruments by which the earthy particles are arranged in order; and, in bone, as in tooth, there may be discerned in this predetermined arrangement, the same relation to the acquisition of strength and power of resistance, with the greatest economy of the building material, as in the disposition of the beams and columns of a work of human architecture,

"The power of the cells so to operate upon the salts of the plasma, which percolates the intervening tissue, seems to reside chiefly in the repellent property of their nuclei: I have been led by observation of some of the phenomena of osteogeny to surmise that the walls and the nucleus of the cell were in opposite electric or magnetic states, one attracting, the other repelling, the surrounding earthy particles.

"Certain of the columnar series of nucleated cells become more aggregated or pressed together; their nuclei become more concentrated, and, according to Miescher and Gerber, they coalesce and become dissolved, leaving a cylindrical tube, parallel with the long axis of the future bone. First, a reddish lymph, and then a capillary vessel is prolonged into each of these cylinders, which is converted into a 'Haversian,' or vascular canal: before, however, the direct influence of the circulation has penetrated so far, the nucleated cartilaginous cells have arranged or propagated themselves in concentric series round the cylinder, and the intervening layers of the molecular blastema begin to be impregnated with the hardening salts, which, being repelled by the nuclei of the cells, are forced into the concentric laminated arrangement around the Haversian canal. The establishment of the capillary circulation in these canals accelerates the progress of ossification by the rapid import of new material: the resisting nuclei of the surrounding concentric cells, pressed on all sides, undergo a remarkable change, and the nuclear matter is forced out in rays, but chiefly in the direction where the resistance is least, viz. towards the Haversian canal. The remaining central nuclear matter and that of the diverging rays finally become dissolved, and establish permanent bone-cells and minute tubes, which tubes traversing the concentric lamellæ open into the Haversian canal, and receive the transuded plasma from the blood-capillary. The tubes branch and anastomose, and form the medium of the transmission of the plasma through the densest osseous tissue." "I have detected a similar system of plasmatic tubes in tendon: and they probably exist under characteristic modifications in all tissues constituting the essential nutritive system of each."

The true nature of the "metamorphosed cartilaginous cells in bone" is maintained by Professor Owen to be the cells or lacunæ filled with earthy salts; in which opinion he follows Müller. It is indeed surprising how any one, who has carefully examined these little "radiated corpuscles" could have come to the conclusion that they were either empty lacunæ, as some have maintained, and still do maintain, or cavities filled with fluid, or, in fact, anything but what Müller and Owen and others have declared them to be. The term *corpuscula ossea*, applied to them by Purkinje, is indeed as incorrect as that of empty lacunæ. The solid white reflection from them, when viewed by reflected light, and the dense opacity which is presented by transmitted light, are surely appearances only to be accounted for by

this view of their nature; and it is amply borne out by the fact first observed by professor Müller, that by the action of dilute acid the dark opacity is removed when observed under the latter circumstances, and the bright whiteness under the former. But we must pass over a very interesting detail of some of the principal modifications on the structure of the bones in different animals, to the consideration of the important question propounded by our author in the words "what is a bone?"

This question will not perhaps strike an ordinary human anatomist as one of any great difficulty. He will probably be quite satisfied with the simple definition that "*a bone is any single piece of osseous matter entering into the composition of an adult skeleton;*" which it is obvious is very little more definite than to say that a bone is a bone. Others, perhaps, will consider it sufficient, with Cuvier, to descend to "the primitive osseous centres as they exist in the fœtus."

The former of these definitions requires no refutation; and we refer our readers to Mr. Owen's treatment of the question at p. 36. The definition of Cuvier, however, must not be so passed over. There appears upon the face of it so much plausibility, the idea is in itself so ingenious, and its application so simple, that we are not surprised at the general favour with which if was received, and the hold it has had on the opinions of the most distinguished anatomists. But the consequences of the admission of this theory are such as to carry its refutation with them.

"According to this rule," says Professor Owen, "we ought to count the humerus as three bones, and the femur as four bones instead of one; for the ossification of the latter begins at four distinct points, one for the shaft, one for the head, one for the great trochanter, and one for the distal condyles. But who will be induced to regard these parts and processes as distinct bones? No such distinction is kept up in any of the lower classes. In both birds and reptiles the femur is developed from a single centre." P. 38.

The consideration of this question leads the author now to the development of those views to which we have already adverted, and which form, as we confidently believe, the true key to the difficulty. The following passage, for the length of which we do not offer any apology, contains the first distinct enunciation of these views; we allude, of course, to the important distinction between homological and teleological structures in the skeleton.

"The rule laid down by the great French anatomist falls in its application to the difficult point under consideration, because he did not distinguish between those centres of ossification that have homological relations, and those that have only teleological ones: *i. e.* between the separate points of ossification of a human bone which typify permanently distinct bones in the lower animals, and the separate points which, without such signification, facilitate ossification, and have for their final cause the well-being of the growing animal. The young lamb or foal, for example, can stand on its four legs as soon as it is born; it lifts its body well above the ground, and quickly begins to run and bound. The shock to the limbs themselves is broken and diminished at this tender age, by the divisions of the supporting long bones,—by the interposition of the cushions of cartilage between the diaphyses and the epiphyses. And the jar that might affect the pulpy and largely developed brain of the immature animal is further diffused and intercepted by the epiphysial articular extremities of the bodies of the vertebræ.

"We thus readily discern a final purpose in the distinct centres of ossification of the vertebral bodies, long bones, and the limbs of mammals, which would not apply to the condition of the crawling reptiles. The diminutive brain in these low and slow cold-bodied animals does not demand such protection against concussion; neither does the mode of locomotion in the quadruped reptiles render such concussion likely; their limbs sprawl outwards, and push along the body, which commonly trails upon the ground; therefore we find no epiphyses with interposed cartilage at the ends of a distinct shaft in the long bones of Saurians and Tortoises. But when the reptile moves by leaps, then the principle of ossifying the long bone by distinct centres again prevails, and the extremities of the humeri and femora long remain epiphyses in the frog.

"This distinction in the nature and relations of such centres, which is indispensable in the right application of the facts of osteogeny to the determination of the number of essentially distinct bones in any given skeleton, has never been considered, so far as I know, in that application. Some homologists have gone beyond Cuvier, and still more beyond nature, in arguing the number of individual bones, as indicated by the number of separate centres of ossification in the embryo, to be the same in all vertebrate animals; and that they afterwards differed, or seemed to differ, only by reason of the greater or less rapidity or extent of the confidence of those ossific centres or essentially distinct bones.

"This primitive conformity of separate osseous pieces in the vertebrate series holds good, however, only in regard to the separate centres of ossification of those bones of higher animals which have homological relations to the permanently distinct bones of lower species; it by no means applies to those which have merely teleological relations to the species in which they exist." P. 39.

"The great aim of the philosophical osteologist is to determine, by natural characters, the natural groups of bones of which a vertebrate skeleton typically consists; and, next, the relations of individual simple bones to each other in those primary groups, and to define the general, serial, and special homologies of each bone throughout the vertebrate series.

"By general homology I mean the relation in which a bone stands to the primary segment of the skeleton of which it is a part; thus, when the basi-occipital bone (basilar process of the os occipitis in anthropotomy) is said to be the centrum or body of the occipital or posterior cranial vertebra, its *general* homology is eunciated. When it is said to repeat in its vertebra, or to answer to the basi-sphenoid in the parietal vertebra, or to the body or centrum in the atlas, dentata, or any other of the vertebral segments of the skeleton, its *serial* homology is indicated; when the essential correspondence of the basilar process of the occipital bone in Man with the distinct bone called 'basi-occipital,' in a Crocodile or Fish is shown, its *special* homology is determined." P. 41.

We have preferred, by these extracts, giving these important views in the words of the author to attempting an abstract of them in words of our own.

The ground being, as it were, thus cleared before him, the author proceeds to propound his theory of the endo-skeleton of the vertebrata, and especially the theoretical structure of a vertebra. It will be readily imagined that it is in the lowest class of the great group in question that the teleological variations of structure are fewest and least considerable, and the homological most obviously and distinctly and consistently developed. It is, in fact, in the fishes that the illustrations of the theoretical vertebrate structure are most striking and simple. A knowledge of the further development of these elements in the higher and more perfect forms is, however, essential to the full comprehension of the theory, and although the permanently separate condition of most of the distinct centres

of ossification in this class will afford an important clue to the understanding of the true character of these homologues in the higher classes, yet it will be impossible to fully understand the relations of the various elements to each other without following them upwards to their most complicated and centralized condition in the mammals. It would not be easy to convey the real objects of the author in his description of a theoretical vertebra without reference to the woodcuts, or some such pictorial illustrations. We will, however, endeavour to afford some idea of the nomenclature employed by him, and of the normal structure of a vertebra according to Professor Owen's idea, by a comparative statement of the names now given to the parts of which a vertebra is essentially composed with those in ordinary use.

Mr. Owen's definition of a vertebra is, "*One of those segments of the endo-skeleton, which constitute the axis of the body, and the protecting canals of the nervous and vascular trunks;* such a segment may also support *diverging appendages.*

The essential parts of which a vertebra is theoretically composed are, according to Professor Owen, as follows:—"A body or centrum—two neurapophyses—two parapophyses—two pleurapophyses—two hæmapophyses—a neural spine—a hæmal spine."

"These, being usually developed from distinct and independent centres, I have termed 'autogenous' elements. Other parts, more properly called processes, which shoot out as continuations from some of the preceding elements, are termed 'exogenous:' the diapophyses, or upper 'transverse processes,' and the zygapophyses, or the 'oblique' or 'articular processes' of human anatomy.

"The autogenous process generally circumscribe holes about the centrum, which, in the chain of vertebræ, form canals. The most constant and extensive canal is that formed above the centrum, for the lodgment of the trunk of the nervous system (neural axis) by the parts thence termed 'neurapophyses.' The second canal, below the centrum, is in its entire extent more irregular and interrupted; it lodges the central organ and large trunks of the vascular system (hæmal axis,) and is usually formed by the laminæ, thence termed 'hæmapophyses.' At the sides of the centrum, most commonly in the cervical region, a canal is circumscribed by the pleurapophysis or costal process, and by the diapophysis or upper transverse process, which canal includes a vessel, and often also a nerve.

"Thus a typical or perfect vertebra, with all its elements, presents four canals or perforations about a common centre; such a vertebra we find in the thorax of man, and most of the higher classes of Vertebrata, also in the neck of many birds." P. 43.

We cannot enter into the details of all the modifications of these elements. They are elaborately given in the work before us. It is equally impossible to give the particulars of the variations in form which the vertebræ undergo in the class of fishes; to which part of his subject the author has devoted a large space. It is extremely probable that the general views here adverted to are correct; and that the details themselves will admit of but little objection or cavilling, as far as they refer to the more normal forms of vertebræ. But whether, in the present state of our knowledge, it is possible to follow our author into all the homologies which he believes himself to have demonstrated in the cephalic vertebræ, is another question, and one on which we are scarcely at present prepared to decide. The following sentence, indeed, at once shows the latitude which

Mr. Owen allows himself, in what may be termed the *morphology* of these parts. He is introducing the description of what he believes to be the "neural arches of the cranial vertebræ."

"The first series of endo-skeletal bones constitutes the axis or back-bone of the skull, as the rest of the vertebral neural arches to that of the trunk; and it includes and protects the encephalon or anterior expanded extremity of the great nervous axis. The under and upper parts of the annular segments are commonly formed by single and simmetrical bones, as in the vertebral axis of the trunk; but sometimes, even in the present low class, the expansion of the cranial cavity is accompanied, not only by a transverse development, but also by a median division of the upper piece or key-bone of one or more of the protecting arches." P. 89.

But now we have something like a law developed in the relation between these presumed "neutral arches" and the primary divisions of the encephalon.

"Though subject to various degrees of anchylosis, the cranial vertebræ always accord in number with the primary ganglions or divisions of the encephalon in Fishes. For the better understanding of this important relation, I may premise that the brain of Fishes consists of four primary divisions succeeding each other in a linear series horizontally, which, viewed from behind forwards, are:—

"1. The medulla oblongata, with the superimposed cerebellum, or the 'epencephalon.'

"2. The third ventricle, with its upper (pineal) and lower (hypophysial) prolongations, and the superimposed optic lobes, or the 'mesencephalon.'

"3. The cerebrum proper, or 'prosencephalon.'

"4. The olfactory ganglionic or chord-like prolongation of the cerebrum, or 'rhinencephalon.'" P. 89.

Now it must be confessed that, if this relation be consistently borne out by fact, it would of itself constitute a strong *à priori* argument in favour of the general theory.

In the exposition of these cranial vertebræ—which are of course confined to four, in consonance with the cerebral ganglia which they severally protect, and to which they stand in relation—Mr. Owen selects the Cod (*Gadus morrhua*) as an example affording the most complete development of the different elements. It is extremely difficult even to attempt any analysis of the long and elaborate details of this portion of the work; but we will endeavour to select the mere heads of each, which is as much as our space will admit of. Taking, therefore, the four "primary ganglia" of the encephalon as our argument or text, we find the first of these, the "epencephalon," is encompassed by what is termed the epencephalic arch of the occipital vertebra. The lowest of these, which answers to the *centrum* of the vertebræ, is the *basi-occipital*, which articulates with the atlas. The *ex-occipitals* form the *neurapophyses* of the vertebræ, and answer to the "occipital litéral" of Cuvier and Agassiz. The *neural spine* of the vertebræ is represented by the *supra-occipital*, answering to the "interpariétal" of Cuvier, &c., and the arch is completed by the representatives of the *par-apophyses*, the *par-occipitals* of Owen, the "occipital externe" of Cuvier.

"The second ring of bones, or that which encircles the mesencephalon," belongs to the parietal vertebræ—the *centrum* of which is the *basi-sphenoid* (sphénoide postérieur, "S. principal" C. and A.) The *neura-*

pophyses of this vertebra consist of the *ali-sphenoids;* ("Grande aile du sphénoïde," C.) The *spine* is considered to be homologically represented by the *parietals*,—ordinarily two in number,—and the *mastoid* bones are the *parapophyses* of this arch.

The frontal vertebra, the neural arch of which is the procencephalic, has for the representative of its *centrum*, the *pre-sphenoid* ("sphénoide principal," C.) Its *neurapophyses* are the *orbito-sphenoids* ("Aile orbitaire," C.) The *frontal* bone ("Frontal principal," C.) forms the *spine* of the vertebra, and the *post frontals* ("Frontal posterieur," C.) complete the ring as the representatives of the *parapophyses*.

The ring of bones which completes the axis of the cranium anteriorly and forms the nasal vertebra, consists of the *vomer* as the *centrum* of the vertebra; the *nasal* bone as its *spine*, and the *prefrontals* ("Frontal anterieur" C.) as the *neurapophyses*. This presumed vertebra is wanting, as it would appear, in parapophyses.

It will thus be seen that there are four vertebræ standing in relation to the four great cephalic ganglia or divisions. The neutral arch of the occipital vertebra is called the ex-encephalic arch, and protects the exencephalon; the neutral arch of the parietal vertebra is termed the mesencephalic from its relation to the mesencephalon;—that of the frontal vertebra is the prosencephalic, from its protecting the prosencephalon; and, finally, the arch of the nasal vertebra is in relation to the rhinencephalon or olfactory ganglion.

Omitting the consideration of the "sense capsules," we pass on to the completion of the cranial or cephalic vertebræ, in the inferior or Hæmal arches, as explained by Professor Owen. "These," says our author, "though apparently more numerous than the vertebral centres, correspond with them and the neural arches, and are essentially four in number, in the osseous fishes; viz. the 'palato-maxillary,' the 'tympano-mandibular,' the 'hyoidean,' and the 'scapular.' Most fishes have likewise appendages, which diverge or radiate from these arches. A special (visceral) system of bony arches, called 'branchial,' also persists in fishes, for the support and movement of the gills." P. 104.

In describing the details of the hæmal arches of the vertebræ, of which we have already attempted a brief analysis of the neural or superior arches, we find, in the present work, the order reversed; and we have to commence in the anterior part of the cranium, with the hæmal arch of the "nasal" vertebra. This is the *palato-maxillary* arch, and its formation is thus described:—

"I am induced to regard this as essentially one arch, from its condition in the Lepidosiren and Plagiostomous fishes, and from the circumstance of its being completed or closed at one point only, viz. where the premaxillaries meet or coalesce. The palatine bones are the piers of this inverted arch, and their points of suspension are their attachments to the prefrontals, the vomerine and the nasal bones. The arch is completed by the maxillary and premaxillary bones, the symphyses of the latter forming its apex; and it is inclined forwards, nearly or quite parallel with the base of the skull; which, in most fishes, extends to the apex of the arch, and in some far beyond it, being usually more or less closely attached to it. In air-breathing vertebrates the arch is more dependent, circumscribing below the nasal or respiratory canal. The pterygoid bones project backwards and outwards as the appendages of the palato-maxillary arch. Both maxillary and

inter-maxillary bones tend by their peculiar development and independent movement in bony fishes to project freely outwards, downwards, and backwards. We find, at least, that the general form, position, and attachments of the single and simple palato-maxillary arch, in the *Lapidosiren* or *Cestracion* are represented in most osseous fishes, by their several detached bones, the names of which have been just mentioned." P. 105.

"The *diverging appendage* of the palato-maxillary arch consists, in fishes, of the pterygoid and ento-pterygoid bones; which, as they are the least important parts of the arch, so are they the least constant." The following sentence shows well the ingenuity and acumen with which Mr. Owen solves difficulties which few men would be disposed to grapple with.

"The ten bones of which the palato-maxillary arch is composed in Osseous Fishes are, in the Cod and most other species, so dispersed, in relation to the peculiar movements of the mouth, as to appear like three parallel and independent arches, successively attached behind one another, by their keystones, to the fore part of the axis of the skull, and with their piers or crura suspended freely downwards and outwards, except those of the last or pterygo-palatine arch, which abut against the tympanic pedicles. The simplification or confluence of the two first of these spurious arches is effected in the Salmonoid Fishes, by the shortening of the premaxillary, and by the mode of its attachment to the maxillary, which now forms the larger part of the border of the mouth and supports teeth: the maxillaries are brought into close articulation with the palatines in the Plectognathes, and the consolidation of the whole series into its normal unity is effected in the Lepidosiren. The palatines form the true bases of the inverted arch at their points of attachment to the prefrontals; the intermaxillaries constitute the true apex, at their mutual junction or symphysis; the approximation of, which to the anterior end of the axis of the skull is rendered possible in fishes by the absence of any air-passage or nasal canal; the pterygoids are the diverging appendages of the arch; but are attached posteriorly to strengthen the pedicle supporting the lower jaw, and combine its movements with those of the upper jaw; just as the bony appendages of one costal arch in Birds associate its movements with those of the next." P. 110.

The *tympano-mandibular* arch forming the hæmal arch of the frontal vertebra, consists of the *epi-tympanic*, the *meso-tympanic*, the *pre-tympanic*, the *hypo-tympanic*, and the *mandible* or lower jaw; and the *diverging appendage* of this arch "consists of the bones which support the gill-cover, a kind of broad and short fin, the movements of which regulate the passage of the currents through the branchial cavity." These bones are known as the *pre-opercular*, the *opercular*, the *sub-opercular*, and the *inter-opercular*.

The *Hyoidean* arch is regarded as the costal or hæmal arch of the *parietal* vertebra. It is composed of the *stylo-hyal* as the pleurapophysis, the *epi-hyal* and *cerato-hyal* as the hæmapophysis, and the keystone or body of the arch is formed by the *basi-hyals* together with the *glosso-hyal* and *uro-hyal* where these exist. The *diverging appendages* to this arch are the branchiostegous rays attached to the *epi* and *cerato-hyals*.

Those remarkable bony appendages the branchial arches succeed the hyoidean arch, with the key-stone of which they are connected.

The remaining inverted cranial arch, the *scapular*, forms the *hæmal* arch of the occipital vertebra. It is formed by the *suprascapula*, the *scapula*, and the *coracoid*; and its diverging appendage is thus described:

"This appendage consists in Lepidosiren of a single ray; but in Osseous Fishes it is composed usually, first, of two, rarely of three, bones immediately articulated with the coracoid; next, of a series of from two to six smaller bones; which, lastly, support a series of spines or jointed rays. These rays in the scapular appendage, or 'pectoral fin,' are a repetition of the branchiostegal rays in the hyoidean appendage, and of the opercular rays in the tympanic appendage. Of the special homology of the pectoral fin-rays with the digits of the pectoral extremity in higher animals there has been no question. The vegetative repetition of digits and joints, and the vegetative sameness of form in those multiplied peripheral parts of the fins of fishes, accord with the characters of all other organs on their first introduction into the animal series. The single row of fewer ossicles supporting the rays, obviously represents the double carpal series in Mammala; and the bones of the brachium and anti-brachium seem in like manner to be reduced to a single series, unless the humeral segment be confluent with the arch. In the ventral fin no segment is developed between the arch and the digital rays; it is in this respect like the branchiostegal fin." P. 120.

We regret that our limits do not permit our giving an analysis of the author's elaborate exposition of the homologies of the other members in fishes; but we cannot omit his observations on the relations and homologies of the pectoral fin, because they are not only highly interesting in themselves, but they further contribute to the development of those general views and theories which we are now endeavouring to introduce to our readers.

"The special homology of the pectoral fins of fishes with the fore limbs of quadrupeds was indicated by Aristotle, and first definitely pointed out in later times by Artedi, in 1735, who says—'Ossa pectoris et ventris in piscibus reperiuntur; suntque in piscibus spinosis: 1. Claviculæ; 2. Sternum; 3. Scapulæ, seu ossa quibus pinnæ pectorales ad radicem affiguntur.' (*Partes Piscium*, p. 39.) Geoffroy St. Hilaire, who has devoted special Memoirs to the determination of the bones of the pectoral fin, had no knowledge of the primary homology of the pectoral fin as the radiated appendage of the inferior arch of a cranial vertebra, or of its serial homology with the branchiostegal and opercular fins. He consequently speaks of the junction of the basis of the fin to the cranium as something very strange:—'Disposition veritablement très singulière, et que le manque absolu de cou, et une combinaison des pieces du sternum avec celles de la tête pouvoient seuls rendre possible.' (*Annales du Musèum*, IX. p. 361.)

"Owen's latest idea of the essential nature of the arms and legs is, that they are no other than 'liberated ribs.' 'Freye Bewegungsorgane können nichts anderes als frey gewordene Rippen seyn.' *Lehrbuch der Natur Phiosophie*, p. 330. 8vo, 1843.)

Carus (I.), in his ingenious endeavours to gain a view of the primary homologies of the locomotive members, sees in their several joints repetitions of vertebral bodies (*tertiar-werbel*)—vertebræ of the third degree—a result of an ultimate analysis of a skeleton pushed to the extent of the term 'vertebra' being made to signify little more than what an ordinary anatomist would call a 'bone.'

"But these transcendental analysis sublimate all differences, and definite knowledge of a part escapes through the unwarrantable extension of the meaning of terms. We have seen, however, that a vertebra is a natural group of bones, that it may be recognized as a primary division or segment of the endoskeleton, and that the parts of that group are definable and recognizable under all their teleological modifications, their essential relations and characters appearing through every adaptive mask.

"According to the definition of which a vertebra has seemed to me to be susceptible, we recognize the centrum, the upper (neural) arch, the lower (hæmal)

arch, and the appendages, diverging or radiating from the hæmal arch. The centrum, though the basis, is not less a part of the vertebra, than are the neurapophyses, hæmapophyses, pleurapophyses, &c.; and each of these parts is a different part from the other: to call all these parts 'vetebræ' is in effect to deny their differential and subordinate characters, and to voluntarily abdicate the power of appreciating and expressing them. The terms 'secondary' or 'tertiary vertebræ' cannot, therefore, be correctly applied to the appendages of that natural segment of the endo-skeleton to which the term 'vertebra' ought to be restricted.

"So likewise the term 'rib' may be given to each moiety of the hæmal arch of a vertebra; although I would restrict it to that part of such arch to which the term 'vertebral rib' is applied in Comparative Anatomy and the term 'pars ossea costæ' in Anthropotomy: but, admitting the wider application of the term 'rib' to the whole hæmal arch, yet the bony diverging and backward projecting appendage of such rib or arch is something different from the part supporting it. Arms and legs may be developments of costal appendages, but cannot be ribs themselves liberated; although liberated ribs may perform analogous functions, as in the Serpents and Dragons.

"A series of developments may be traced from the primitive form of the appendage, as a simple plate, spine, or ray, through the many-jointed single ray in the Lepidosiren and the bifurcate jointed ray in the *Amphiuma didactylum*, up to the wing of the Bird and the arm of the Man, without the essential nature of the part being lost sight of; of all these forms of the pectoral member are, in their ultimate or general homology, 'diverging' or 'radiated appendages' of a hæmal arch; but not 'ribs,' nor 'vertebræ.' We may further define the fore-limb, wing, or pectoral fin to be the radiated appendage of the arch called 'scapular,' and this to be the 'hæmal arch of the occipital vertebra.'" P. 126.

In this exposition of the vertebral theory of Professor Owen, it will be seen that considerable support is derived from the simple homologues exhibited in the structure of the *Lipidosiren*, which is here taken for granted to belong to the class of *fishes*. It has always, we confess, appeared to us doubtful whether the reasons elsewhere assigned by Professor Owen for this allocation of the interesting animal in question, be the true one. We have not now space to enter into an elaborate consideration of this point; but we believe we may with less reluctance dispense with the discussion, as it appears from the recent investigations of Mr. John Quekett, on the intimate structure of bone in different classes of the vertebrata, that the *Lepidosiren* is actually a *reptile*, and not a fish. This view is also borne out by many facts in its natural history, and, as we believe, also by its anatomical structure. As far, therefore, as the support derived from this source to the present theories would go, we fear it must be wholly given up.

We have now endeavoured to exhibit, as succinctly as possible, an outline of Professor Owen's views on the homologues of the vertebræ, as exemplified in the osseous fishes, in which the cranial elements are developed in so distinct a manner as to afford the best means of exhibiting their theoretical tendencies. The modifications of these elements as they exist in the so-called cartilaginous fishes, and particularly in the Sturgeon, are extremely interesting, as showing them in one continuous cartilaginous structure; but it is not necessary here to follow out the elaborate statement of the homological relation of the different portions of this mass of cartilage with the separate elements which we have just enumerated as existing in the Cod. The dermal osteology of the Sturgeon is adduced as illustrative of

the author's views, and the median dermal bony plates are considered as "obviously homologous with the dermal bones forming the helmet of the Armadillo, and as bearing the same relation in the Sturgeon to the cartilaginous skull as those bones do in the Armadillo to the osseous skull beneath."

It is not our intention to enter at present into the consideration of the portions of this elaborate and really valuable work, as our principal object has been to present to our readers some account of the author's theories respecting the homologues of the vertebræ. From even this sketch, we believe it will be seen that it is in the logical exercise of the reasoning faculty that Professor Owen's great strength lies; and although here and there we cannot help fearing that his disposition to theorize may carry him into the mazes of hypothesis, yet, there is so much of sound induction, such clearness in the mode of putting his argument, and so much of the dignity of truth in his conclusions, that no one can rise from the perusal of his works without the conviction that he has been communing with a master-spirit.

The foregoing observations are more particularly borne out in the present work, by that portion of the sixth Lecture which relates to the "Teleology of the Skeleton of the Fishes;" and we regret that we cannot present this admirable example of inductive reasoning and eloquent discourse in Natural Theology—for such it may be truly termed—in its completeness. We have no hesitation in saying that the whole of this chapter or at least that part of it to which we are now referring, exhibits such a combination of acute reasoning, of close observation, and of appropriate and eloquent diction, as is rarely to be met with within the range of scientific literature. The value of every fact is so carefully weighed, its true bearing upon the question is so accurately defined, and the author's theory brought so irresistibly to bear upon the difficulties which former writers have in vain attempted to solve, that scarcely any thing seems to be wanting to render it a perfect example of scientific discussion. We offer a few short passages from it, and with these we conclude our present notice of a work which must be acknowledged fully to confirm the justice of the author's high and extended fame.

"To determine the parts of the Vertebrate skeletons which are most constant; to trace their general, serial, and special homologies, under all the various modifications by which they are adapted to the several modes and spheres and grades of existence of the different species, should be the great aim of osteological science; as being that which will reduce its facts to the most natural order, and their exposition to the simplest expressions. It is impossible, in pursuing the requisite comparisons upwards through the higher organized classes, not to recognize the close and interesting analogies between the mature states and forms of ichthyic organs, and the embryonic condition of the same parts, in the higher species. But these analogies have been frequently overstated, or presented under unqualified metaphorical expressions, calculated to mislead the student and to obstruct the attainment of true conceptions of their nature. We should lose some most valuable fruits of anatomical study were we to limit the application of its facts to the elucidation of the unity of the Vertebrate type of organization, or if we were to rest satisfied with the detection of the analogies between the embryos of higher and the adults of lower species in the scale of being. We must go further, and in a different directoin, to gain a view of the beautiful and fruitful physiological principle of the relation of each adaptation to its appro-

priate function, and if we would avoid the danger of mistaking analogy for homology or identity, and of attributing to inadequate hypothetical secondary causes manifestations of Design, of supreme Wisdom and Beneficence, which the various forms of the Animal Creation offer to our contemplation." P. 146.

"Yet there are some who would shut out by easily comprehended but quite gratuitous systems of progressive transmutation and self-creative forces, the soul-expanding appreciations of the final purposes of the fecund varieties of the animal structures by which we are drawn nearer the great First Cause. They see nothing more in this modification of the skeleton, which is so beautifully adapted to the exigencies of the highest organized of fishes, then a foreshowing of the cartilaginous condition of the reptilian embryo in an enormous tadpole, arrested at an incomplete stage of typical development. But they have been deceived by the common name given to the Plagiostomous fishes: the anima basis of the shark's skeleton is not cartilage; it is not that consolidated jelly which forms the basis of the bones of higher Vertebrates: it has more resemblance to mucus; it requires 1000 times its weight of boiling water for its solution, and is neither precipitated by infusion of galls, nor yields any gelatine upon evaporation.

"In like manner the modifications of the dermal skeleton of fishes have been viewed too exclusively in a retrospective relation with the prevalent character of the skeleton of the Invertebrate animals. Doubtless it is in the lowest class of Vertebrata that the examples of great and exclusive development of the exoskeleton are most numerous; but some anatomists, in their zeal to trace the serial progression of animal forms, seem to have lost sight of all the vertebrate instances of the bony dermal skeleton except those presented by the Ganoid and Placoid fishes. He must have sunk to the low conception that nature had been limited to a certain allowance of the salts of lime in the formation of each animal's skeleton, who could affirm that in the higher Vertebrata 'the internal articulated skeleton takes all the earthy matter for its consolidation,' forgetting that the bulky Glyptodon and its diminutive congeners the Armadillos have their internal skeleton as fully developed and as completely ossified as in any other mammals. The organizing energies which perfect and stengthen the osseous internal skeleton do not destroy nor in any degree diminish the tendency to calcareous depositions on the surface, when the habits and sphere of life of the warm-blooded quadruped require a strong defensive covering from that source." P. 148.

"All writers on Animal Mechanics have shown how admirably the whole form of the fish is adapted to the element in which it lives and moves; the viscera are packed in a small compass, in a cavity brought forwards close to the head; and whilst the consequent abrogation of the neck gives the advantage of a more fixed and resisting connection of the head to the trunk, a greater proportion of the trunk behind is left free for the development and allocation of the muscular masses which are to move the tail. In the caudal, which is usually the longest, portion of the trunk, transverse processes cease to be developed, whilst dermal and intercalary spines shoot out from the middle line above and below, and give the vertically extended, compressed form, most efficient for the lateral strokes, by the rapid alternation of which the fish is propelled forwards in the diagonal, between the direction of those forces. The advantage of the bi-concave form of vertebra with intervening elastic capsules of gelatinous fluid, in effecting a combination of the resilient with the muscular power, is still more obvious in the Bony Fishes than in the Shark.

"You may be reminded that all the vertebræ of the trunk are distinct from one another at one stage of the quadruped's development, as in the fish throughout life; and you might suppose that the absence of that development and confluence of certain vertebræ near the tail, to form a sacrum, was a mark of inferiority in fishes. But note what a hindrance such a fettering of the movements of the

caudal vertebræ would be to creatures which progress by alternate vigorous inflections of a muscular tail! A sacrum is a consolidation of a greater or less proportion of the vertebral axis of the body for the transference of more or less of the weight of the body upon limbs organized for its support on dry land; such a modification would have been useless to the fish, and not only useless, but a hindrance and a defect.

"The pectoral fins, those curtailed prototypes of the fore-limbs of other Vertebrata, with the last segment, or hand, alone projecting freely from the trunk, and swathed in a common undivided tegumentary sheath, present a condition analogous to that of the embryo buds of the homologous members in the higher Vertebrata. But what would have been the effect if both arm and fore-arm had also extended freely fromt he side of the fish, and dangled as a long flexible many-jointed appendage in the water? This higher development, as it is termed, in relation to the prehensile limb of the denizen of dry land, would have been an imperfection in the structure of the creature which is to cleave the liquid element: in it, therefore, the fore-limb is reduced to the smallest proportions consistent with its required functions: the brachial and antibrachial segments are abrogated, or hidden in the trunk: he hand alone projects and can be applied, when the fish darts forwards, prone and flat, by flexion of the wrist, to the side of the trunk; or it may be extended at right angles, with its flat surfaces turned forwards and backwards, so as to check and arrest more or less suddenly, according to its degree of extension, the progress of the fish; its breadth may also be diminished or increased by approximating or divaricating the rays. In the act of flexion, the fin slightly rotates, and gives an oblique stroke to the water. For these functions, however, the hand requires as much extra development in breadth, as reduction in length and thickness; and mark how this is given to the so-called embryo or rudimental fore-limb: it is gained by the addition of ten, twenty, or it may be even a hundred digital rays, beyond the number to which the fingers are restricted, in the hand of the higher classes of Vertebrata. We find, moreover, as numerous and striking modifications of the pectoral fins, in adjustment to the peculiar habits of the species in fishes, as we do of the fore-limbs in any of the higher classes. This fin may wield a formidable and special weapon of offence, as in many Siluroid fishes. But the modified hands have a more constant secondary office, that of touch, and are applied to ascertain the nature of surrounding objects, and particularly the character of the bottom of the water in which the fish may live. You may witness the tactile action of the pectoral fins when gold fish are transferred to a strange vessel: their eyes are so placed as to prevent their seeing what is below them; so they compress their air-bladder, and allow themselves to sink near the bottom; which they sweep as it were, by rapid and delicate vibrations of the pectoral fins, apparently ascertaining that no sharp stone or stick projects upwards, which might injure them in their rapid movements round their prison. If the pectorals are to perform a special office of exploration certain digits are liberated from the web, and are specially endowed with nervous power for a finer sense of touch, as we see in the gurnards, where they compensate for the loss of the tactile property consequent on the hard covering of the exterior of the mouth in these mail-checked fishes." P. 155.

LETTRES DE GUI PATIN. Nouvelle Edition augmentée de Lettres Inédites et precedée d'une Notice Biographique. Par *J. H. Reveillé-Parise*, Docteur en Medecine, &c.

Letters of Guy Patin. A new Edition, augmented by Inedited Letters, and preceded by a Biographical Notice. By *J. H, Reveillé-Parise*, M.D. Three volumes, 8vo, pp. 1974. Paris, 1846.

GUY PATIN may claim a foremost place in a department of literature—the difficult art of letter-writing—in which his countrymen have attained an acknowledged superiority; and the only regret we feel at the conclusion of our perusal of the portion of his voluminous correspondence here presented to us by M. Parise, arises from the fact of its termination having been reached, and our conviction that, take what pains we may, we can only present our readers with a very inadequate idea of its interesting characteristics. Translation may be undertaken by the practised hand in the case of didactic or narrative literature with the assurance of an accurate, and even an elegant, execution; but when vivid pictures of men and manners, dashed off in a few lines of happy but idiomatic phraseology, are attempted to be thus transferred, they are apt to lose all their original brilliancy, and even become distorted by the different refracting power of the medium of transmission. Thus it is that attempts at the adequate rendering of the various celebrated French letter-writers have always proved marked failures—washy pointless productions being substituted for the elegance and vivacity of the originals. We may, however, indicate to our readers what they will find by referring themselves to these volumes, and can assure them that the various scenes therein depicted by the ever-active writer are endowed with a life-like energy that at once transports us to the scene of action, mingles us with the throng of events, forcibly impresses the imagination, and conveys a more accurate idea of the manners of the epoch than volume upon volume of history written for the express purpose could impart.

Guy Patin was born, in 1601, in the province of Picardy. His father was an Advocate, but, being tempted by the inducements held out by the Seigneur of his village, he left Paris, where he was endeavouring to acquire practice, and established himself as a kind of steward or superintendent of that gentleman's estate. The promises which had been made him not having been fulfilled, his circumstances always continued very limited. Guy often refers to the excellent moral character his father bore with becoming pride. "My father," says he, "was a good man if ever there was one. If every one was like him we should have little need of lawyers. He came to Paris annually on his master's business, and his credit was always excellent. I found, on my arrival there, numbers of friends whom I knew nothing of, but who loaded me with kindness on his account, which has made me many a time deplore his loss more and more." His father gave him a good preliminary classical education, and as soon as he had left college, some of the nobility of the district, who had become indebted to

his father, and were desirous of paying him by what cost them nothing, offered him a living for his son: but Guy, with a spirit that seemed to foreshadow his future energetic opinions upon the subject, "flatly refused it, protesting absolutely I would never become a priest; blessed be God for so impressing my mind at so early an age. My father recognising in this refusal something good and ingenuous, was in no-wise irritated by it; but my mother continued exasperated against me for more than five years." Referring to the subject some forty years after, in a letter to a friend, he says, "I have oftentimes praised God that he never made me either a woman, a priest, a Turk or a Jew."

The anger of his mother caused him to leave his home, and he came to Paris to continue his studies. What his resources were he mentions not, but Bayle tells us he supported himself as a corrector of the press, Drelincourt, Professor of Medecine at Leyden, having taught him this. "We must not be surprised," says M. Parise, "at Patin choosing such a profession to gain a livelihood. At this period, as well as in the preceding century, it was the occupation of many distinguished men of letters, and especially of Erasmus and his friend Budæus. It sometimes happened that a philosopher printed in the morning that which he had written the night before." While so occupied, however, he fell in with the celebrated Riolan, who induced him to study medicine, and in 1627 he received his Doctor's degree, which effectually appeased the ire of his mother. He soon acquired reputation and practice, and having married a rich wife was henceforth placed at his ease in regard to pecuniary concerns. His practice also rapidly augmented, and Riolan, having quarrelled with his son, obtained for Patin the reversion of the professor's chair he held in the college of France, to which he did not succeed however until 1654

He was not the man to vegetate in wealthy ease, or to pursue his profession as a mere lucrative calling. On the contrary, his activity was prodigious and usefully directed in the main. As was the custom of the period, he entered into a voluminous epistolary correspondence with the learned in various parts of Europe. He accumulated a large library of valuable books, which he not only read, but commented upon with great critical acumen. He delivered lectures to crowded auditories, and joined in or presided over the disputation of theses with a zest and erudition which even the learning of that epoch exhibited few examples of. Thoroughly imbued with the conviction of the dignity of the medical art, and of the superiority of the scholastic and Galenical mode of acquiring and practising it, he was in continual war with the charlatans, the chemists, the apothecaries, the barber-surgeons, and the hosts of unqualified practitioners of the day. A warm sympathizer in the cruel sufferings the masses of his countrymen were subjected to by the oppressions of the priesthood and noblesse, and by the corruption which pervaded all society from the court downwards, many of his letters are occupied in stigmatizing the authors and causes of these, evincing an amount of information which the most active intercourse with general society could alone furnish him with. Notwithstanding his extensive town-practice, and his being frequently called away from Paris (journies he always bitterly complains of as tearing him away from his beloved books), he could still find time to direct his son's or his pupils' studies, to expound some passage of Galen or Fernel to them,

and to endeavour to impress upon them the elevated objects and the responsibilities of the profession they had chosen.

He was a man of strong feelings and prejudices, and the manners of his times permitted the giving currency to these in terms which the more subdued temper of our own has properly discountenanced. A withering sarcasm, a great facility of expression, whether in Latin or French, an utter carelessness of wounding the feelings of those attacked, must have rendered him indeed a formidable enemy; and nothing can be happier than many of his sarcastic allusions and cutting epigrams. But if he was a fierce opponent, no mean or personal considerations prompted his enmity. He neither manifests anxiety for lucre or jealousy at the legitimate success of brother-practitioners. On the contrary, he refused appointments which would have torn him from his books and other associations, and mentions with eager delight the instances of good fortune which befel his friends and colleagues. The bitter reviler of Cardinal Mazarin, and the Jesuits, the harsh antagonist of the lower grades of the profession in their laudable efforts to raise their position, the cordial hater of the chemists and charlatans of the day, expresses with admirable pathos his sympathy for the miseries under which his country groaned, was a doating though not a foolish father, and could drop a tear when a beloved colleague or favourite author left this mortal scene. His friendships once formed seemed immutable by time and circumstances, and so acutely did he feel the death of Grotius that he at once fell ill, and was not enabled to resume his duties for several days.

But the most admirable trait in Guy Patin's character was his noble frankness and love of truth, and detestation of hypocrisy and meanness of every description. That several of his views and prejudices were unfounded and exaggerated, events have proved; but no doubt as to the purity of his motives, the uprightness of his conduct, will ever be entertained. It will be believed that qualities such as we have mentioned, procured for him an abundant supply of enemies. He was well aware of this, and, full of courage and confidence in his cause, was ever ready for the combat, and never contented with taking the mere defensive. But so likewise the goodness of his heart, his upright character, his active disposition, learned eloquence, and great practical knowledge insured him numbers of attached friends and a vast reputation, which continued long after his decease. Unfortunately, beyond a few theses and ephemeral publications, he issued nothing from the press, so that the opinion posterity will form of him must rest upon his letters, several collections of which, addressed to medical men and literati, have been published, but none having the least pretension to completeness or accuracy, until the present one, brought out under the able superintendence of M. Reveillé-Parise. Guy Patin continued his active career until 1672, when he died, having undergone much vexation in his latter years from the ingratitude of his elder son, the exile, for reasons he never could divine, of his younger and favourite one, and from the rapid progress which the chemical practitioners made.

To a few of the multifarious subjects embraced in his correspondence we may now direct attention, and first to the writer's ideas upon the practice of physic. These were eminently scholastic. Galen is his deity, and Fernel, as the ablest expounder of Galen, his arch-priest. By this test must every doctrine be examined and every practice measured. Although,

certainly, the Galenical views and his own sound sense sometimes conducted our author to modes of treatment which would be approved of even in our own day, yet the manifest advance we have made will be at once seen by those who consult his book. He made little or no use of drugs, save purgatives, and sternly prohibited the use of opium, cinchona, or antimony, then beginning to be introduced, under any circumstances whatever. Although confiding much in nature's powers, he by no means stood by as an idle spectator, for, if he declined interfering with her operations by means of the materia medica, he was by no means chary in the employment of the lancet. Perhaps he is the most daring phlebotomist on record, glorying in the victories he achieved, and regarding those who objected to his practice as rash with the greatest contempt. Here are a few of the innumerable passages in his letters illustrating this point.

"He died in the 48th year of his age of a continuous fever of twelve days duration, for which only two small bleedings were performed. So much for bleeding in Italy! About that time my son was also very ill; but I rescued him from a bad condition of continued fever into which he had unfortunately fallen, *quia adolescentuli semper stultè agunt*, by means of *twenty* free bleedings from the arms and feet, with at least a dozen good purgations with cassia, senna, and syrup of pale roses, without ever using your bezoards, juleps, cordials, or confections of hyacinth, or alkermes; and yet God has preserved him to me, in such a manner that he has not even lost a single lecture. * * * * * Our Paris people usually take but little exercise, drink and eat a great deal, and become very plethoric; and in this condition they are never relieved of any disease that may attack them, if bleeding is not powerfully and copiously first resorted to; and yet, if it not an acute disease, the effects are not so soon visible as from purgation. About the year 1633, M. Cousinot, now first Physician of the King, was attacked by a severe and violent rheumatism, for which he was bled *sixty-four* times in eight months, by order of his father and M. Bouvard. After being so often bled, they began to purge him, by which he was much relieved, and at last cured. The idiots who know nothing of our art, imagine that to purge is sufficient: but they are much mistaken; for if bleeding has not preceded, to repress the impetuosity of the wandering humour, to empty the large vessels, and correct the intemperature of the liver, which produces this serosity, purging could not prove useful. I have heard him say himself that the bleeding would have alone cured him, and that without it the purging would have been of no avail. Formerly, I was called to a young gentleman of this place seven years of age, who fell ill of an acute pleurisy. His guardian disliked bleeding very much, and I could only oppose his hatred of it by a piece of good advice, that he should call in two of our seniors, MM. Seguin and Cousinot. The lad was bled *thirteen* times and cured as if by a charm, so that the guardian himself was converted. * * * * * * * *

Since I last wrote to you, I have been seized with so horrible a cold that I have been obliged to quit every thing and go to bed, where I have been bled *seven* times. God be praised I am now rid of it, and only want strength. * * * * * * * To prevent the marks of the small-pox, we employ here tepid almond oil. But I think the best remedy is bold venesection from the commencement of the disease *ad cotemporandum fervorem et extinguendam acrimoniam sanguinis exuberantis ex utráque basilica*, and steeping, during the first twelve days, the eyes and the face *ex aqua optima tepida; qualem hic habemus sequanicam*, so as to procure the evaporation of his malignant humour imprisoned under the skin. * * * * * *

I have just been reading something of your Sennertus; I am quite in a rage: first, on account of the great number of faults there is, and secondly, because

this good man was new at, and understood little of, practice. He knows nothing about bleeding old persons and children; for see what he says at p. 616 of his first volume. This miserable example excites my pity. I think the poor man has scarcely ever seen any patients, and that *nullus fuit in praxi saltem admodum indiguit Delio Natatore.* If we acted thus at Paris all our patients would quickly die. We cure patients more than eighty years old by bleeding, and bleed, just as successfully and without any ill effects, children two or three months old. I could point out at least two hundred persons living here who were bled in their infancy. I think the patients in Germany are much to be pitied with such physicians, who have indeed only the names of the qualities they bear, and who, understanding nothing of remedies or method, seek for the secrets of chemistry in Paracelsus and Crollius, who were never physicians at all. A day does not pass in Paris in which we do not bleed several infants at the breast and septuagenarians *qui singuli feliciter inde convalescunt.* There is not a woman in Paris who does not believe in its efficacy, and who would refuse to allow her infant to be bled for the fever of small-pox or measles, convulsions, or teething, so convinced have they become by experience."

Patin explains in many places that it is not blood which is drawn from the veins on these numerous occasions, but mere "matter," "sanies," or "corrupted humours." Energetic, however, as he was in wielding the lancet and in the subsequent employment of purgatives, the various active remedies then becoming introduced into practice were met with his absolute prohibition and utter aversion—the very name of "specific" exciting his indignation and contempt. The chemists, following in the footsteps of Basil Valentine and Paracelsus, employed the most energetic endeavours to procure the admission of some of the various mineral preparations, especially of mercury and antimony, into the materia medica; but to this the Galenical physicians opposed the stoutest resistance, and were long successful in their opposition, especially in France. In our own country, Sir Theodore Mayerne, alike skilled in Galenical lore and in chemical knowledge, contributed much to soften animosities and prepare the way for innovations; but in Paris the bitterest contentions prevailed, and no form of speech was deemed too harsh for employment, and no imputation too odious for assertion. Guy Patin repeatedly refers with pride to two solemn decrees made by the Faculty of medicine in 1566 and 1615 against *Antimony* as a virulent poison, decrees which were afterwards sanctioned by the Parliament; and although, towards the latter part of his career, great numbers of the doctors of the Faculty had gone over to the chemists, he could never persuade himself that this was the case, until at last the opponents of the Galenists were strong enough to obtain, in 1666, a formal reversal of the obnoxious decrees. Nothing could convert Patin, his disbelief in the virtues of the medicine being as complete, and his diatribes against those who employed it as virulent from first to last. There can be no doubt that the use of antimony, at first administered by the hands of charlatans and in considerable doses, was frequently followed by the evil effects he represents as always attending its employment; and even in the hands of those of the regular physicians who employed it, it seems frequently to have induced death. Any one of the Faculty venturing to recommend, or even sanction, its administration, was at once degraded in Patin's eyes to the level of the lowest charlatan, and stigmatized accordingly. Speaking of one of these named Guénaut, Patin says:

"Antimony is here much decried. Guénaut's third daughter was buried on the 18th. She died in her confinement of her second child. Her hangman of a father is so wicked, that in this last illness of hers he ordered her to take the emetic wine six times. I think the man is mad, or is possessed of the devil. Most families are complaining of this poison, and yet Guénaut and some others are only the more determined to give it, declaring in mockery, 'Come, it's not so bad as they say: if it's not good for those who take it, it is at least so for their heirs.' Thus do they play with the lives of men, and yet impunity prevails everywhere. May God protect us from such drugs and such physicians!"

In other places he says:—

"I send you the epigram M. Augier has written upon the 'Triumph of Antimony.' It is approved of by everybody, as antimony is detested by all honest persons, having only on its side charlatans, empirics, apothecaries and such like rabble. * * * Observe how Messieurs the Antimonians play with the lives of men, and impudently send their poor patients into the other world with their poison, under the pretence of having particular secret remedies, the very terms of charlatans, *a quibus decipiuntur idiotæ tam togati quam tunicati.* The great desire to be deceived, and the little cannot help being so. * * * The ministers and Mazarin are the demons of France, the Turk of Christianity. The chemists, apothecaries, and charlatans are, in their kind, the demons of the human race. The pretended demon of the infernal regions kills not so many as does this chemical demon and chemical poison. * * * * * The wife of Meyssonier is dead then from the emetic wine! This poison seems to be doing its work at Lyons as well as Paris. Some of your doctors too have given it to their wives, who will never take it again, seeing that, by the grace of God, they are all dead; and some of them here have taken younger ones in their places. Guénaut has had his family in mourning three times, and has killed so many in different places that he now nowhere dares propose its administration."

But, alas! poor Patin. Antimony pursued its triumphant career, in spite of no one, as you say, daring to give it, and the young king, Louis XIV., falling ill at Calais, it was administered to him—and with success. An empiric was brought expressly from Abbeville, who, seating himself on the king's bed, exclaimed, "This lad is very ill, but die he will not," and in a short time he was completely cured. Patin utterly denies that antimony exerted any agency here, and then consoles himself and his friends with retailing the epigrams made on the great delivering themselves into the hands of quacks and imposters. Even hexameters and pentameters stigmatizing *Stibii noxæ vindiciæ* were freely circulated. The chemists were by no means disposed to remain idle, and met their opponents upon their own field, after the example of Paracelsus, with pen in hand, and nothing can exceed the indignation of Patin and his brethren at the treatment they received in the Legend of Antimony and Antimony Triumphant. Even persons unconnected with medicine mingled in the fray, and a monk named Carneau composed a burlesque poem upon the war waging between the rival practitioners, which M. Parise thinks may have suggested to Garth his, in every way superior one, entitled the Dispensary. Here is Patin's amusing version of the king's illness.

"The king is a well-made, strong, and tall prince, twenty years of age, who has never drank or injured his frame by any kind of debauchery. His disease

was but an excess of heat from having ridden too long in a burning sun, which, according to Galen, is one of the most powerful external causes of disease; assisted by the infected air of the marshy districts in which the army is encamped. It was a putrid continued fever, which only required bleeding and a refreshing diet, with slight purgatives. * * * * *

Three purgative apozemes, composed of cassia and senna, were prepared. The Cardinal inquired if there was anything unusual to be put into them. Esprit, physician to the Duke of Anjou, said some of the emetic wine might be added. Pretty politics these of our times! The physician of the next heir and immediate successor to the Crown called into consultation, and daring to prescribe his stibial poison. If his advice had been followed and the king had died, his master would have become king, and he the king's first physician. *Non sic erat in principio.* Formerly the physicians of the princes of the blood were never called in to a sick king, for very good political reasons; but now-a-days everything is reversed. Guenaut stated that the emetic was in no-wise to be feared if only a little were used, and Mazarin therefore directed it to be given, an ounce of the wine being placed in three doses. The king took one of these, which in two hours began to act, and in the course of the day he went twenty-two times to stool, which fatigued him much. In the evening, his fever was much increased; he passed a very bad night, and early in the morning they were obliged to bleed him, and regretted having given the antimony, for if matters had become still worse from it they would not have failed to have suffered. The king was again bled twice, and then re-purged, after which he felt better. So foolish is it then to say that the emetic wine saved him, since he took as little as could be; and no other medicine would he take until they swore to him it contained none of it, so much did he detest it. What saved the king was his innocence, his age and strength, nine good bleedings, and the prayers of *worthy people like us*, and especially of the courtiers and officers, who would have been terribly afflicted at his death, particularly Mazarin." P. 91.

We see, by the above extracts, Patin, as indeed was the custom at the epoch in which he flourished, dealt in the most odious imputations. His phraseology was not always very choice, and thus the terms, impostors, charlatans, stibial torturers and executioners, &c., are of constant occurrence. "Chemistry," he says in one letter, "is the false coinage of our art," and on several occasions he relates the punishment of persons engaged in chemical pursuits as coiners Doubtless at this period, when the philosopher's stone was still an object of alchemical research, many availed themselves of the cloak this offered them for their designs. After the decree for reversing the edict against antimony was passed, Patin writes:

"These gentlemen declare a poison is not a poison in the hands of the physician. They speak against their own experience, for most of them have killed their wives, their children, or their friends with it; and they speak well of the drug in order to curry favour with the apothecaries, although they do not dare to taste a drop of it themselves. I console myself; for there must be heresies in order that good men may be proved; but it has never been my humour to worship the golden calf, or to consider fortune as a goddess. God preserve me from doing so in future. I am content with the mediocrity of mine, peace and little. When the wind changes, all these champions of antimony will be dissipated like the smoke of their own furnaces. *Ipsi peribunt: dii meliora piis—Vale.*"

It was not only heresies among the learned Faculty that Patin vainly endeavoured to oppose, but likewise the encroachments of the *Apothecaries* and the *Barber-Surgeons*, against whom he is quite as severe as against the veriest charlatans. His especial aversion was the poor apothecary.

whom he considered an useless and expensive appendage, covering with well-deserved ridicule the complex and absurd formulæ then so much in vogue. His indignation was also excited by the patronage afforded the apothecaries by certain of the Faculty for their mutual advantage, at the expense of the patient. The apothecary was evidently then beginning to be much employed, and developing into the general practitioner; and Patin and his colleagues, as well as the practitioners of our own country, would have been better employed in aiding, rather than obstructing, his attempt at placing his position upon a sounder basis, by means of an improved education.

So violent were the diatribes of Patin, that the Apothecaries brought an action against him for libel; but this he defended without the help of any advocate, to the great discomfiture of his assailants. "This trial," he writes, "has brought me nothing but honour, and made my thesis well known, for everybody is asking for it." He endeavoured to countermine them in another way by publishing a small work, entitled the "Charitable Physician," which met with great success, and instructed the public in the mode of preparing the few purgative potions and enemata he employed. Indeed, his pharmacopœia seems to have been limited to manna, senna, rhubarb, cassia, and the syrup of pale roses, by the help of which, bleeding, and an appropriate regimen, he maintains all curable diseases were to be cured. He declares that the Faculty would have rid Paris in this way of the whole tribe, had not some of its members found their interest in conniving with the apothecaries. Addressing a friend at Lyons, he writes:

"You have come to an agreement then with that pest of medicine the apothecaries. They deserved no such favour from their masters, upon whom they should absolutely depend. If you wish to prevent their presuming and encroaching upon you, remind them that the 'Charitable Physician' has well nigh ruined those of Paris. Make them understand that the grocers sell rhubarb, sena, and syrup of pale roses, with which remedies we do without them, and have rendered them so ridiculous that they are no longer seen in families, and have somewhat too much leisure now to look after their shops. God be praised we hear nothing now of bezoards, cordials in the small-pox, cordial julep, and pearls in all kinds of diseases. The people are undeceived, and are much obliged to several of our faculty for having delivered them from the tyranny of these Arabic cooks. Those who used to complain of the great expense and excessive charges of the apothecaries were the first to become undeceived; and it is observable that, whereas heretofore we were constantly seeing Apothecaries' bills brought before the law-courts to be taxed by the physicians appointed for the purpose, we see this now no longer. * * * The people of Paris have become so accustomed to the saving, that when they become ill they no longer seek for the apothecary, but send at once to the physician; and although, owing to the badness of the times, there are several patients who do not pay very well, yet at least we have the advantage of being the first called, and we do not see at their houses that litter of juleps, apozemes, powders, opiates and cordials, which for the most part only served to prolong the disease, and heat and disgust the patient, besides proving very expensive to him."

Subsequently he writes as if this defeat, as in truth it was, were rather desired than accomplished.

"So little do I care for this sort of persons, that they excite neither my envy

or pity, and never will I, by God's help, deceive a poor patient to gratify them. Strange it is indeed that they so easily find certain persons in our profession, who, abandoning their honour and conscience, undertake the defence of so deplorable and unfortunate a cause. We ought to hate them as a pest, inasmuch as they have corrupted and endeavoured to destroy true medicine by their avarice and tyranny, and no doubt would have accomplished their ends, had not God raised up honest persons who have strongly and courageously opposed their wicked designs. As for me, I neither like nor fear them. If they deprive me of practice, I shall find other for my recompense, and on no acconnt whatever would I accept their friendship. * * * * * * * * *
Polypharmacy never yet made a good physician; *ad bene medendum, pauca, sed selecta et longo usu probata requirantur remidia, tempore et loco adhibito.*"

There can be no doubt that many of Patin's strictures upon the enormous abuses of drug-giving prevailing at the epoch were well-founded: but he is no less overbearing and severe towards another class of practitioners, the *Surgeons* and *Barber-surgeons*—the "booted lacqueys" of the physicians, as he contemptuously calls them. Every effort was opposed to all their attempts at increasing their acquirements and extending their sphere of action: and, so many years after Ambrose Paré had cast such lustre upon French surgery, we find the domineering superiority of the physicians rigorously maintained. It is true that certain operations, such as lithotomy, were performed by a special class of persons, who seem to have been held in considerable estimation by the Faculty.

"As to the surgeon-barbers they are never admitted without an approbation, and are examined in our presence. They are permitted to practice nothing but surgery —no pharmacy whatever; and especially neither purgative nor anodyne may be given *sine prescriptione medici.*"

A species of tribute seems annually to have been paid as an ackowledgement of the superiority of the physicians.

"After the submissions which the surgeons are accustomed to make to the Faculty, they pay to the Dean 100 *sous tournois* for annual quittance, which has long been a mark of their subjection to the Faculty. Besides this, each Master, on the day of his election, pays, from gratitude to his good mother, the Faculty, to the Dean four livres for his reception, which we take care to make them pay, lf they themselves forget this quittance."

Nothing could be more reprehensible than the conduct of Patin and his colleagues towards the surgeons (so called as distinguished from the mere barber-surgeons) who, from having received an academical education, were termed "surgeons of the long robe," and who were anxious to incorporate themselves into an independent college, with appropriate costume, and power to grant degrees. All attempts of this kind were fiercely opposed by the Faculty, and after long litigation, a degree confirmatory of the predominance of this body was obtained from the Parliament so late as 1660 and 1676. "Surgery was degraded in 1660," says Louis, in his History of the Royal Academy of Surgery, "and yet, when the Academy of Sciences was established in 1666, the surgeons were admitted therein and maintained a distinguished rank among the illustrious men whom the government presented to the nation as the chosen among her philosophers." M. Parise furnishes us with a copy of the oath the surgeons and apothecaries were compelled to take before the Faculty of Medicine, the

latter only being admitted when the former had retired. The surgeon's oath runs thus:—

"You swear you will obey the Dean of the Faculty in all things honest and permissible; as well as pay the same honour and respect to the doctors of the Faculty as scholars owe to their masters. 2. That you will not divulge the secrets of the Faculty, supposing that you know them, and, on the contrary, you will reveal to it whatever you may learn is being contrived against its interests. 3. That you will proceed determinedly against all those who practice medicine illegally, that is, those who have not been approved by the Faculty, and that you will aid with all your might all the proceedings it may take against them. 4. That you will not execute the orders of any physician unless he be a doctor of the Faculty or approved by it. 5. That you will administer no purgative or alterative or cordial medicine; and that you will interfere only with what concerns the manual operations of surgery."

It may be easily imagined that the surgeons made every effort to get so iniquitous a decree repealed, which we believe was only accomplished when the various institutions were re-modelled during the Revolution. Patin notices these attempts with his usual vituperations.

"Most of our surgeons are great rascals, *putidissimi nebulones*, *iniquissimi ardeliones*. By means of the king's first barber, whom they would much prefer having at their head to being subjected to our Faculty, which has brought them up, preserved them, and maintained them to the present time, they obtained a respite of the execution of the decree. Next day they began defying us, and replaced on their gate the word '*Collegium*,' which they had effaced. Three days afterwards our Dean procured another decree, which commanded them to discuss the matter no further, but to obey the original one. The king even said he would not interfere in the matter. It is a race of vipers that is constantly rebelling against justice and honesty."

Patin's animosities were far from being confined to professional delinquents. Cardinal Mazarin, the monks and priests, especially the Jesuits, the popes, and their agents, all come in for a share of his wrath and sarcasm. And when we contemplate the corrupt condition of the ecclesiastics, the profligacy of the higher ranks, and the miseries, oppressions, and exactions to which the masses of the people were subjected, as abundantly set forth in these letters, we can well excuse the strong expressions of this indignant exposer of abuses, which, allowed to continue and accumulate for another century, at last gave rise to the terrible revolutionary tempest that shook Europe to its very centre. To this part of his letters, however, we can only direct the attention of those of our readers desirous of studying the facts of history rather than the opinions of historians, as containing for them a rich fund of information.

The student of literary history will also derive much gratification in the perusal of these volumes from the very numerous and often valuable bibliographical notices they contain. Patin was an erudite scholar, and a man of taste and critical acumen. He accumulated a valuable library of some ten thousand volumes, and was ever upon the lookout to procure, at any expense, the new works of note, as they appeared: and many are the interesting comments we meet with upon Salmasius, Riolan, Erasmus, Gassendi, Grotius, Scaliger, Hoffman, and a host of others, not to mention the ancients, of whom he was a passionate admirer, and frequently a most pertinent quoter. He also much occupied himself in accumulating

the best thesis he could meet with, and his letters are constantly referring to the means of procuring these and other new publications, which it would seem were at that period more often printed in Holland and England than in France, a rigid censorship of the press being maintained by the Jesuits in this last century. The lovingness and respect with which he speaks of his favourite writers, and his anxiety for new productions from the pens of those still alive, have something even affecting in them. Never is Patin so happy as when in his study, dipping into his Galen or Virgil, or devouring the last new work of Riolan or Salmasius; and the ire which the bold practice of some "stibial" doctor, or the pretensions of some unfortunate chirurgeon had recently excited is exchanged for the enjoyment and grateful expressions of the literary enthusiast. "My little library," he exclaims, "is the light of my eyes and the solace of my labours." "I hold myself to be happier there with my books and a little leisure, than is Mazarin with all his riches and his cares." His range of reading was large, embracing the whole field of the belles-lettres, perusal of works relating to which he termed his *debaucheries*, as compared with the sterner reading of the medical classics he so much admired.

Although a severe censurer of the conduct of the great, and manifesting on many occasions very democratic opinions, Patin was always a loyal subject, and put great faith in the good disposition of the young king proving a corrective for the evils of the state. The decapitation of Charles I. by the English seems much to have startled him, and he always speaks of us as a very ferocious people, as we were doubtless considered in those days on the continent, traces of such feeling even yet remaining there. He the more cordially disliked us from the fact of one of his most favourite writers and intimate friend, Salmasius, having written so forcibly in condemnation of the proceedings of Cromwell and his followers.

"As for the English, if you except a small number of honest persons, I wish them as much harm as they have done to their king. It is a haughty, proud, and malignant nation, '*quæque παπροκαςαδοτον habet odisse Gallos*,' as Scaliger somewhere says in his beautiful letters. * * * * The English are *crudeles et feroces*. Theodore Macille said they were a species of man *de genere lupino*; as the Spaniards and Italians were of the nature of the fox, *callidi, versipelles et astuti*. The Jesuits are hermaphrodite, wicked as the English, and cunning as the Italians. * * * * * The Queen of England arrived to-day at Calais, where her son the Duke of York came to meet her. The executions are going on in London, where there have been already ten hung. The last were two colonels, who were ordered by the Parliament to see execution done on the late King. All these criminals are strange people, who will repent nothing either of the fact or the death. They are martyrs of the State and of the times. It seems to me they are quite infatuated. I think this only occurs in that nation which seems to differ from all others. They are so cruel, ferocious, and blood-thirsty, as to be nearly fatuous."

It may be believed that one so jealous of the dignity and privileges of the Faculty as Patin was not indifferent to the highest honours it had in its gift; and accordingly we find him frequently alluding to his two successive elections as Dean of the body with evident satisfaction; although, by reason of the dissensions excited among its members by the advocacy of the claims of antimony, matters did not always run so smooth as might be desired. In fact, the Dean, as M. Parise observes, "was a dictator at

the head of a republic difficult enough to govern." The following is Patin's description of his functions, and the somewhat complex mode of election:

"He is master of the bachelors, and regulates the discipline of the shools. He keeps our registries, which are 200 years old, as well as the seals of the Faculty. He receives our revenue, and renders us an account of it. Every thesis requires his signature and approbation. He arranges the order of precedence of the doctors, and assembles the Faculty only when he pleases. He, with the four examiners, conducts rigorous examinations, which last for a week, and he is one of the three deans of the University. It is a laborious office, one of much honour, but harassing in its duties. He conducts the actions brought by the Faculty, and even pleads before the Advocate-general. It is a very honorable but a most laborious charge, which a worthy man ought to feel very happy in escaping, satisfied in being thought worthy of filling it by being elected to it. This is the mode of the election. All the Faculty being present, the Dean who is going out of office thanks the company for the honour they have done him, and desires they will elect another in his place. The names of all the doctors present are written on so many tickets and laid on the table. The first half of these are taken and placed in a hat, and are called the great bench. We are now 112 in number, so that the first 56 are placed in the hat. When these tickets have been well shaken by the oldest of our company, who is M. Riolan, the Dean quitting office draws three names one after the other, and draws two others from the little bench. None of these five doctors can be the Dean on that day, but they are the electors, who, after having taken an oath of fidelity, are shut up in the chapel, where they choose the names of three of the assembled doctors whom they believe worthy of the office, two from the great bench and one from the little one. These names are put into the hat by the senior doctor, and the Dean placing therein his hand wide open, draws one who is to be the future Dean. I have been several times an elector, and have even been placed in the hat three times, and have remained there, and I shall not be sorry to do so whenever I am put there, for I have no leisure for such a charge *sortes in urnam mittuntur, sed temperantur a Domino.* All these ceremonies are very ancient, and are religiously observed out of respect to their antiquity."

Patin, however, at last attained the honor, and his active disposition found time to creditably fulfil the duties, some of which, such as presiding at the humiliating admission of the surgeons and apothecaries, pursuing illegitimate practitioners, and holding disputations on the thesis, were eminently congenial to his tastes. The disputation of the thesis was one especially so; and he constantly refers in his letter to the collection of those he and his friends are making, negotiating exchanges, purchases, &c., or liberally presenting them. "I can give you several," he says in one letter, "seeing I have always collected as many as possible, so that I have here more than 700 in good order, besides a great number of duplicates." Some of these productions were of great merit, and had a large circulation, but even some of the most approved in that scholastic and disputatious period would seem oftentimes insignificant to our more practical age. But, although the authority of the ancients and the schools was still predominant and indisputable in the Faculty, the means of correcting their errors were already in operation; for not only had the propriety of administering the various new remedies, cinchona, antimony, mercury, and opium, been admitted by a majority of the doctors; but the study of human anatomy was pursued with zeal, though not with much regularity. Patin in his letters repeatedly gives us accounts of the post-

mortem examinations he had been present at; and frequently interrupted his course of lectures in order that his pupils might have the opportunity of witnessing the dissection of criminals—opportunities, indeed, from the dreadful frequency with which capital punishment was at that period inflicted, of no uncommon occurrence.

A curious and somewhat undignified custom prevailed with the Faculty, by which all the candidates, received or rejected, were required to provide a handsome entertainment for their examiners and a number of the Faculty besides. "We formed three tables, at each of which were from twelve to thirteen of us. I never saw such rejoicing, such laughter, and such good cheer." Imagine the rejected of the College of Physicians or Surgeons thus contributing to the gladdening the hearts of their examiners! The custom prevailed till recent times, for the celebrated Pinel, although received as physician at Montpellier, having been rejected at Paris, was still obliged to pay his share of the banquet. As Patin often mentions that many of the Doctors (especially those who prescribed antimony) were much addicted to deep potations, we can easily imagine that such scenes did not always reflect credit on the learned body.

The Paris Faculty seems always to have been on terms of bitter rivalry with the other establishments for medical education, especially the Faculty of Montpellier; and Patin again and again accuses them of disposing (an abuse prevalent amongst some "learned" universities of our own times) of their degrees for mere pecuniary considerations.

"If the physicians of Montpellier are badly paid by their patients, they recompense themselves by giving licenses for whoever will pay for them; *modo fiat nummis præsentibus.* It is an abuse I am astonished at, but have no means of preventing. * * * * * * The little universities *manifeste peccant in publica commoda;* they reject no one. If the young doctor is not received cheaply at one place he goes to another: and this is why the physicians of Rheims are now pleading against those of Angers; seeing that they sell their degrees cheaper, after a slight examination, in a little time, and if it is desired even without theses. Really if some remedy be not found for this disorder, we shall have more physicians in France than there are apples in Normandy or priests in Spain and Italy. Without exaggerating their ignorance, which is in truth extreme, shameful, and dangerous, they will not even study or have any books, it being enough for them *si habeant in manibus diplomata academica, etiam vili ære redempta,* and if they are the cousins or neighbours of some surgeon or apothecary. I have seen some of them who have even forged their diplomas. They repair to their native country, village, or small town, with scarcely a *Perdulcis* or a *Fernel* about them, or not understanding them, pretending however to do so, just as if they had the *jus vitæ et necis.*"

It must not be believed from the scraps of Latin with which Patin, after the manner of his time, delights to interlard his letters, that he was any pedantic smatterer. On the contrary, those of his letters written entirely in that language, and the numerous and apt quotations from the poets in the present collection, prove him to have been an elegant scholar; and indeed it is surprising, amidst his numerous professional occupations, that he could have acquired so intimate an acquaintance with the literature of antiquity and of his own times.

We have space only for one other of the numerous extracts we had marked, and this relates to the pathological indications of *red hair.*

"The disease you have taken the trouble to describe to me is somewhat of a gouty nature. I know the patient whose complexion is delicate. His father, who had black hair, died of a pulmonary catarrh, and his mother of an inflammation of the lungs. She was the most quarrelsome and choleric woman in existence, and more than that, she had very red hair. Now, it is constantly observed that, inflammation of the lungs is fatal in the red-haired. I was called in consultation with the late M. De la Vigne upon the case of Collier, Secretary to the King, who was 75 years of age, and suffering from inflammation of the lungs. As he had red hair, I at once predicted a fatal issue. M. De la Vigne inquired where I had learned this prognosis. I replied that I had always observed it to be a very true one, besides which, I had heard it from M. N. Piètre, who had it from his brother, the great Simon Piètre, and that its explanation was, that red-haired persons abounded in an acrid and malignant serosity. He replied that he had always observed the same thing, and I have since read of it in the *Ephemerides* of Baillou."

In concluding our notice of these interesting letters we feel, as we feared on commencing it, that we have been enabled to convey only a very inadequate idea of them to our readers. But at least they will be aware of the existence of so remarkable a book; and will do well to put themselves in possession of it. Numbers of topics are embraced within the scope of the correspondence, relating to the politics, the general literature, the manners, and the gossip of the day, to which our limits and the objects of this Journal alike prevent our alluding. To M. Revillé-Parise the medical profession in France owe much for recalling their attention to these interesting letters and their once celebrated author.* It was a work of love and of labour, and we suppose no where could an editor have been found more fitting for the work. Himself an elegant writer and acute critic, evidently an admirer, though not a blind one, of the erudition of a by-gone age; well versed in the literary history of our profession, and indeed in general literature; a firm upholder of the true dignity and rights of medicine when pursued as a liberal profession and not for mere lucre; but a stern denouncer of all charlatanerie, professional and non-professional; he is fully enabled to enter into the spirit and views of his author, to indicate at once his merits and his failings. This he has carefully and faithfully done in a series of "scientific, historical, philosophical, and literary" foot-notes, which add much to the value of the book, by explaining some obscurities, and indicating comparisons between the state of medicine as it was and as it is—not invariably, however, in favour of the latter. The preparation of these, and the rectifying the numerous errors of the spurious editions of Patin's letters, by a comparison with the difficultly-decipherable originals, must have been a work requiring untiring patience.

* Alas! how fugitive is fame, unless built upon some solid undertaking which may descend to posterity. The reputation of Guy Patin during his life-time was European, and shortly after his decease several collections of his letters were published and eagerly read; and yet, in less than a couple of centuries afterwards, several well-informed persons are found calling at the publisher's, upon hearing of the new edition, to inquire who is this physician and where is his abode!

I. A Dictionary of Practical Medicine. By *James Copland*, M.D., F.R.S., &c. &c. Parts X. and XI. Article—Hæmagastric Pestilence (Yellow Fever).

II. Report of a Special Committee of the House of Assembly of the State of New York, on the present Quarantine Laws. Albany, 1846.

III. Report of the Fever at Boa Vista. By Dr. *McWilliam*, R.N. Presented to the House of Commons, in pursuance of their Address of the 16th March, 1847.

IV. Letter addressed by Sir William Pym to the Lords of the Council, relative to a Report on the Fever at Boa Vista, by Dr. McWilliam. Presented to the House of Commons, in pursuance of their Address of May 14th, 1847.

In recent Numbers of this Journal we have considered at some length two of those wide-spread and devastating diseases, to which the term of Pestilence has been more peculiarly applied by Dr. Copland, and which he has treated of apart and by themselves in his great work on Practical Medicine. The especial object of our remarks in these articles has been to examine the important question—important in a political and commercial, as well as in a strictly professional, point of view—whether the diffusion of the diseases in question over large districts of the earth can with reason be attributed to their direct transmission from one individual to another, or whether it be not mainly owing to some peculiar, although unknown and almost inscrutable, condition of the atmosphere, over which the utmost efforts of man can have no controul, save and except by removing all those causes of insalubrity which, it is well known, invariably add malignancy to morbific miasms, while at the same time they render the human body not only more susceptible of their influence, but also less capable of resisting their fatal operation. In the case of the Plague, abundant cause was shown that many of the opinions, that have hitherto been maintained (for the last century, at least), are utterly erroneous, and therefore that the system of Quarantine that has been based upon these opinions, has been most absurdly vexatious, and most unnecessarily oppressive. The great relaxations of the restraints to commercial intercourse, that have been made within the last six months by the Governments of England and France, are the gratifying results of our more accurate and enlightened knowledge of the disease;—thanks chiefly to the admirable Report of the French Academy, with which our readers are so well acquainted.

With respect to the Epidemic Cholera, the evidence adduced in our last Number will be deemed sufficient, we trust, to satisfy almost every impartial mind that infection, whether by persons or by the medium of inanimate objects, was a very subordinate and merely an occasional agent in the

diffusion of that extraordinary pestilence, which almost girdled the earth in its career, from the wall of China on the East, to the banks of the Ohio and the St. Lawrence on the West. Its history is especially interesting to the medical philosopher, as presenting, on the whole, by far the best instance on record, where we have a full and complete account of the rise, progress, and gradual and progressive extension of a great and destructive epidemic. For a period of twenty years, it appeared to be travelling on from one part of the world's surface to another. Not that, during this time, it always followed a direct or an uninterrupted course along any one tract; for we know that this was not the case. Still, its general march was onward, and advancing from East to West; as, indeed, has been observed to be the case with some other wide-spread diseases. And never were the attempts of man to stay the progress of an epidemic distemper more signally—we might almost say ludicrously—impotent. Truly it may be said that in the event of a second irruption of the Cholera into Europe, it would be as reasonable to expect to screen a province from the invasion of a pestiferous blight by building a lofty wall around its borders, as seek to keep out that disease from a country by any system of quarantine establishment. And here we cannot but suggest how very useful an accurate history of certain blights and mildews—and what are they but pestilences of the vegetable world?—might be to the elucidation of that of epidemic disorders. Is it not probable that there is something more than a mere analogy between their exciting causes? Nay, is it unlikely that their origin is sometimes the very same? At all events, the comparative investigation of animal and vegetable pestilences affords, we humbly think, the fairest prospect of our ever attaining to anything like a reasonable hypothesis as to the cause or causes which gave them birth. The appearance of the potatoe blight among us has invested the subject at this present time with more than ordinary interest, and may thus have the effect of awakening the attention of scientific men in general, to the vast importance of studying the wide-spread diseases of plants.* But this only in the way of parenthesis.

We shall now proceed to the more immediate subject of the present article; viz. the history of the Yellow Fever, or, as Dr. Copland proposes to call it, the Hæmagastric pestilence,† more especially in reference to its probable causes, its relation with other diseases, the general mode of its propagation, and the means to be employed to prevent its development or arrest its extension. The subject, it may be remarked,

* Has any accurate account of the chronological and geographical history of the potatoe disease been published? Where and when did it first make its appearance? Have different countries been cotemporaneously or successively visited by its invasion? How long has it remained in each country, and what are the circumstances of soil, locality, &c., that seemed to favour or resist its incursions? Although Mr. Smee and some other writers have collected a good deal of information elucidatory of these questions, there is no complete history of this singular pestilence; at least we know not of any.

† From αιμα *blood*, and γαστηρ *the belly*; so called, we presume, from the black sanguineous discharges, upwards and downwards, from the alimentary canal.

in limine, is in some respects surrounded with much greater difficulties than attend the like history of its two predecessors; difficulties that chiefly arise from the circumstance that medical writers, even in the present day, are far from being agreed among themselves as to what assemblage of symptoms or morbid phenomena the appellation of Yellow Fever is to be restricted. A somewhat similar difficulty, indeed, occurred in the case of the Epidemic Cholera; for Dr. Copland, it will be remembered, has endeavoured to establish a specific distinction between that disease and the endemic cholera, that had been long known in the East. But the grounds adduced for this distinction we found to be altogether so very insufficient, that it seems not at all likely that many will be inclined to adopt the same opinion. With respect, however, to the Yellow Fever, the point is by no means so easily disposed of; and indeed the solution of this very point is, as we have remarked, the chief difficulty that awaits us in the following inquiry.

All our readers are probably aware that, for the last half-century and upwards, a very keen and occasionally very acrimonious controversy has been going on among medical men, more especially among those belonging to the naval and military services, as to the true nature of the disease so designated. One party contends that more than one malady or morbid state, although dissimilar (it is asserted) in phenomena as well as in origin and attributes, have been most inaccurately confounded together under a single name, and that it is to this unfortunate confusion of things really and essentially distinct, that all the perplexities of the subject are owing. The gentlemen who hold this opinion assert that Yellow Fever is a disease *sui generis*, and as specifically different from all other forms of fever as Smallpox, Scarlatina, or Measles; and they maintain that it has no alliance whatsoever, save only what is merely coincident and accidental, with any other malady of the country where it happens to appear. On the other hand, a large proportion of writers on the diseases of hot climates regard Yellow fever as merely an aggravated and malignant form of the ordinary endemic fever of these climates; the only difference between them, according to these gentlemen, being in degree and not in kind. We frankly confess that much may be said, and said, too, with force and show of reason, on both sides of the question, and that its solution is far from being so very easy, as many writers on both sides of the controversy have far too dogmatically asserted.

The appearance of the last two parts of Dr. Copland's Dictionary of Practical Medicine appears to us to afford a singularly favourable opportunity to sift and try the merits of the question fairly. Our reasons for saying so are these. The objection that has always been made by the one party against the other, by the advocates of the specific and essentially distinct nature of the Yellow Fever against those who will not admit this doctrine, is that their opponents have confounded different diseases together, and have thus too often ascribed to one the characters and attributes which really belong only to the other. If, for example, an appeal be ever made by the latter to the writings of Bancroft and Jackson, for evidence or authority upon one of the disputed points in the history of the disease, they are at once met with the reply that these writers never distinguished with any degree of accuracy the genuine pestilential disease from other fevers,

which, although resembling it in many respects, are really and truly of a very different nature. Now it is not easy, it must be confessed, to get rid of this objection; resolving itself, as it does, into a simple assertion which it is often impossible, from the absence of sufficient, or at least admissible, data, to gainsay. To attempt to settle the dispute by a mere counter-assertion will never do; this is only entangling and perplexing the subject the more. An angry controversy arises; the most conflicting and contradictory statements are made; facts are distorted to serve a particular purpose; and the disputants become, it is generally too obvious, more anxious about the triumph of their own opinions than the discovery and elucidation of the simple truth. It cannot be denied that, among medical writers, controversial discussions are rarely carried on with that spirit of courtesy and firmness which might be reasonably expected from men engaged in so honourable a profession as ours.

But to return to our line of argument: we were alluding to the objection that will assuredly be urged against any appeal to the works of many of the most able and experienced writers upon the subject. Now, how is this objection to be obviated? Not certainly by balancing the numerical strength of the two contending parties, nor yet by calculating the comparative ability or means of observation of the opposing authorities, nor even by judging from the alleged results of their experience in different climates and in all variety of circumstances. We must therefore follow another plan, and try if we can steer clear of the difficulty that has been mentioned. The argument we shall follow is this. If indeed Bancroft, Jackson, and other writers on the same side have erred in the way supposed, the same thing cannot surely be said of Dr. Copland, who has not only written at great length on the various diseases involved in the controversy, but has also laboured with the utmost energy to establish those very distinctions that have been so stoutly denied by the gentlemen now named. That his own party will be willing to accept his championship of their cause, we cannot for a moment doubt; for where can they find an advocate more zealous for their cause, and better, or indeed so well, prepared to enforce their arguments, and repel the attacks of their adversaries? His immense erudition, his intimate acquaintance with all that has been advanced on the subject, his great experience in marshalling his facts, so that they may appear as strong and conclusive as they possibly can do, and in exposing the weakness or fallacy of those which do not favour his views, are all well known, and will naturally cause him to be looked up to as the most learned and able expositor of his side of the question. Such then being the case, it only remains to be seen whether his opponents are equally ready to admit his descriptions, as the standard by which to test the soundness of those principles which he labours to inculcate. In the present article we propose to do so; and, by comparing together the accounts which he has himself given of those diseases that are (it is asserted) so liable to be mistaken for each other, to endeavour to arrive at something like a high degree of probability—we do not presume to speak of certainty in the matter—upon the point at issue, viz. the nature of Yellow Fever; whether, in short, it is a disease specifically and essentially distinct from every other, or whether it is but a modified form of the common endemic fever or fevers of the country where it happens to prevail.

It may be as well to prefix the general definition which Dr. Copland has given of the malady. It runs thus :—*After chills, shivering, languor, severe pain in the orbits and forehead, also in the loins and limbs;—rapid pulse, flushed face, glassy suffused eyes;—peculiar burning heat of skin and frequent delirium—nausea and vomiting with epigastric pain, costiveness, great anxiety, restlessness, and watchfulness; subsequently hiccough, black vomiting, scanty or suppressed urine, hæmorrhages from the mucous canals, lemon or muddy yellowness of skin, generally terminating in death in its most severe form.*

The yellow discolouration of the skin and the black vomit are by some writers considered to be characteristic or pathognomonic phenomena of the disease ; but Dr. Copland admits that neither of these symptoms is uniformly or invariably present, and moreover that both the one and the other occasionally occur in other descriptions of fever "The term *yellow* fever," he says in one passage, "ought to be entirely discarded ; as yellowness, being improperly viewed as a pathognomonic symptom of one kind of fever, all others, in which it is a contingent phenomenon, although not more frequently met with in one than in another, have been confounded with that fever." After alluding to the different shades of the discolouration of the surface, he adds :—"It may proceed either from the passage of the colouring parts of the bile into the circulation, or from incipient dissolution of the blood, with loss of the vital tone of the capillaries."

With respect to the *black vomit*, it is admitted, in more passages than one, that it is not necessarily present even in the worst and most rapidly fatal cases of the disease. If such then be the case with the two most conspicuous and remarkable symptoms, we cannot be surprised that the others, enumerated in the definition, are still more uncertain and fallacious. Truly has Dr. Gillkrest, one of the most recent and candid writers on the subject,* remarked that "the anomalies which this disease has been observed to present—the absence, under the observation of one medical man, of some of the symptoms which during another epidemic had been well marked—the fact of practitioners having observed that certain symptoms, prominent during one period of an epidemic, have at another period been totally absent—the fact that of patients in the very same ward of a hospital being frequently found to labour under symptoms so variously grouped as to lead an inexperienced practitioner to believe that he had before him three or four diseases bearing little affinity to each other ;—all these circumstances have thrown difficulties in the way of this disease having had a place assigned to it in nosological arrangements free from objections." Of the perfect truth of these observations the reader will soon be convinced, by the perusal of the details which we now intend to bring under his notice.

And first with respect to the very close resemblance that may be traced between the different degrees or modifications of the genuine pestilence, and some of the ordinary endemic fevers of inter-tropical regions. This point we shall illustrate by presenting in opposite columns the descriptions of each, as given by our author himself.

---

* Cyclopædia of Practical Medicine. Vol. II., p. 264.

Yellow Fever.—"The mildest form of this pestilence generally makes its appearance with languor and slight chills, soon followed by heat of skin; quick and full pulse; uneasiness in the loins and limbs; severe headache, confined chiefly to the orbits and forehead; a peculiar shining or drunken appearance of the eyes; hot dry skin; a loaded but moist tongue, with little thirst; sickness at stomach, with costiveness, and a feeling of uneasiness, not amounting to pain, at the epigastrium, and a sense of rawness or soreness in the fauces and in the course of the œsophagus. These symptoms may continue from twelve to twenty-four or thirty-six hours, when the patient, having taken only some purgative and febrifuge medicines, or an emetic, falls into a refreshing sleep, from which he awakes in a gentle perspiration, free from pain and fever, and complaining only of debility, from which he rapidly recovers." Vol. III., p. 139.

*Bilio-gastric Fever.**—After the usual premonitory symptoms of a febrile attack, there "soon succeed severe frontal headache, vertigo, nausea, vomiting, burning heat of skin, restlessness, watchfulness, slight anxiety at the præcordia, pain and oppression in the epigastrium, and in one or both hypochondria, with more or less soreness, fulness, and tenderness. The eyes are moist and injected, the conjunctiva often yellowish; the face is flushed; the breathing oppressed and accelerated; the pulse full, large, quick, and strong, rarely hard; the tongue is clammy, moist, furred, and yellowish, with a bitter taste in the mouth; the thirst is urgent, the breath fætid; the bowels are obstinately costive, or loose; the stools bilious, and the urine scanty and dark. When the stomach and bowels are inordinately affected, cerebral congestion very frequently supervenes at a later period. As the disease advances, the pulse feels less full and is weaker than in health. The thirst and anxiety are increased; and the upper parts of the body are sometimes covered by a profuse sweat whilst the skin still continues hot." Vol. I, p. 984.

That there may be no mistake respecting the Bilio-gastric Fever here spoken of, it will be well to give the following few particulars touching its history:—

"This fever is either sporadic, endemic, or epidemic.—It is *endemic* in warm countries and marshy situations among Europeans, particularly those who have not been long resident in these parts: and in marshy localities in the summer and autumn in temperate climates.—It is *epidemic* in some seasons, particularly in autumn when the summer has been hot, after a wet spring, or after great falls of rain, or after inundations, and when great numbers of predisposed persons, especially from high latitudes, visit such localities. In these circumstances and persons, it proves the seasoning fever. It is observed chiefly in adults of the bilious or bilio-sanguine temperaments, and in persons addicted to spirituous liquors. It is a very prevalent fever in the countries bordering on the Mediterranean, in the East Indies, and in America, and consequently in fleets and armies in these parts.

"Gastro-bilious fever is *caused* chiefly by exhalations from the soil, or from vegetable and animal matter undergoing decomposition, in connection with at-

---

* The fever, so denominated by our author, is the common "bilious," or "gastro-bilious" fever of many writers. It is very well defined by Dr. Copland thus:—*Vascular re-action following chills or rigors and other symptoms of premonition and invasion, with predominant affection of the biliary functions, and of the digestive mucous surface; frequently with yellowness of the skin in the severer cases.*

mospheric heat; by exposure to the sun; by the night airs or dews, and the influence of cold following such exposures, or excessive exertion or high ranges of temperature; by intemperance and errors of diet or of regimen; by excesses in vinous or spirituous liquors." P. 984.

With regard to the *mild form* of the true Yellow Fever, all that requires to be said respecting it is that, however slight and unalarming the symptoms may be, the disease is declared by our author to be essentially and invariably infectious; so much so that, in his opinion, the most fatal and malignant infection may originate from a case of the least possible severity. He acknowledges, indeed, that in this mild form it cannot be distinguished but with very great difficulty from the common bilious or bilio-gastric fever; so nearly do their symptoms resemble each other. This difficulty, too, must be not a little enhanced by the circumstance, admitted by Dr. C., that the latter may acquire, under peculiar circumstances, infectious properties. "The influence of infection in producing it," says he, "has been doubted; but the experience of Drs. Denmark and Boyd, in ships and hospitals in the Mediterranean, has demonstrated its occasional origin in the cause—or, at least, the power infection evinces in producing a severe modification of it."

Before taking leave of the two kinds of fever described above—viz. the Bilio-gastric and the mild form of Yellow-fever—we must not omit to state that there is quite as close an analogy or alliance between the Remittent fever of warm climates and these, as there is between the two kinds themselves. This alliance is rendered the more conspicuous from the fact admitted by our author, that the bilio-gastric fever not only arises from the same causes as the remittent, but also not unfrequently lapses into the periodic type. Compare the following description of the first form or degree of the latter with those already given:

Mild Remittent.—"The stage of *invasion* is similar to that already described; it being attended by coldness of the surface, and frequently by shivering. The cold is soon superseded by heat, by febrile flushes, or by alternations of heat and cold, by nausea, and occasionally by vomiting, which soon develop the stage of *excitement*. With it, the pains of the head, back, and limbs become remarkably aggravated; the mouth is clammy and dry; the tongue white or loaded; the surface very hot and parched; the face flushed; the features tumid; and the pain of the head attended by a feeling of distension and throbbing, often passing into delirium. The pulse, which, at the invasion, was small, irregular, and weak, is now full, large, strong, and frequent; thirst is urgent; the bowels constipated; and the urine scanty and high-coloured. There is always more or less tenderness at the epigastrium, with nausea, and often with vomiting. These symptoms generally continue from about ten or twelve to eighteen hours, when perspiration breaks out; the pulse falls in frequency and strength; the irritability of the stomach subsides; delirium disappears; and the skin becomes cooler; but there is merely a remission or abatement, but no intermission of the febrile symptoms. The remission usually continues from three to nine or ten hours, when an exacerbation occurs, sometimes preceded by chills or shiverings, at other times not, and the severer symptoms are renewed. Thus the disease proceeds with alternate remissions and exacerbations, the former generally taking place in the morning, until the seventh day, or the ninth, eleventh or fourteenth day, or much later, in temperate countries, when a copious perspiration generally puts a termination to its progress." Vol. I., p. 947.

Having thus shown how nearly the milder form of the true pestilential

fever of the West Indies and of the African coast resembles, in its outward features at least, the common endemic fevers, remittent as well as continued, of these countries, we shall now advance a step farther in our *comparative* investigation of their histories, and this we shall do according to the same plan we have followed above.

Yellow Fever.—"The more severe and more frequent form appears more suddenly (than the mild form already described—*Rev.*) and the symptoms are much more violent. The attack is ushered in by shivering and rigors. The pain in the orbits and forehead is excruciating; severe pain is also complained of in the loins and calves of the legs; the face is flushed; the eyes are glassy, suffused, or apparently inflamed; the pulse is rapid; the skin burning hot and dry; and the tongue is loaded, but moist, with little thirst. A few hours afterwards, uneasiness of stomach, with nausea and vomiting, supervenes, followed by severe pain and tenderness at the epigastrium, with a sense of rawness, heat, or inflammation in the fauces and down the œsophagus; great anxiety, restlessness, and watching, with a desire of sleep. The bowels are constipated, the evacuations scanty and deficient in bile; the urine dark-coloured and small in quantity.

"If the disease be judiciously treated, these symptoms often become ameliorated on the second or third day; the patient falling into a sleep, from which he awakes refreshed, with a perspiring or moist skin, and nearly free from all the symptoms. Debility only remains, the recovery from which is generally rapid. In many cases, however, either a partial amelioration only occurs, or the more complete subsidence of the symptoms is of short duration; the patient in a few hours beginning to be troubled with flatus in the stomach and distressing hiccough. Not unfrequently the patient is suddenly and unexpectedly seized with faintness, sickness, and painful retchings, followed by vomiting, at first of whatever had been taken into the stomach, but soon afterwards of a

*The Ardent or Seasoning Fever.**—"The attack is usually sudden. Giddiness, faintness, and general uneasiness, sometimes, however, precede it for ten or twelve hours. There is, occasionally, a slight and brief chilliness at the commencement, especially in the less violent cases, rapidly followed by a sense of universal heat; by flushed face, frontal headache, and vertigo; by inflamed heavy eyes, and great sensibility to light and sound; by pain in the occiput, neck, back, and limbs; and by a strong, full, hard, and accelerated pulse. A sense of heat, oppression, pain, or anxiety, is felt at the præcordia, sometimes with a dry cough, and pain in the side; respiration is quick, laborious, suspirous, or anxious; the tongue is white, excited, and its edges red; the fauces are arid, thirst urgent, and skin hot and dry; the urine is scanty, the bowels costive: and there is generally nausea, but seldom vomiting until some time after the attack. If the disease be not mitigated by treatment, the patient becomes extremely restless; the headache is rending and intense; vascular action is excessive, and the heat very great. Vomiting now supervenes, and follows the ingestion of whatever is taken to allay the urgency of thirst. The matters thrown off are generally tinged with bile; and a bilious yellow suffusion of the skin is frequently observed. Bilious vomiting and purging occasionally occur with the yellowness of the surface, and, in the slighter cases, become a favourable crisis. There is often great drowsiness, but no refreshing sleep. These symptoms of excessive excitement proceed with various degrees of violence, and occupy a period of from twenty-four to sixty hours, but most commonly from twenty-four to

* This is the *Endemial Causus* of Moseley, the *Inflammatory Endemic* of Dickenson, and the *Endemic Yellow Fever* of some writers.

imperfectly and loosely, and is deficient in fibrine. It afterwards becomes still more loose and defective as to crasis, and ultimately very dark, partially dissolved, and grumous; and apparently insufficient in quantity, in many cases, to distend the veins." Vol. III., p. 141.

Dr. Copland remarks that the Ardent or Seasoning Fever, "after the inflammatory excitement is subdued by copious depletions, sometimes assumes a *remittent* character."* He also expressly says that, "it is *not liable to recur.*" "A first attack," he adds, "prevents a second, if the individual continue in the climate which caused it; but, if he return to a cold country, and reside there until the energy of his system is restored, he becomes liable, upon his return to the hot climate, to a second attack, although less so than before, and in a milder form." He afterwards qualifies this statement by saying that a second attack is rare even in persons who may have returned to a temperate climate and then gone back to their tropical residence; the seasoning fever, under these circumstances, being much more frequently of a *remittent* than of a *continued* type. The Seasoning Fever, we are also told, "will not prevent those diseases which proceed from marsh exhalations; but, if the person who has been seasoned by it, be seized with fever from this cause, the periodic type will be assumed." Lastly, Dr. C. does "not believe that this—the climate or seasoning-fever—will exempt from the pestilential yellow-fever, although it may lessen the susceptibility to it, when the individual has not intermediately changed the climate. Instances are numerous of seasoned persons—of those who have suffered from this, the climate or severe inflammatory, fever—afterwards being seized with endemic or remittent fever, or with the pestilential disease (genuine yellow fever)."

This form of fever "is the disease which most frequently attacks new comers into the West Indies, more especially sailors and soldiers * * * * * * * It was also very prevalent during the last war among the British troops and sailors in the Mediterranean, and was described by Burnett, Irvine, Boyle, Brunton, and others; but it generally assumed a milder form than in the West Indies." Dr. C. does not allude to this fever ever exhibiting *infectious* properties, unless perhaps the fact implied in the following remark:—"If animal or vegetable miasms concur with them (the usually exciting causes, viz. a very high temperature, often conjoined with rich, nutritious and heating food, stimulating drinks and suppressed perspiration), the fever will present adynamic or malignant characters in proportion to the activity of either of these agents." Here, before proceeding further, a good opportunity occurs of alluding to the nature and cause of the "black vomit" and black discharges from the bowels, that constitute so remarkable a character of certain fevers in hot climates. The following excellent remarks on the subject are introduced by our author at the close of his description of the Climate or Seasoning fever.

---

* Mr. Martin, speaking of the ardent fever of Bengal, says—"The type of this fever is usually *continued*,—but sometimes *remittent*."—*Influence of Tropical Climates, 6th Edition.*

brownish fluid, resembling dirty water, mixed with a dark-coloured flaky matter, which floats upon its surface; and at last, by a matter resembling coffee-grounds or thin pitch. At this time, also, a great change takes place in the countenance, which assumes a putrid, dingy, and bloated appearance, which is most remarkable in those of a florid or sanguine complexion. A light yellow or lemon tinge appears under the eyes and ears, and soon spreads down the neck to the chest, and over the whole body. The vessels of the conjunctiva appear relaxed, and distended with blood. The vomitings continue, and the quantity of fluid ejected much exceeds that which has been drunk. They often return without being excited by ingesta: or even suddenly or unexpectedly, and when the patient has just before considered himself relieved from them."

* * * * *

"The *third form* of attack also commences with shivering or rigors, and is an aggravation of the symptoms of the second from the beginning. In this form, the face is more flushed, and the burning heat of the skin is greater than in the preceding. The sickness of stomach, hiccough, and black-vomiting appear much earlier. The bowels are obstinately constipated, and resist strong purgatives; the motions being watery, of a dirty colour, and rarely feculent or bilious. Violent delirium often occurs early in the attack, and hæmorrhages frequently take place at an early period from the nose, mouth, eyes, ears, and even from all the outlets of mucous canals. The tongue is often clean, moist, livid, or red, and raw-like, or covered with dissolved blood. The action of the kidneys is suppressed, either little or no urine being secreted. The countenance changes to a livid and yellowish hue, with yellowness of the skin. In the most severe of these attacks the patients may be carried off on the second, but generally on the third day, sometimes in convulsions." P. 139.

* * * * *

"The *blood* is always more or less changed—most remarkably after the calm occurring on the third day. Even at the commencement and during reaction, it does not coagulate, or does so forty-eight hours. During this period, blood taken from a vein is remarkably florid, warm, and fluid. The fibrin coagulates firmly, but the crassamentum is without crust, and is rarely cupped.

"The excitement having reached its acme, is quickly followed by exhaustion. This is indicated by a subsidence of the most urgent symptoms: the pain and heat are lessened; the skin becomes damp or clammy; and the patient has a sense of cold or slight chilliness. This delusive remission is a state of great danger: in some cases, it passes into rapid sinking—into a speedily fatal collapse."

* * * * *

"*Discolouration* of the skin generally takes place in this stage; appearing in yellow, yellowish brown, and livid patches. It never occurs in the period of excitement, for it is quite dissimilar from the bilious yellowness occasionally observed in that period. It is commonly attended by passive hæmorrhage from the nose, gums, eyes, ears, &c., and by black and grumous vomiting. The change of colour, and hæmorrhage, proceed from exhaustion of the vital influence in the extreme vessels, and from the changes induced in the mass of blood. The matters thrown off the stomach consist at first of ingesta and serous fluid, often coloured by bile. In a more advanced stage they are ropy, mixed with numerous small shreds, flocculi, or films, which soon acquire a dark brown, purple, or black colour; but do not, at first, communicate much of the same tint to the fluid containing them. Afterwards, the matters vomited are more intimately mixed; and, from dark-coloured blood which has been effused into the stomach, vitiated bile, and other morbid secretions, assume a dark or coffee-grounds appearance. At the same time, dark-coloured matter, resembling tar mixed with black blood, is freely discharged from the bowels."

"The *blood* at this period is black, thin, and dissolved, its fibrin seems diminished, and it does not separate into crassamentum or serum; or if it does, the former consists of a thin, dark jelly, with the black colouring matter precipitated towards the bottom of the vessel." Vol. I., p. 977.

"The source of the black matter passed from the stomach and bowels in the last stage of this and of other severe fevers of warm countries, has been variously stated. Some consider the black colour to proceed from the exudation of dark blood, which, in mixing with the secretions of the stomach, liver and bowels, imparts to them a still darker tint. Some ascribe it chiefly to the bile, and secretions from the digestive mucous follicles, which are often both very dark and thick, in the last stage of the more malignant kinds of intertropical fevers; and others believe it to arise both ways. There is no doubt that all the secretions poured into the digestive canal are more or less diseased, particularly in the latter stages: but it is as clear, that the black colour mainly depends upon the state of the blood; and that all the matter ejected upwards and downwards, presenting this appearance, does not consist of altered secretions merely,—a great part of it probably being an exudation of blood from the mucous surface. I believe, also, that these matters vary very remarkably in the ardent climate fever, in the more malignant forms of marsh or endemic fevers, and in the pestilential yellow fever —the diseases thus characterized. Dr. Jackson remarks that the secretions from the digestive mucous surface are ropy and clear during the early periods, and are brown or black in the latter—sometimes black as soot; and that the sooty or ink-like colour is chiefly observed where the head and stomach are simultaneously attacked. When we consider that the blood becomes darker than natural, as well as otherwise changed, early in the period of exhaustion, and that the liver and mucous follicles of the digestive canal, with the kidneys, are the principal organs of depuration, or channels by which the elements producing these changes are eliminated from the circulation, we need not be surprised at the secretions, which these elements go to form, and which these organs excrete, presenting somewhat similar characters. It must however be admitted, that the share which the secretions perform in producing this phenomenon, or that which the exudation of blood has in giving rise to it, will vary much in different varieties or cases of intertropical fevers.—The rapidity with which a dissolution of the tissues takes place after death, in the severe forms of climate fever, deserves notice, as marking the rapidity of vital exhaustion, and as resulting from the changes of the blood; these changes commencing with the stage of exhaustion, and advancing until this fluid is no longer capable of influencing the nervous system, and of preserving the irritability of contractile parts—or until it poisons, instead of exciting, the sensitive and moving tissues." Vol. I., p. 980.

We may here observe that it is scarcely, if at all, possible to perceive any distinction between the *ardent* or climate fever mentioned above, and the *inflammatory* and the *bilio-inflammatory* forms of *remittent* fever, as described by our author. Every symptom, that occurs in the one, is met with in the others, and in exactly the same progression. They are seen under the same circumstances, in the same regions, and are most frequent in the same class of persons, viz. in Europeans recently arrived in hot climates. The only (apparently) recognisable distinction between them is, that the one is spoken of as a continued, whereas the other is described as a remittent, fever. But then this distinction will not always hold, according to our author's own admission. We have already seen that the ardent fever sometimes puts on a remittent type; and now we find that the inflammatory and the bilio-inflammatory forms of remittent fever, are apt at times to become continued. For example, of the former we read that "if the disease be neglected at the beginning, the *remissions disappear*, the skin becomes dry and caustic, or moist and clammy; the pulse small and irregular; the tongue black and crusted; and the vomiting, pain at the epigastrium, &c., more constant. In the most severe and unfavour-

able cases, *yellowishness of the skin, or vomitings of matters like coffee-grounds, or both* occasionally supervene. * * * If the disease be not actively treated at the commencement, an unfavourable termination takes place between the 3rd and 7th day; but it is often prolonged beyond this period, and it then generally occasions visceral disease."

Nearly the same characters are assigned to bad cases of the "bilio-inflammatory form" of the disease: "*the countenance and skin become dusky or yellow;*" and, "after vomiting has continued some time, the appearance of the matters is changed, and ultimately assumes, in fatal cases, the characters just described;" *i. e.* like coffee-grounds.

We need scarcely say that it must be extremely difficult to distinguish such cases of aggravated Remittent (or semi-remittent) from the genuine Yellow fever, more especially as it (the former) also, like the bad form of the Ardent or seasoning fever, chiefly affects new comers into tropical countries, and occasionally prevails there epidemically in unhealthy seasons, as stated by Dr. Copland.

But there is yet another form of epidemic tropical fever described by him as the *adydamic* or *malignant remittent*, which brings us a step nearer still to the very worst cases of the genuine Hæmagastric pestilence. We shall now give his description of the former and the remaining portion of the description of the latter (the earlier portion having already been given in page 190), that the reader may judge of the close resemblance between these "essentially distinct diseases," and also between them and the malignant form of ardent fever, already described.

Yellow Fever.—"In plethoric persons and in the sanguine temperament, the attack is often most violent; and in addition to the symptoms just mentioned, the countenance appears bloated and heavy, with an unnatural expression, or wild and agitated. The heat of the surface, which was at first great and pungent, falls first in the extremities, and afterwards over the whole body, especially after the occurrence of black vomiting; and ultimately it sinks below the ntaural standard. The skin becomes compacted, losing its vascularity, and is insensible to the irritation of blisters. It is rarely dotted with petechiæ, but m uch oftener streaked with yellowish lines, particularly in the course of large blood-vessels, or is covered by patches of a blueish or leaden colour, especially in flaccid parts. The sense of internal distress increases as the febrile action subsides. Distention of the hypochondria, and explosions of flatus from the stomach, are frequent, with occasional obscure hiccuppings. Sometimes the vomitings are hardly complained of until the more febrile symptoms begin to

Malignant Remittent.—"In some cases the vascular excitement is at first more or less intense, with remarkable determination to the head, liver, and stomach, and maniacal delirium, the disease very nearly approaching the inflammatory, or bilio-inflammatory forms. In others, vascular reaction is very low and imperfect; the pulse small and quick; the abdomen tumid and hot, whilst the extremities are cold or clammy; the evacuations foul, morbid and offensive; the tongue fuliginous; the gums spongy, or oozing a bloody sanies; the vomiting constant, and ultimately grumous and dark; the stools, towards the close, black or pitchy; the urine scanty or nearly suppressed; the solids flaccid; and the skin earthy and discoloured. In both these states, a yellowness of the surface occasionally presents itself about the third or fourth day, beginning in the conjunctiva, neck, and breast. The yellowness often passes to a pale greenish hue, in patches, shortly before death; and the soft solids present a liquescent state, having lost their vital cohesion.

abate, when they become unrestrainable: the matters rejected are then muddy or turbid, like unstrained coffee; occasionally they are of inky blackness, like the juice of the cuttle-fish. The evacuations by stools sometimes also present a black appearance at this stage. In the more severe states, the disease frequently terminates fatally within the fifth day. In the less severe cases, signs of an imperfect crisis sometimes appear about the seventh day; and improve to favourable indications; but occasionally they are arrested in their course, and superseded by an unfavourable train of symptoms, as hæmorrhages from the throat, gums, mouth, and sometimes from other outlets of mucous canals. The blood is dissolved, dark, incoagulable or grumous, particularly at far advanced periods of the disease.

"The *fourth form* of the pestilence seems a modification of the symptoms by temperament and habit of body, although the precise conditions of these cannot always be assigned, the phlegmatic, apparently, most frequently exhibiting it. In this the symptoms are not so violent as in the third form, but they are equally fatal. It often commences insidiously, the patient complaining, for hours, or even longer, of nothing but languor or fatigue, which is followed by chilliness or rigor, with pains in the loins and calves of the legs. The headache is not very severe. The pulse is quick and small. The heat of skin is very little increased; but there are great anxiety and oppression at the precordia, and an indifference to surrounding objects. The bowels are obstinately confined, and the secretion of urine is arrested. The tongue is often unnaturally clean, and of a clear shining vermilion colour. Hæmorrhage appears early from the nose, gums, or mouth, and is sometimes attended by petechiæ and vibices. There is little or no thirst, but great irritability of stomach, with hiccough and black vomiting, attended sometimes, as the distemper proceeds, especially towards the fatal close, by an involuntary discharge of the same appearance from the bowels. The peculiar change of countenance, with yelow skin, takes place as in the other

"In other cases of this form, the symptoms are at first mild, and the excitement inconsiderable; when, after two, three, or four, exacerbations, the powers of life appear suddenly exhausted; the pulse becomes weak and fluttering; the tongue foul, black, and dry; the evacuations offensive; the prostration of strength extreme; and the fætor of the perspiration remarkable. At last, great anxiety; tenderness and tension of the epigastrium; fulness of the hypochondria; collapsed features; a squalid or yellowish surface; vomiting of dark or grumous matters, supervene, and indicate the utmost danger. This insidious modification of the adynamic form generally occurs in persons highly predisposed, or who have suffered from bowel complaints, or who are debilitated and are subjected to the more concentrated effluvia.

"In some instances the remittent commences in so mild a form, that the patient is even able to walk about his apartment; and, for several days, complains only of irregular exacerbations of fever, when suddenly, violent and malignant febrile action supervenes, which rapidly exhausts vital power, and either quickly carries off the patient, or induces serious structural change in several of the abdominal organs. In other cases, vascular excitement is hardly manifest at any period of the disease, the exacerbations consisting merely of increased anxiety, restlessness, general distress, and mental depression, occasionally with augmented sickness; and pain in the head, epigastrium, and loins; the pulse being but little accelerated until the close, and the temperature, unless at the epigastrium, rather under than above natural. In these, however, the weak, soft and open or irregular pulse; the dark-coated, or soft, flabby, and lobulated tongue; and the blackish, greenish-brown, and morbid execretions, in connexion with the other symptoms, denote extreme danger. It would seem as if the causes had nearly annihilated the irritability of the moving fibre, and deprived the system of its ability of reacting upon or superseding, the morbid condition induced by their first impression."

states, and is frequently accompanied with a low muttering delirium."

* * * * *

"In the most violent seizures, the patient is suddenly struck, and the distemper proceeds rapidly without reaction, or nervous or vascular excitement, to vital and structural dissolution, with every indication of extreme vital depression, of vascular contamination, and of impaired or nearly lost irritability and cohesion of the tissues." * * *

"When the infectious agent is very powerful relatively to the constitutional powers of the patient, the attack may then be so violent, and its subsequent course so malignant, as to deprive the vital energy of all power of reaction. In this case, the invasion is sudden and severe, and is attended with general tremor, dread, terror, or despondency; the vital depression of this period passing into vital and even structural dissolution, with greater or less rapidity, and either with no attempts at reaction, or with weak and abortive efforts merely." In the majority of cases, however, the stage of vital prostratation is preceded by one of high and violent febrile excitement. Vol. III., p. 140 and 142.

"To this minute description we shall merely add, that this malignant form of intertropical remittent "is variously *modified* in different circumstances and persons. It sometimes assumes more of a *cerebral or typhoid character*; at others, it is *bilious or gastric*, according to peculiarity of season or concentration of the cause. In some intertropical countries it becomes epidemic, or rather this endemic is more than usually prevalent. Occasionally the remissions are indistinct from the commencement, and they generally become so after three or four days." Vol. I., p. 498.

As affording a graphic illustration of a *malignant remittent* in a tropical region, we shall here insert the account of an epidemic fever at Batavia, as given by Mr. Shields, and inserted in Dr. Johnson's work on Tropical Climates. We are the more desirous of drawing the reader's attention to this narrative, from the circumstance of certain writers having inaccurately stated that the fevers of the East never exhibit the peculiar features—we allude to the discoloration of the skin and the black vomit—of those of the West Indies and of the African coast, and too dogmatically asserted that the true yellow fever is wholly unknown in the eastern hemisphere.*

Edam, where the dreadful pestilence was experienced, is a small island, a few miles distant from Java: it is overrun with jungle, and is full of marshes. The time of the year was August, when the sun was nearly vertical. After some introductory remarks, Mr. Shields observes:

"The patient, without much previous notice, was suddenly seized with giddiness and cold chills, sense of debility and vomiting, and with pain over the orbits and in the epigastric region. He frequently fell down, and was insensible during the paroxysm; his body covered with cold clammy sweats, except at the pit of the stomach which always felt hot to the palm of the hand: the pulse was small and quick. On recovering a little, the train of symptoms was succeeded by flushings of heat, increased pain over the orbits and in the sinciput, pain and a

* It is rather singular that one of the earliest appellations of the yellow fever was *Maladie de Siam.*

sense of internal heat about the stomach and præcordia, oppressed breathing; the lower extremities at this time not unfrequently covered with cold sweats. The eyes now become as it were protruded, and the countenance flushed. Retching, and at length vomiting of discoloured bilious matter came on; the tongue was white and furred, and the abdomen tense and full, with pain in the loins and lower extremities. The length of this paroxysm varied from 6 to 18 hours, and was generally succeeded by cold rigors; very often low delirium, preparatory to the next stage or paroxysm of the fever. The intellectual faculties now became much impaired, the patient not being at all sensible of his situation or of any particular ailment. If asked how he was, he generally answered 'very well,' and seemed surprised at the question. This was a very dangerous sympton, few recovering in whom it appeared. In this stage all the symptoms became gradually, often rapidly, aggravated; particularly the headache, pain and tension in the epigastric region and vomiting. Some patients on shore were carried off in 18, 24, 30 or 40 hours, and others not till as many days after the attack, especially when they were removed on board from the noxious air of the island. A great proportion changed in a few days to a bright yellow; some to a leaden colour. Other cases terminated fatally, in a very rapid manner too, without the slightest alteration in that respect. Generally, however, the change of colour indicated great danger. Vomiting of black bilious stuff, resembling the grounds of coffee, frequently commenced early, and continued a most distressing symptom. * * * * * Hæmorrhage from the mouth and nose seldom occurred; in two cases, which terminated fatally, the blood did not coagulate, but tinged the linen yellow. * * * Two kinds of eruption appeared about the lips; one such as we often see at a decline of common fevers; the other consisted of small black or brown spots round the lips, and was likewise a dangerous, indeed a fatal, symptom. With this eruption, the teeth, tongue, and fauces generally became covered with a brown or black coat, and the breath intolerably fætid. * * * In those cases which occurred on board, and where the patient had not slept on shore at Edam, the symptoms were much milder, and the fever resembled more the bilious remittent of other parts of the East."

"Never," continues Mr. Shields, "was there a disease so deceitful as this fever. I have frequently seen instances where every symptom was so favourable, that I could have almost pronounced my patient out of danger;* when, all at once, he would be seized with restlessness, black vomit, delirium, and convulsions, which, in a few hours, would hurry him out of existence! * * * No constitution was exempted from the assault of this fever. It seized with equal, or nearly equal, violence on those who had been many years in India and on the most robust and plethoric, or newly-arrived European. Even the Dutch officers and Malays, who had been drawn from different parts of Java, and whom we had prisoners at Edam, fell victims as fast, or nearly so, as the English."

The mortality was frightful. Almost every individual, who slept on shore at Edam, perished. The disease was certainly not infectious or transmissible from one person to another; "for," says Mr. S., "on our raising the blockade of Batavia, great numbers of sick, in every stage of the fever, were brought on board from the hospital at Edam, yet not a sin-

* Mr. Shields mentions one case, where the patient felt himself on the 2d or 3d day so much relieved that he called for some mutton broth and sago, which he eat with a good appetite; he spoke naturally, and was in good spirits. Towards evening, a change took place, and he was a corpse before morning! Such an occurrence is frequent in the yellow fever of the West Indies. A feeling of hunger in the delusive lull, that so often occurs about the 3d day or so, is well known to be a frequent forerunner of a mortal change.

gle nurse or medical attendant of any description ever suffered the slightest attack; nor did any circumstance transpire that could in the least favour the idea of contagion (infection), notwithstanding that the great accumulation of sick on both decks rendered it a matter of impossibility to separate them completely from those who were well, nor at all times to prevent a considerable generation of effluvia."

So much for the phenomena exhibited during the life of the patient in a malignant form of the remittent fever of an intertropical country. Let us now compare the *post-mortem* appearances, as detailed by Dr. Copland, in the two diseases between which there is so close a resemblance.

Yellow Fever.—"In the most malignant and rapidly fatal cases, the *muscles* are softer and flabbier than natural, of a dirty or dusky hue, and are easily broken down by pressure. The substance of the *heart* is similarly changed. * * * The *liver* is changed chiefly as regards its cohesion and degree of congestion. It is almost always softer and more friable than natural; in some cases congested, in others pale. * *

The *spleen* and even the *pancreas* are somewhat softened; and the former frequently congested. * * * The epithelium of the *digestive mucous* surface seems to be more or less detached in the several portions of the canal; and the mucous membrane is softened and readily separated from the adjoining tissue. The *follicular glands* are not prominently affected, further than being somewhat enlarged in some instances.

"In those cases which present congestion of the chief organs, as of the brain, lungs, auricles of the heart, liver and kidneys, slight *serous effusion*, sometimes sero-sanguineous, is occasionally also found in the chief cavities, particularly the *pericardium* and *arachnoid*, and but rarely in the *peritoneal* and *pleural* cavities." Vol. I., p. 143.

Malignant Remittent Fever.—"The substance of the *heart* is frequently soft, flaccid, and readily torn, the cavities being occasionally dilated, more especially after the adynamic states of the disease. * * * * The *liver* is usually injected, remarkably softened, of a dark colour, friable, and sometimes enlarged. The *spleen* is often so soft as hardly to admit of being handled."

The *digestive mucous surface* is softened, injected, ecchymosed, of a dark hue, and sometimes thickened, abraded, or even ulcerated in the lower parts of the canal. The *mesenteric* glands occasionally, and the *pancreas* more rarely, are enlarged or otherwise changed.

The *lungs* are sometimes congested, infiltrated, condensed, or inflamed. The *pleura* and *pericardium* often contain some dark sanguineous serum. * * * * The changes *within the cranium* consist chiefly of congestion of the veins of the pia mater and sinuses, with a fluid dark blood, and sometimes of *effusion of serum* into the ventricles and between the membranes. Vol. III., p. 950.

It may not be unprofitable to introduce here, for the purpose of comparison with the details now given, the most remarkable *post-mortem* appearances described by our author as being usually found in the bodies of those who die from a rapidly fatal attack of Ardent or Seasoning Fever.

"The *digestive mucous surface* is studded with numerous dark or ecchymosed spots, from which a fluid black blood seems to ooze. The *liver* is frequently congested, sometimes larger and softer than natural, and of a dark colour, owing to the quantity of black blood in its vessels. The *spleen* is somewhat enlarged, soft, and friable; and the omentum injected. The serous as well as the mucous surfaces, especially in the abdominal cavity, often present livid or dark patches. The *blood* is everywhere fluid, black, and dissolved. The internal surface of the *heart* and large vessels, both arteries and veins, was of a dark red or livid tint in

a few cases which I examined; but this point requires further investigation." Vol. I., p. 978.

The most inattentive reader cannot fail to observe how remarkably alike the post-mortem appearances are in the three diseases to which reference has been made. It is surely unnecessary to add a word in the way of comment. We shall therefore now proceed to make a few remarks on a point connected with the history of Yellow fever, on which great stress has been laid, by many of the ultra-infectionist party, as indicating, they contend, a marked difference between it and all malignant remittents.

That genuine yellow fever ever exhibits anything like distinct remissions in its course is emphatically denied by Dr. Copland; and so convinced is he of the truth of his opinion, that he even goes so far as to assert that, whenever this feature is observed, the existing disease may at once be set down as not the genuine pestilence. This mode of reasoning has certainly the merit of boldness; for it unmistakeably declares in very simple language the point to be determined. So far it is well; there can be no mistake as to the *q. e. d.* It is obvious, however, at the same time, that the difficulty of disproving the position is considerable; for the argument resolves itself into a mere unproved assertion. It would be easy, indeed, to adduce a multitude of most respectable authorities in flat contradiction of the statement in question; but these, we shall straightway be told, are of men like Bancroft, Johnson and other infectionist writers, whose object it is to show that there is no essential or infallible distinction between malignant remittent, and genuine yellow fever. Dr. C. forgets, however, to tell his readers that some of the most zealous advocates of the infectiousness and importability of the disease have declared the very same thing. He knows, for example, that Dr. Arejula, who saw so much of the yellow fever in the south of Spain in the beginning of the present century, and whom he is so often glad to cite as a most competent witness respecting it, expressly says, that the disease "without doubt deserves the name of remittent fever." Dr. Balmis, too, whose experience was very large, called the disease, as he saw it at Cadiz in 1800 (the genuineness of that epidemic is more than once admitted by Dr. Copland), "a putrid malignant remittent." The same view was held and expressed by various others of the Spanish physicians, almost all of whom, it is to be remembered, were out-and-out infectionists and importationists. Several of them, Arejula among the number, have even asserted that the disease sometimes assumes an intermittent form. "In conclusion," we quote from Dr. Gillkrest's excellent article in the Cyclopædia of Medicine, where ample particulars respecting the opinions of the Spanish physicians will be found, "on this part of the subject, it may be stated that the records of the Gibraltar yellow fever epidemics furnish the following names in support of the fact, that remissions not unfrequently take place in this disease—Drs. M'Mullin and Browne, Messrs. Sproule, Wild, Martindale, Amiel, Daw, Donnett, Humphries, Lee and Hugh Frazer." Dr. Copland himself admits in one passage that, "slight exacerbations are sometimes remarked in the evening and ameliorations in the morning; but these are rarely so considerable as to amount to remission." True; but then does he not over and over again tell us that, in the bad forms of endemic fever, the remissions are sometimes scarcely, if at all, perceptible? Nay, according to his

own statement, this is of not uncommon occurrence; for, in one part of his narrative, we read that "the passage of that type (malignant remittent) into the continued is extremely frequent, especially when vital organs become more and more implicated, and when the disease increases in prevalence, so as to assume an epidemic character; the fever, with this change of type, presenting in the worst cases the chief features of the Hæmagastric pestilence." Again; "there is generally much difficulty in distinguishing the malignant remittent of Africa from this pestilence, owing to the *imperfect remissions of the former*, and to the presence, at an advanced stage of fatal cases, of many of the symptoms characterizing the early and rapid progress of the latter." It would be easy to quote passage upon passage, from the writings of our most experienced army and navy medical officers in the West Indies, in illustration of the point in question. One will suffice: it is an extract from the official report of Sir D. Dickson, the late able physician to the Leeward Island Fleet, and is quoted in Dr. Bancroft's work.

"At Barbadoes and Antigua, I had generally seen the disease of an ardent *continued* form, and did not fully understand why authors talked of a bilious *remittent* yellow fever, until after the capture of the French and Danish islands. But the anomalies of fever, the shades and changes which it assumes according to the intensity of the exciting causes (while these were purely and solely local,) the state of predisposition or the spot of residence, could nowhere be more strongly portrayed than in the destructive epidemic of Marigalante in the Autumn of 1808, from the most concentrated marsh miasmata; when the different types of fever were controverted into each other, of the worst and most aggravated species I have ever witnessed. Some were affected with the highly concentrated yellow fever in the *continued* form; others with comatose *remittents* or *intermittents*, the exacerbations of which were so violent as to carry off a patient in two or three paroxysms; while others sank into a low protracted character of fever resembling *typhus*."

Need more be said to show how utterly vain it must be to attempt to build any satisfactory distinction between malignant remittent and genuine yellow fever upon the character of the continuedness or periodicity of the febrile symptoms? Surely not. We shall pass on to the investigation of another point, on which also there has been no inconsiderable discordancy of opinion among medical writers.

The point to which we allude is that respecting the non-liability to second attacks of yellow fever, and the value of this character or attribute as a diagnostic sign between it and the other fevers with which it is apt to be mistaken. Dr. Copland goes so far as to assert that one attack of the genuine disease "protects the system more decidedly from a future seizure than even Small-pox or Scarlet Fever from a second attack of these maladies." This is certainly a somewhat exaggerated statement; but we shall concede its truth just now for the sake of argument; and we admit, at the same time, that second attacks and relapses of common marsh Remittent fever are very frequent. Was the question simply and entirely between this fever on the one hand and well-marked severe Yellow fever on the other, there could be no difference of opinion upon the question we are at present considering. A second attack of the latter is a very rare occurrence indeed, and that of the former is a very common one. But then have we not already been told that second attacks of the Ardent or

climate fever are also very rare ; and that, when such an occurrence takes place, from the circumstance of the person having intermediately returned to a temperate climate, this disease generally assumes on its second invasion a remittent type ? This admission has very important bearings upon more than one of the controverted points in the present discussion. For example, it is very confidently stated by our author, when speaking of some of the epidemics of yellow fever at Gibraltar, that none of the garrison enjoyed immunity from its attacks, except those who had had the disease before, either in the West Indies or in Europe. Now we should like to know how he learned so satisfactorily that all these protected persons had had the genuine disease, and not that very many of them, probably a large majority, had had, on the previous occasion referred to, the mere seasoning fever of new comers into a tropical climate ? Not a particle of evidence is adduced to prove the fact as stated; its truth rests solely upon the testimony of the parties themselves. Can any impartial person, acquainted with the history of disease, be satisfied with such evidence ? We have already seen how difficult it is, even for a medical man, to discriminate the one distemper from the other. What reliance then can be placed upon the mere affirmation of unprofessional persons as to the real nature of a febrile disease, which they may have experienced at some previous period ? Is it not the case, too, according to our author's own showing, that the majority of practitioners in the West Indies and elsewhere are continually mistaking other fevers for the genuine pestilence ? If the patients, therefore, derived their information from their medical attendants, the probability therefore is, that it was not the true yellow fever that they had suffered from.

And this probability is surely much increased, when we call to mind Dr. Copland's opinion that, while the Ardent, the Bilio-gastric, and the various forms of Remittent fever are endemic, and therefore of continual occurrence, the yellow fever occurs only occasionally (at intervals often of several years), and then generally as an epidemic.*

Is there not therefore the strongest presumption that, of the 6000 persons who, during one of the Gibraltar epidemics, escaped the disease in consequence of having had it previously in the West Indies or in the South of Europe, the previous disease was in short nothing more than the climate fevers of these countries ? At all events, we must have something more than the mere assertion of the fact, before we can admit our author's opinion, that in all these 6000 cases the primary fever was his genuine pestilence, as contradistinguished from all other intertropical fevers.

Here too it may be worthy of notice that, although relapses and second attacks of common Remittent fever are abundantly common, we find no evidence to show that second attacks of the malignant form of this fever have ever been met with. It may suit the convenience of a one-sided dis-

---

* "I doubt much," says Dr. C. "his (Dr. Bancroft) having seen a case of true hæmagastric fever, when he published his Gulstonian Lectures, delivered at the College of Physicians in 1805." Are we to regard this remark as an illustration of the comparative rarity of the disease; or is it merely a little explosion of angry feeling ? On either view, it will not surely aid our author's argument.

putant to make the general assertion respecting Remittent fevers in the lump; but this will not do with any one accustomed to sift the ambiguities of medical testimony. Not only have we no proof of any second attacks of the aggravated form of remittent fever, we mean that in which there is yellowness of the skin, and any tendency to black vomit; but there is also an utter lack of evidence to prove that any one, who has recovered from the disease in question, has ever subsequently been attacked with yellow fever.

It is altogether amusing to watch the conjectural shifts, to which the adoption of extreme and exclusive opinions upon an intricate question of medical evidence leads even the most experienced. For example, it is laid down by Dr. Copland, with all the confidence of a well-ascertained fact, that, while the Ardent fever is liable to be followed, upon recovery, by different forms of periodic fever, the genuine Yellow fever never is; but then we find this very important admission at the same time, that "as it (the latter) usually prevails in situations where Remittent fever is endemic, and in seasons when this is most prevalent and malignant, recovery from the malady is extremely likely to be followed by an attack of the latter; and, more than this, the one disease is very liable to be mistaken for the other." This is certainly a very easy way of getting rid of an objection, that has often been urged with force against the opinion of the essential and specific difference between Yellow and Remittent fevers; but will it satisfy any inquisitive mind? Again, we read in another passage, that "most of the instances of recovery which we hear of from the black vomit, are recoveries from those states of malignant bilious or remittent fever which have been confounded with the pestilence." Here, then, we have another element of perplexity introduced into a subject, intricate and involved enough in all conscience; for is it not possible that the few of the 6000 exempt persons, mentioned above, who may have been previously affected with a fever attended with black vomit, had not had the genuine yellow fever, but merely a malignant form of the endemic remittent? It is obvious, therefore, that according to the testimony of Dr. Copland himself, we cannot be warranted in admitting, as an argument for the non-liability to second attacks, the very strongest of all the cases that he is able to adduce. As we shall afterwards have occasion briefly to allude to this matter again, in commenting upon some of the statements of Sir William Pym, we need not say more upon it at present.

Before proceeding to make any remarks on the alleged essential and invariable *infectiousness* of Yellow Fever, we shall avail ourselves of this opportunity to notice Dr. Copland's views respecting its ætiology. The true and only cause of the disease is declared by him to be "an infectious or contagious poison which, however formed originally, infects the healthy by contaminating the air immediately surrounding those already affected, or which, being absorbed and retained by other bodies, is afterwards given out from them on exposure to the air, thereby contaminating and infecting the air and adjoining objects." This morbific poison is alleged to arise "independently of endemic or terrestrial sources or malaria," and, in almost all cases, to be an emanation of effluvium from the body of a person already affected with the disease. Dr. C., as a matter of course, feels at a loss how to account for the origin of the first case during an epidemic visitation; for it is obvious that there must be a *fons et origo*

mali somewhere, independent of personal transmission. But even this consideration will scarcely shake the settled conviction of his mind as already expressed; nay, it even serves, in a most unexpected manner, to establish it the more; for thus he reasons:

"It has appeared in one or two isolated persons who had not previously breathed the foul air of sick wards or apartments; nor visited the places in which either the sick or the healthy had been confined; nor inhaled the effluvium from decomposing exuviæ and animal substances; and thus, by the exclusive process of reasoning, we have left only that cause to which I have imputed it; and by means of which it is undoubtedly propagated and perpetuated as any other malady whose infectious properties are admitted." P. 177

But we must not forget to state that, while our author rejects the idea of the true pestilence ever originating from the operation of any local or endemic malaria, he is not without his own hypothesis on this difficult subject. It is broached in the following passage:

"That the contamination of the air, especially when it is humid, warm, and close, either by other fevers, or other maladies, or by a number of persons previously in health confined in it, will take place, so as produce fevers of a malignant character, more especially that fever which I have called PUTRO-ADYNAMIC (see FEVER, §§ 484—496), I have shown when treating of that malady (§ 496); but satisfactory proofs are wanting of this pestilence ever having originated in this way. Since my visit, however, to several places in Africa, and knowing the very limited space in which a large number of slaves are often confined, both on shore and in slave-vessels, I entertained the idea that this pestilence or its seminium, or specific infection, had been generated originally by the congregation of negroes in a close atmosphere, or is generated *de novo* by thisrace when placed in the circumstances now stated; and that, although it affects them in a comparatively slight manner, it is most particularly baneful to the natives of cold countries; as small-pox is comparatively mild in the white races, whilst it is most pestilential and fatal amongst the negroes. This opinion, entertained since 1817, I have endeavoured to ascertain the truth of whenever I have had an opportunity of making any inquiry respecting it; but the evidence is not sufficient to establish this as the source of the infection. The following, however, may favour the truth of this idea. A small vessel in which I was a passenger was anchored, in May of 1817, a short distance from Sierra Leone; and the ship's boat with four of the crew was bringing me on board when a tornado suddenly overtaking us we took shelter on board of a ship recently brought into the harbour full of slaves, and near which we were at the time. The men belonging to the boat took shelter down between the decks. I remained under a small poop on the quarter-deck. All these men in two or three days were seized with this distemper, the vessel having just put to sea, and I escaped. The sick men were constantly kept on deck, free ventilation was enforced, and every possible precaution under the circumstances was used, and no more were attacked." Vol. III., p. 175.

Here the distinct impression is left upon the reader's mind that it is only the black population that is capable, under the circumstances supposed, of originating the disease. But that malignant and pestilential fevers of an highly infectious nature, and exhibiting all the characters, if not possessing the nature, of the true yellow fever, may be generated in hot and sickly climates by the crowding together of other races of men without due ventilation, had already been admitted by Dr. C.

"Some of the epidemics of yellow fever said to have occurred in the West

India Islands, and in parts of the American continent, have been much milder than the visitations of this pestilence have been in Spain, during the early part of the present century. But it is by no means fully ascertained whether all the epidemics observed in the western hemisphere were actually the true yellow or hæmagastric fever, or merely an unusual prevalence of endemic or remittent fever, rendered more continued by intensity of attack, predisposition of the affected, and other circumstances. No doubt several of these epidemics were the pestilence under consideration. Their symptoms, remarkable prevalence, and fatality proved that some of them were this distemper; but others were of a different nature; and probably some of them resulted from the crowding of a number of human beings in a confined space, either in barracks, or in transports, or between the lower decks of ships of war, in a high range of temperature, and without sufficient renewal of the air." Vol. III., p. 149.

How much it would have tended to elucidate this perplexed subject, if Dr. Copland had specified which of the epidemic visitations, alluded to, he regarded as belonging to the genuine pestilence, and which he looked upon as belonging to other forms of fever! As the matter stands, it is not possible to establish any comparison between the one set and the other, and thus seek to discover their prominent points of difference, if any such really existed in nature. But, without dwelling upon this point, and adverting only to the (supposed) negro origin of the genuine disease, one or two difficulties will immediately suggest themselves to the reader's mind. How comes it that we do not hear of the disease (so, at least, it is asserted by our author) "in the eastern hemisphere, and on the shores of the Pacific?" Have not many crowded slavers arrived, at different times, at the Mauritius and the Isle of Bourbon? and is there not a regular slave trade carried on in the present day at the island of Madagascar, where, nevertheless, the true yellow fever is said never to have been known? Apart, however, from these considerations, is there, we would ask, any other example of an essentially specific disease, a disease *sui generis*, and proceeding, as it is declared, from a morbific *seminium* or virus analogous in its nature and peculiarities to that of Small-pox or of Measles (for all this is asserted), being produced by, and originating from, the mere accumulation of human beings under any circumstances? Are any of the Exanthemata ever so developed? or is there any reason to believe that the other two pestilences described by Dr. Copland—viz: Epidemic Cholera and the Plague—ever originate from the cause mentioned, apart from the operation of certain endemic malaria! Again, is it not somewhat surprising that a disease, (supposed to be) generated by negroes, should exert its infectious properties comparatively so exclusively upon the white races of mankind, and more especially upon those who have recently arrived in a tropical region? Is there any analogous instance of such a limitation on the part of an essentially infectious disease? We surely know of none. Small-pox commits its ravages with equal fatality among all races of the human family, who have not been protected by vaccination; and the same thing may be said of Measles and Scarlet fever. But it is very different with the Yellow fever. The negro races, it is well known, although certainly far from being exempt from its invasion, seldom suffer so severely from it, as those whose systems are less acclimated to the heat and malaria of a pestiferous climate. Does not this circumstance alone point to some connection, in nature as well as in cause, between this and

the ardent or seasoning fever? We have already seen how closely a malignant case of the latter resembles, in all its features, the most fatal yellow fever.

But passing from these considerations, we should like to be told how the Malignant Remittent, when it assumes a continued type and acquires infectious properties, is to be distinguished from the genuine Pestilential disease. This dangerous modification of the former is apt to take place in certain seasons, when the endemic fever prevails in an epidemic form, and especially when the vital organs and the circulating fluids are more seriously affected than usual. Under such circumstances, it is unquestionably liable, as Dr. Copland allows, to become infectious whenever a number of the sick are crowded together in badly-ventilated places, "during very warm and humid states of the air, so as to contaminate the surrounding atmosphere, and thereby either to superadd the additional cause of a morbid effluvium, exhaled from the sick, to existing marsh miasma, or to generate a morbid poison or vapour which is of itself capable, independently of marshy or other miasma, of infecting the healthy and disseminating the distemper." "That a superadded quality," our author goes on to say, "or at least a change of character, should result from the circumstances just alluded to, may be rationally inferred; for the aggravation of symptoms and the development of new features in these altered circumstances have frequently been observed, are undoubted, and are the chief sources of much of the differences of opinion and of the discussions which have appeared, since the end of the last century, on the subject of the Yellow fever." Yet he afterwards, in more passages than one, appears to argue as if the possibility of the endemic fever ever becoming infectious was entirely out of the question. For example, when combating the doctrines of the venerable Jackson on the subject, he writes thus:—

"He (Dr. J.) remarks that 'there is not one practitioner in one hundred who has resided for years in the West Indies, who believes that the concentrated endemic of that country, usually called the yellow fever, is a disease which possesses the power of propagating itself from person to person within the tropics.' Certainly there is not. It is well known that all the writers on West India diseases during the last and present centuries admit this, but many of them—nay, the majority—also admit, what is the fact, that the severe endemic of that climate is not this pestilence; that the former is liable to be mistaken for the latter; and that both are often confounded together, although they are as distinct, indeed more distinct from each other, than measles and small-pox. And in this Dr. Jackson errs with the minority, using at the same time terms which involve a theory, or mean nothing. Thus his 'concentrated endemic' must either mean the more malignant form of remittent, which I have described from frequent observation of it in warm climates, in the *art.* FEVER, by the name of *malignant remittent*, and which I know well *is neither infectious*, nor the pestilence now under consideration, the differences between which have been long since pointed out by many very intelligent and experienced observers." Vol. III., p. 164.

Here Dr. C. makes the broad unqualified assertion that the Malignant Remittent (which, we have already seen, he admits not unfrequently assumes a continued type) does not exhibit infectious properties; and, in another passage a little further on, we find him laying hold of an extract from Dr. Jackson's writings, wherein allusion is made to the effects of crowding the sick together upon the diffusion and aggravation of the dis-

ease, to found a charge either of ignorance or of unfairness against this most esteemed physician. We leave the reader to judge for himself from its perusal:

"The yellow fever," says Dr. Jackson, "during the reign of epidemic influence often strikes like a pestilence by the mere concourse of people in a close place; and if a mass of sick persons be collected into an hospital during the epidemic season, the common emanations from the sick bodies, whether saturated with contagious particles or not, often act offensively on those who enter the circle, and often appear to be the cause of the explosion of a disease which, without such accessory or changed condition of the medium in which men live, would have probably remained dormant for a time, and perhaps for ever. The instances of persons who have lived in apparent good health in simple epidemic atmospheres, and who have become sick soon after they entered into the circle of a crowded assembly, or the crowded wards of an hospital of sick, are numerous, and so well marked, that they stagger, on a superficial view, the opinion here contended for, of the non-contagious nature of the yellow fever.' To be sure they do, and being admitted by Dr. Jackson, they become evidences of infection as strong as 'proofs from holy writ.'" P. 165.

Indeed! Has not Dr. Copland himself told us that the Malignant Remittent may become infectious, "when a number of the sick are crowded together in badly-ventilated places, during warm and humid states of the air?" and has he not admitted the very same thing in respect of his "bilio-gastric fever," another denizen of those very regions where the yellow fever is most often to be met with?

We therefore see, from the data supplied by our author himself, that the character or attribute of *infectiousness* cannot be fairly set down as a diagnostic or discriminating feature of the genuine Pestelential fever; and thus the presumption that this attribute is not a uniform or unnecessary, but rather a contingent and occasional, property of the disease appears to be rendered more and more probable. We shall afterwards see how far this view is borne out by the history of the Eclair fever. Meanwhile, let us keep our minds as free as possible from all extreme and exclusive opinions, taking part neither with those who can see no cause save that of personal transmission in the work of morbific dissemination, nor yet with the ultraists on the opposite side, who have unguardedly asserted that Yellow fever is never propagated in this way.

What has been said by an able writer in the Dictionaire de Medecine et de Chirurgie, *art. Contagion*, of the labours of the late indefatigable M. Chervin in reference to one epidemic of the pestilence, may be applied with strict truth to almost every other one of which we have any published account:

"Observe, in regard to this last subject, (viz. the error of attributing to infection what is often referrible to local causes), what occurred respecting the yellow fever epidemic of 1821 at Barcelona. Read the work of the French medical commission appointed to examine into that epidemic, and it will be impossible for you, (admitting as true the statements therein contained), not to admit the existence of infection. But afterwards, when you have read the documents collected by Dr. Chervin with a degree of zeal and patience truly admirable, you will be convinced that the circumstances, which led you to be of the same opinion with the commissioners as to the reality of infection, are anything but conclusive."

So necessary is it to weigh and balance all things together, before we form our opinion.

There is a subject, connected with the occasional outbreaks upon a large scale of the Yellow fever, that deserves much attention, but which like every other point in the history of this perplexing disease is surrounded with difficulties. Is it truly endemic, or does it occur only occasionally, in those countries where it usually appears? in other words, does it exist there in a partial or sporadic form in all years, just as the malignant Cholera does in the East Indies, or as the plague does in Egypt? or is it entirely extinct, save and except when it appears epidemically, as is usually the case with exanthematous fevers, viz. Small-pox, Measles, or Scarlatina? Dr. Copland seems to adopt the latter opinion; for he says, when pointing out the diagnostic characters of the inter-tropical fevers that are so apt to be mistaken for each other, that "the *remittent* is endemic in warm climates, and in several temperate countries in warm seasons, especially those abounding with the sources of malaria; the *ardent* fever occurs only among persons who have recently arrived from cold or temperate climates into a very hot country; and *true yellow* fever appears only occasionally; and then the infection may either extend to a few only, the circumstances favouring its diffusion not existing, or to great numbers, the disease thereby becoming epidemic. Thus the *first* and *second* of these fevers, are always occurring, especially the first; the *third* seldom, or after long intervals.

Now here is a declaration in express contradiction of the opinion held by at least nineteen-twentieths of the medical men practising in the West Indies. That not a year passes over without a few sporadic cases, at least, of yellow fever occurring in those places that have usually been the scene of its epidemic visitations, is admitted by an overwhelming majority of those who have resided long in such districts. Indeed, we strongly suspect that Dr. Copland himself, notwithstanding the above quoted opinion—which, we should mention, occurs in the 1st volume of his Dictionary, published 12 years ago—is now by no means satisfied of its correctness; for in the very last Part, when alluding to an outbreak of the disease at Port Royal Jamaica, we find him saying;—"it should be premised that the infection of this pestilence had lurked for several years, *or even longer*, in the most frequented sea-ports of the West Indies, as Port Royal, the Havannah, Vera Cruz, &c." We may mention also that some of the most zealous infectionists, as well as others of the opposite party, have strenuously maintained that the disease is never entirely extinct in these and such-like places.

The casual mention of this point naturally leads us on to the consideration of another, which has given rise to a vast deal of learned and elaborate discussion. The question to be determined is, whether the so-called genuine Yellow fever has been always an indigenous product of the West Indies, or whether it was conveyed thither, towards the close of the last century, from Bulam on the western coast of Africa; the malignant fevers that may have previously existed in the western hemisphere being, on this supposition, believed to have been only aggravated forms of the endemic remittent, but not belonging to the true specific pestilence which the appellation of the Bulam or black-vomit fever should, it is said, be employed to designate. Few indeed, in the present day, entertain the latter of these

opinions; but, when it was first brought forward in 1794 by Dr. Chisholm, the confidence of his assertions, and the fortuitous outbreak of the pestilence in many of the West India islands after a lull of several years, had the effect of making a good many converts to his new and startling doctrine. Sir William Pym has always been one of its warmest espousers. We are glad, however, to find that Dr. Copland, albeit so energetic an advocate of the specific and essential difference between the genuine yellow fever and every form of endemic tropical fever, repeatedly and unequivocally acknowledges that the former had frequently broken out not only in the West Indies, but also in different parts of the United States, during the last, and even during the 17th century; and, although he refers more than once to "the re-appearance (in 1793) of the malady in the West Indies after an immunity from it during many years," he judiciously avoids all allusion to the often-repeated assertion by certain ultraists that it was then imported for the first time from Bulam, on the African coast, by a vessel called the Hankey. As he therefore, with all his strong bias on the subject of importation, evidently considers the arguments of these gentlemen utterly unsatisfactory, but his not even so much as alluding to the name of this vessel, it may be very fairly presumed that the point in question is altogether untenable, and is not likely ever again to become a topic for much difference of opinion.

That there was an outbreak of the disease, in an unusually severe form in the West India islands in 1793, after many years of comparative immunity or rather mildness, cannot for a moment be doubted. The question of dispute is, whence did it come? Dr. Copland, as we have already stated, wisely avoids giving any definitive or peremptory reply. He merely says that it *re-appeared*. It broke out, at that time, first in Grenada; but not a word is said by him as to the producing, far less of any imported, cause of it in that island. He leaves indeed the impression on the reader's mind although he does not make any positive assertion, that it spread thence by infection to the other islands of the West India Archipelago, and to different parts in the United States. Without canvassing this opinion at present, we shall merely remark that the course which the disease seems to have pursued in its travels, appears to be any thing but very consistent—seeing that there was no restriction to free intercourse between the different islands—with the supposition that personal infection or direct transmission from one individual to another was the sole or principal agent in its dissemination. The pestilence broke out at Grenada in February, 1793. In July of the same year it made its appearance in Dominica, and in the very same month, at Philadelphia,* many hundred miles distant. Barbadoes remained unaffected until the beginning of 1794, and St. Domingo did not suffer until late in the course of that year; when it seems to have reached most of the West India islands. But it would be unprofitable to attempt to pursue the examination of a subject, on which such contradic-

* There seems to be no little difference of opinion as to when the disease made its appearance at New York, during this epidemic. Dr. Copland quotes the testimony of two gentlemen, Drs. Charlton and Bard. The former says that it appeared there in 1793, while the latter states he never saw a case of it until 1795.

tory evidence has been brought before the public. Even with respect to the cause of the occasional appearance of the pestilence at Sierra Leone and other parts of the west coast of Africa, no little contrariety of opinion has been expressed by different medical writers. While Chisholm and Pym maintain that Bulam is the true birth-place and nursery of the disease, the late Mr. Fergusson wrote an article* to prove that it is unquestionably indigenous in Sierra Leone, 130 miles distant, and that it is generally transported from that pestiferous place to other points on the African coast. On the other hand, of the two epidemics which prevailed there in 1823 and 1829, Dr. Copland expresses his opinion that they (at least the latter) did not originate in the colony, but had been imported:—from what place, he does not say. He rests his opinion on the report of the then governor, Major Ricketts, "who," our author says, "has thrown light upon a subject which they (the colonial surgeons) have confused and mystified." Allusion must here be made to Mr. Boyle, who wrote a work upon the subject.† In that work, after adducing a variety of details, this gentleman says:—"The foregoing information appears to be so conclusive as to the origin and original nature of the disease, that further discussion upon these points must be unnecessary. That there are differences in the accounts given by the natives is true; but they do not amount to discrepancies of such a magnitude as at all to invalidate or shake the one leading supposition and conviction, *viz.* that the epidemic fever, which raged so fatally in Freetown in 1829, was immediately caused by peculiarities in the seasons, originated in the interior, was borne to Freetown by the North-east winds, and, in its primary and true character, was not contagious."

With respect to the former epidemic, we learn from Dr. Gillkrest that Mr. Showers (who was resident Colonial Surgeon from 1816 to 1826) has stated that, when the fever broke out in 1823, it was supposed by some to have been imported thither from the Mediterranean, by a vessel called the Caroline! Mr. Showers, however, was of opinion that the disease proceeded from the atmosphere. Well may Dr. Gillkrest say, when alluding to this matter, that "to those who had been led to believe that the true black-vomit fever had been not unfrequently *exported* from the coast of West Africa, its reputed birth-place, this visitation as a perfect stranger, and its alleged importation from Europe, must appear somewhat strange."

It is remarkable how unwilling the residents of a place often are to allow that a pestilential disease has originated among themselves: they will generally assert that it has been brought *ab extra* to them.

Dr. Copland's views, as to the origin of these two epidemics at Sierra Leone, may be gathered from the following passage:—

"Although persons belonging to the colony had visited places adjoining where the pestilence was raging at the time, had returned to the colony, and were immediately afterwards attacked and died, other cases of the disease following upon these admitted to have been the first; and, although ships, more especially slave-ships, with sickly cargoes of human beings, arrived at the colony just before and at the time of the outbreak of these epidemics, some of the writers on the subject contended that the distemper had arisen from malaria brought from a distance by

* *Medical Gazette* for August 31, 1839.

† *Account of the West Coast of Africa, &c.* London. 1831.

the winds, and others concluded that it had travelled from the interior of Africa to the coast—a sufficient admission of infection; whilst the more observant of the residents believed in its introduction through one or other of the channels just indicated, or through both." Vol. III., p. 172.

The reader will naturally inquire whence came the pestilence to the *adjoining places*, from which the colony is supposed to have received it? The mention of "the slave-ships with sickly cargoes of human beings," has reference, doubtless, to the hypothesis of the origin of the disease, which has been noticed in a preceding page. From the coast of Africa, we now pass to the shores of Europe.

Without entering upon the angrily-disputed field of inquiry as to the probable origin of the several epidemics of yellow fever, which have made their appearance in different parts of Spain during the course of the present century—and what good could result from such an investigation, when we have not a single datum or position that has not been flatly contradicted?—one or two remarks may not be without some interest and even profit to the reader.

The evidence, adduced by Dr. Gillkrest to show that many parts of the Peninsula had repeatedly suffered from visitations of this fever in the course of the last and the preceding centuries, and probably also at a still earlier date, cannot, we should think, be resisted by any unprejudiced person. Sir Gilbert Blane himself expressed his belief that it was known at Cadiz in 1764; and Dr. Copland not only alludes to this case, but also mentions that the pestilence "appeared at Lisbon in 1723, black vomitings being the most prevailing and fatal symptom." With such an admission, it seems rather strange that he should afterwards talk of "the first appearance of the pestilence" at Gibraltar in 1804; and, notwithstanding this too, the particulars mentioned by Dr. Gillkrest of the visitation in 1796 (not to allude to any earlier ones), when "Dr. Harness, then physician to Lord St. Vincent's fleet, and afterwards one of the Commissioners to the Sick and Hurt Board, declared the fever to be precisely the same he had seen in the West Indies;"—a statement that is confirmed by Dr. Moseo in his Work on Diseases of Armies.* It may be mentioned also, that Dr. Copland himself acknowledges that the pestilence was at Cadiz and Xeres in 1800, and at Malaga in 1803, places not above forty or fifty miles east and west from Gibraltar. He says that the escape of the "Rock" in these years was owing to the quarantine regulations that were adopted. Yet he is obliged to confess that, in spite of these regulations, enforced too, be it remembered, under the vigilant eye of Sir W. Pym, then superintendent of quarantine at Gibraltar, it found its entrance in the following year, 1804.† Such was the case also in 1810 and 1813; and again at the last visitation, that of 1828; "the quarantine," says our author, in reference to the vessel called the Dygden, that was believed to have imported the disease from the Havannah, "appears not to have been rigidly enforced." Alas!

---

* We might allude also to the well-authenticated case of the yellow fever prevailing on board of the Bedford man-of-war, 74, in the bay of Gibraltar in 1794.

† Dr. Copland tells us that Sir W. Pym met with *one case* of the distemper in this fortress (Gibraltar) in 1803.

the watchmen seem always to be asleep, at the very moment when the enemy is ready to enter!* Dr. Copland gives a minute and most circumstantial account of the exact mode in which the pestilence was introduced by that unfortunate vessel, and he even traces each successive step in its stealthy progress "from the smuggler and washerwoman to whom the foul clothes had been sent," the first affected, until its full-blown development, when it prevailed to a fearful extent in almost every part of the garrison. It is surely very strange, to use the gentlest expression, that he does not even so much as allude to what Dr. Gillkrest has written on the subject, although this gentleman was on the spot at the time of its outbreak, and had an opportunity of watching its rise, progress, and termination. Courtesy alone might have prompted Dr. C. to have referred to the statements of an honourable opponent, writing in what may be fairly deemed a rival work. Any one, who reads Dr. Gillkrest's apparently most fair narrative of this epidemic, will hesitate before he receives our author's circumstantial details, if he does not at once wholly discredit them. If we are to give credit to Dr. Gillkrest's statement that "the failure of proof as to the importation of the disease has been admitted by the Army Medical Board in England, to whom a copy of all the proceedings of the Gibraltar Commission was sent for examination," can we require any further reason to make us pause in our decision upon the matter, and not hastily to adopt an opinion, one way or the other, merely because it may happen to coincide with our previous and favourite notions? These remarks must suffice upon this part of our subject. The fair and legitimate conclusion may surely be asserted to be that the yellow fever was not a *nova pestis* introduced, for the first time in 1703, into the western hemisphere from the African coast, and re-exported from the former, in some subsequent year, into the south of Europe. Yet this opinion is still most tenaciously held by one, if not by more, of the veteran writers upon the disease! We are glad to find, as already stated, that Dr. Copland does not give the weight of his authority to a doctrine not only not proved, but altogether so improbable.

There still remain one or two topics connected with the history of yellow fever, that we had wished to notice; but we must pass them over altogether, and proceed, without further delay, to examine, and that very briefly, the Report of the Commissioners of the New York House of Assembly on the quarantine laws of that state. We had hoped to have been

---

* As a matter of course, whenever the pestilence does not appear for a number of successive years, the immunity is at once attributed by our author to the salutary effects of the quarantine which has kept the disease out. But to show how fallacious such a mode of reasoning may be, it is worthy of notice that we have the high authority of Humboldt, in stating (we quote from Dr. Gillkrest) that, "although the disease usually exists among the newly arrived every year at Vera Cruz (one of the most pestilential spots in the world), it never prevailed epidemically there between 1776 and 1794, although the intercourse with the Havannah and other places, where the disease continued to prevail, was quite free. He even says that there was not a single example of the *vomito* during the eight years preceding 1794, although the concourse of Europeans and Mexicans from the interior had been very great."

able to give a condensed analysis of the medical testimony in this Report; our want of space, however, prevents us doing so at present. As usual, there is no little discrepancy of opinion among the medical witnesses upon many of the questions that were proposed to them. The weight, however, of influential evidence is unquestionably in favour of the infectiousness and consequent importability of the disease; but not to the extent that Dr. Copland might lead his readers to suppose. Take the following as an instance. Dr. C. says:

"From 1751 until 1791, this pestilence made its appearance in New York on several occasions; but after the latter period, it appeared more frequently and more destructively, as might have been expected from the increased size and population of the city, and the more frequent intercourse with places where it prevailed. It even occurred during two or three successive years, and was seldom absent for a longer period than this, until 1832, when it prevailed most fatally. Since that year, quarantine regulations have been strictly enforced, and the distemper has not appeared again in that city—now a period of nearly a quarter of a century—although it has scarcely been a year absent from vessels detained in quarantine, and in the quarantine hospital.

"Can any stronger proof of the propriety of enforcing these regulations be adduced than that to which I have now referred?" Vol. III., p. 237.

To estimate, however, fairly the value of the statements here made, and the conclusion thence deduced, the reader should be informed of one or two circumstances that cannot fail to qualify in some measure the opinion which he might otherwise at once adopt. The following extracts from the Report will explain what these circumstances are.

"The State of New York, as early as 1784, enacted quarantine laws, and the examination of them will show that they are scarcely behind those now existing in their practical operation; and yet in 1798, when the city contained only about fifty-eight thousand inhabitants, between two thousand four hundred, and two thousand five hundred persons, died with the disease the law was passed to prevent. At subsequent periods laws have been enacted, and precautionary measures taken, still, in the years 1805 and 1822, the city found itself in the midst of pestilence, and, in some other years, cases have occurred, but the number has been so small, that the disease could hardly be regarded as epidemic, and they have not been as numerous, if we except the years 1805 and 1822, as at the quarantine grounds." P. 41.

Moreover, we learn that there was no yellow fever in New York from 1805 to 1822, a period of *fourteen years;* therefore before "the quarantine regulations have been strictly enforced."*

---

* To give an idea of the excessive stringency of these regulations, we quote the first:

"All vessels direct from any place where yellow, bilious-malignant, or other pestilential or infectious fever existed at the time of their departure, or which shall have arrived at any place, and proceeded thence to New York, or on board of which, during the voyage, any case of such fever shall have occurred, arriving between the thirty-first day of May and the first day of October, shall remain at quarantine for at least thirty days after their arrival, and at least twenty days after their cargo shall have been discharged, and shall perform such further quarantine as the health officer shall prescribe." P. 51.

Thirty days quarantine imposed upon a vessel, although not a single case of fever has ever been on board!—and this, too, after it has been declared that "the yellow fever will develop itself in from two to twelve days after infection,

The exemption of the city from any invasion of the pestilence since 1822,* is attributed by our author, as well as by one or two of the medical gentlemen of New York, whose communications appear in the report, entirely to the more stringent operation of quarantine regulations; but then they omit to mention, not only that the general sanitary condition of that city has undergone a most material improvement since the period mentioned, by the removal of the crowded grave-yards, the purification of the quays, the cleansing of the streets, &c., but also that—and this is a very important circumstance—while a most rigorous system of defence and exclusion has been adopted, during the last twenty-five years, at New York against *suspected* as well as against *infected* vessels, an infinitely less severe code of regulations has been in force all the while at Brooklyn, which is only a few hundred yards distant, and between which and New York there is an incessant and unrestricted communication. We gather this from the following statements:

"Vessels arriving at the quarantine from interdicted ports with a healthy crew, not allowed to come to New York, have usually been permitted to come to the wharves at Brooklyn, after two days quarantine, and immediately discharge their cargoes, and the vessel remain at the wharves, or reload for any outward port." P. 17.

We are told by Mr. Brower, a most intelligent witness, that "after a vessel has discharged a healthy cargo at Brooklyn, during the quarantine season, and has been cleansed and ventilated, she is not permitted to come to New York to re-load until after the first of October, and these vessels are frequently thrown out of employ for months, while nothing can be more evident than if they were unhealthy at Brooklyn, it would be known as soon, and the effects as prejudicial to the health of New York as if they were at the wharves of the city. The contiguity and almost instantaneous communication between the two cities forbid any other conclusion."

It thus appears that there is good reason to believe that the health of New York during the last twenty-five years would, in all probability, not have suffered in the least, although its quarantine regulations had been much less rigorous than they have been all the while. We have alluded to the good effects of the improved sanitary condition of the city of New York, in rendering it less liable to the diffusion, if not to the invasion *ab extra*, of yellow fever. How strikingly is the truth of this exemplified in the following statement of Dr. Hort with respect to New Orleans, where he has resided for upwards of twenty years.

---

and even sooner, if the exposure is to the morbid effluvium of the disease in a concentrated form." It will be observed that no distinction is made between the (alleged) true, or genuine pestilence and malignant bilious fever. Are we to infer from this, that they are regarded as one (or all but one) and the same disease? At all events, this circumstance alone shows how hard a thing it is to distinguish between what Dr. Copland and some other writers assert to be "essentially distinct diseases."

* "Since the year 1822 the city has been free from any epidemical yellow fever; yet some of our eminent physicians have expressed the opinion that deaths occur every year in the city, of a disease, if not the yellow fever, one which has all its characteristics except its epidemic, contagious, or malignant form." P. 14.

"Twenty years ago, only two streets, parallel with the river, were paved. In wet weather, the other streets were a perfect quagmire. In the back yards of the houses, there was only a thin crust of earth over an offensive black mud. At that time, most of the ground floors were almost in contact with the alluvial soil, and the back of the city was low, presenting numerous pools of stagnant water. It was no wonder, under such circumstances, that a yellow fever atmosphere was generated. At present, the streets are paved. Yards are bricked, or flagged, or coated with asphaltum, which is also now generally used to coat the soil under the houses. The low parts in the back of the city, have been filled up, and the wet swamp in the rear has been completely drained; all filth and offal in the streets is speedily removed, and the gutters are washed every day. There remains much to be done, however, before we can feel confident that a yellow fever atmosphere can no longer exist here. A great change has certainly taken place in the health of the city, which has been commensurate with the improvements carried on by the corporations and by individuals. Since 1841 we have had no yellow fever worth speaking of, and I feel assured that it is in our power, if the authorities will only adopt the necessary measures, to expel it for ever." P. 174.

The hope expressed here may be too sanguine; but the grounds suggesting it are at all events very interesting, and full of instruction. Dr. Hort is of opinion that the exemption of New York since 1822, and of Charlestown since 1832, is owing to "the general improvement in the cities, and greater attention to cleanliness particularly. It may also be in part owing to the changes of climate which we can observe, and to changes in the atmosphere which may happen, but which we cannot explain.

Several of the correspondents who strongly maintain that the Yellow Fever is an infectious disease, liable to be imported by shipping into New York, nevertheless declare it as their firm opinion, that the disease will not spread in places that are clear and well-ventilated. The commissioners themselves use these words:

"If the yellow-fever, cholera, or small-pox are imported diseases, they may not be epidemic, except in a vitiated atmosphere, and if the atmosphere of the city is vitiated by bad practical sanitary regulations, then commerce should be relieved, and the city authorities or its citizens held responsible for furnishing disease with food to make it epidemic and destructive. Charging the epidemic yellow fever *solely* to commerce, guilty though she be in part, would be unjust, and laws to regulate her, be very inadequate to preserve the life of our citizens." P. 42.

The evidence of Mr. Allen, who was a member of the Board of Health during the epidemic visitations in 1819 and 1822, is strong upon this point. After stating his conviction that the disease was imported in these years by infected vessels coming from the West Indies, he says:

"It is a fact, too, that the disease, whenever it has prevailed in this city, has been confined to a particular district, and always commenced its ravages near the wharves of the city, while other parts, particularly the upper districts, have been exempt from contagion, and sick persons removed from the infected district to said parts, in no instance have communicated the disease to those attending them, or to others residing in their vicinity." P. 231.

What a striking illustration of this last remark is to be found in the evidence of Dr. Rodgers, who was health officer of New York, in the year 1805, when the yellow fever prevailed there in such a malignant form that no fewer than 270 out of 600 cases proved fatal.

"Of those attached to the health officer's department, boatmen, orderlies, and

attendants, not one has been sick from any infection, or from any connection with the sick or infected vessels—of the lighters employed in carrying goods to the city, or bringing cargoes to vessels at Quarantine, not one of them was, in the smallest degree, indisposed, till the 24th or 25th of September, when one of them was taken with fever, which left him in a few days; another was seized just after the first, and died on the 2d of October. Neither of them had any connexion with any foul ship, to which they could attribute the complaint; but they took it from having had daily intercourse with the eastern part of the city, and from being under the necessity of going into houses and stores in that quarter, and staying longer in them than they had been accustomed to, by reason of the want of hands to receive their goods in consequence of the desertion of that part of the city." P. 11.

After quoting largely from Dr. Rodgers' official letter to the Board of Health, dated December, 1805, the Commissioners add:—"This able physician and faithful health officer concludes his report by saying, I have now clearly shown, as far as negative proof can go, *that whatever might have been the cause of the late epidemic, it did not arise from any neglect of duty at the Quarantine ground*, NOR DID IT COME THROUGH THAT CHANNEL."

Equally strong is the testimony of Dr. Bayley, who was health officer during the epidemic of 1822. Of 70 persons sick with the fever, who were sent to the marine hospital on Staten Island, 37 died, of whom 18 had black vomit. These cases were regularly attended by the physicians and nurses of the establishment, not one of whom was affected by the disease; nor has a single case," says the doctor, "come to our knowledge of any person taking it, who was engaged in transporting the sick from the city to the hospital."

The inference, that surely may be drawn with confidence from these statements is that admitting, the general question of the infectiousness of the disease to be clearly made out, the risk of its spreading in a pure atmosphere is shown to be exceedingly small. The case of Mr. Wells, related at page 10 of the Report, is an apt illustration of this. It would be easy to quote a host of others to the same effect.

We must not quit the examination of the Report before us without directing especial attention to the condition of Boston in reference to the question as to the importation of the Yellow fever, and the proper measures for its prevention and arrest. Boston has been singularly exempt from any severe visitation of the disease. It appeared there in 1798, in 1802, and again in 1819; but on none of those occasions did it spread to any extent, or cause great mortality. Now it seems that there is no fixed or standing code of quarantine regulations in that port against yellow fever or any other febrile disease in particular; at least, nothing at all approaching to that in force at New York, of which we have given a specimen. In place of there being any specified number of days appointed for the isolation of a merely-suspected vessel coming from a particular port, the harbour physician—who (a not unimportant circumstance) is paid by a fixed salary, and not by fees payable by the masters of the vessels—is invested with ample discretionary, but responsible, powers to adopt all means that he may deem proper to prevent the development or introduction of any infectious malady, and arrest its diffusion. The following two regulations embody his chief duties:

"It shall be the duty of the said Physician to examine into all nuisances, sources of filth, and causes of sickness, which may be on board of any vessel, within the harbour of Boston, or which may have been landed from any vessel, or any wharf or other place: and under the direction of the mayor and alderman, to cause the same to be removed or destroyed." P. 237.

"Whenever any vessel shall arrive in the harbour of Boston, which is foul and infected, or whose cargo is foul and infected with any malignant or contagious disease, or any of whose crew or passengers are sick with any malignant or contagious disease, it shall be the duty of the port physician forthwith to report the same to the mayor and aldermen, and if directed by them, to cause the said vessel, her cargo and crew, or either or any part thereof, to be removed to the anchorage ground or wharf at Rainsford island, and the said vessel and cargo to be thoroughly cleansed and purified at the expense and charge of the owners, consignees or possessors of the same. And also, when directed as aforesaid, to cause all or any persons arriving in such vessel, who are sick of any malignant or contagious disease, to be removed to the hospital, on the said island, and all expenses incurred on account of such persons shall be paid by themselves."* P. 238.

By acting upon this simple and intelligible system, we are informed that "the city (of Boston) has been protected, for many years, from the introduction of any malignant or infectious diseases."

In the evidence respecting Boston, as well as that respecting New York, allusion is made, on more occasions than one, to the Yellow fever having made its appearance on board vessels when their cargoes have been begun to be moved or their holds exposed, although the crew had remained healthy during the voyage, and were so at the time of the ship's arrival in port. In illustration of this point, we give the following instance: it is mentioned in the evidence of Dr. Sweetser.

"In July, 1819, the ship Ten Brothers arrived at Boston from the coast of Africa. Her crew were all in good health, and had been during her voyage. She remained a fortnight at quarantine, and then came up to the wharf, all on board continuing well. Her hatches were now removed, when immediately a large part of her crew, and also various others who had been employed on board,

---

*"The Boston code of quarantine regulation is in accordance with (was it suggested by?) the sentiments of a very able and enlightened writer in the 10th vol. of the North American Review. The following passage appears to us to be replete with the best practical wisdom; it contains we think, the key of all sound legislation upon quarantine matters.

"The application of quarantine laws ought not to be regulated in reference to the places from which vessels arrive, *but by the state in which they arrive.* There is no cause for detaining a ship on account of the danger of yellow fever, which is itself in a pure and healthful state, from whatever port she may have sailed, nor however sickly that place may have been. On the other hand, no ship that is foul and offensive, or that has a cargo in a putrid state, although the place from which she sailed, or the persons on board be ever so free from sickness, ought to be permitted to approach the town until she has been thoroughly cleansed. There ought to be a rigorous system of inspection, during the summer and autumn, into the state of every ship which has a cargo of a perishable nature, while she is discharging. In this manner the danger, when it exists, will be detected before it can have extended to any considerable number of persons, and will be promptly removed." P. 46.

as custom house-officers and labourers, and even the crews of other vessels lying alongside, were seized with yellow fever, and which, in most of the cases, terminated fatally. The hold of this vessel had become exceedingly foul from putrefying corn. At this time, 1819, so entirely was the system of quarantine founded on the assumption of imported contagion, that the internal condition of the ship itself was not at all regarded, hence the Ten Brothers was not even subjected to ventilation at quarantine, her hatches not even being removed till she reached the wharf, when there issued from her close and foul hold such concentrated streams of poisonous effluvia that nearly all who were exposed to them perished of a malignant yellow fever. That yellow fever, however, be thus produced, the aid of certain incidental influences would seem to be demanded, since foul ships are constantly arriving in our ports, while it is only occasionally that the fever is generated by them. A long-continued elevation of temperature, united with stillness of the atmosphere, are among the adjuvant circumstances conducive to the development and activity of the specific poison originating yellow fever." P. 158.

If the Report of the New York Commissioners is favourable to the opinion of the yellow fever being, under certain circumstances, infectious and liable to be introduced into healthy parts by vessels having the disease on board, that of Dr. M'William on the Boa Vista epidemic must be deemed conclusive. We earnestly solicit the attention of our readers to the analysis which we are about to give of this very able document; as, from the strict accuracy of its details, and the great care with which it has been prepared, it cannot fail to be appealed to, not only by all future professional writers on the history of yellow fever, but also by the Legislatures of this and of other countries whenever the subject of quarantine in connexion with this disease is brought under their consideration. Being a parliamentary paper, and therefore not likely to fall into the hands of many medical men, our summary of its contents shall be as complete as possible; the main wish and object which we have had throughout this article being to bring forward a mass of admitted facts and statements, so that the reader may be enabled to form his own judgment of their value, apart from any reasoning of ours.

In the number of this Journal for July of last year, we gave a minute account of the very fatal fever which prevailed on board the "Eclair," from the time of her leaving the African coast until she arrived in England. It was then stated that, in consequence of news having come to this country, that the disease had broken out with great malignancy at Boa Vista soon after the steamer's departure, the Admiralty had sent out Dr. M'William—than whom, from his talents and experience, a better selection could not have been made—to that island, with the view of ascertaining all the particulars of the case, and more especially of determining, as accurately as possible, the point whether the fever raging there had been introduced by the Eclair, or was of local and endemic origin.

At the close of that article, we gave it as our opinion, after a candid examination of all the facts that were at that time known, that the disease "had been imported by the infected steamer into Boa Vista." It will be seen presently whether that opinion was correct or not; but, before entering upon details, it may be well very briefly to recapitulate the leading events in the outbreak and spread of the fever on board that vessel, before and after her arrival at the Cape de Verd islands.

The "Eclair" left England for the coast of Africa in Nov. 1844. She reached Fernando Po on the 25th of December. For two months she kept cruising along the coast. On the 23d of February, she was at Sierra Leone, and took on board a complement of forty Kroomen. After remaining there for a few days, she returned to Seabar, off the island of Sherboro. At this place, the men were much engaged in the boats, exposed to the heat of the day, and to the dews and tainted atmosphere of the night, in that unwholesome climate. The water also, which they had for drink, was often foul and unwholesome. The first case of fever (remittent) occured on the 3d of April: the man recovered. The next three cases proved fatal. No fresh cases occurred from the 18th of April to the 22nd of May, when three more of the men were attacked; all died, one on the 5th, another on the 7th, and the third on the 13th day. During the next fortnight, other five cases occurred: four of the patients died in from four to nine days. The fever after this seems to have ceased for several weeks: *i. e.* from the 8th of June to the 19th of July, when the Eclair again reached Sierra Leone. While there, the men were engaged in cleaning out the Albert steamer, which was in a most foul and offensive state, and, while employed in this most unwholesome duty, had unfortunately the opportunity of indulging to excess in the use of ardent spirits. On the day last mentioned, the fever re-appeared on board the Eclair: the man died on the eighth day. During the next three days, three fresh cases occurred: all proved fatal. On the 23rd, the Eclair left Sierra Leone, having the Albert in tow: they proceeded to the northward. Three fatal cases of fever occurred within a few days after sailing. A merchant who had embarked in the Albert at Sierra Leone, died on board that vessel on the 27th. On the 1st of August other two cases occurred: the patients recovered. On the 2nd, there were two fresh cases: they proved fatal. On the 8th and 9th, four men were added to the sick list: two died, and two recovered. On the 10th, the steamers reached the Gambia. After remaining till the 14th, the Eclair sailed for Goree, having four sick of the fever on board. On arriving there, she was refused pratique: one man died there. On the 17th, she proceeded to Boa Vista, which she reached on the 20th: three men had died on the passage. During the first ten days she was at Boa Vista, five fatal cases occurred. The men, the healthy as well as the sick, were then landed on the small island at the entrance of the harbour, and the vessel was overhauled: the officers, &c., were accommodated at Porto Sal Rey, the chief town of Boa Vista. Notwithstanding the removal of the sick on shore, the fever continued to prevail, and even to increase; for no fewer than 28 deaths took place between the 31st of August (the day of landing) and the 12th of September, when the Eclair sailed: making in all 39 deaths from the time of leaving Sierra Leone. When the Eclair was at Boa Vista, the "Growler" steamer arrived from Sierra Leone on the 6th of September, just as the assistant-surgeon of the former was taken ill. It was then that Dr. Maclure—who, having just received his promotion, was returning to England in the Growler—nobly volunteered his services on board of the Eclair; a memorable, but not a rare, we trust, instance of heroic self-devotion! We need not pursue the rest of the sad tale of the vessel from the time that she left Boa Vista until she arrived in England; as it is altogether with the events, which occurred in the former place after

her departure, that we have now to do. Before taking up the narrative of these, let us here premise that, from all accounts, the island was entirely free from sickness at the date of the steamer's arrival, as well as during her stay there. It is said by Mr. Macaulay to be "a remarkably healthy place," and to be by no means subject to any pernicious fevers. In some seasons, it appears that there is a good deal of remittent fever; and this has been known occasionally to assume a more than usually severe and fatal character. The last year when such was the case, was 1833. In order that nothing at all important may be omitted in our narrative, we may state that the English consul, in his letter of the 22nd Dec. 1845, to Lord Aberdeen, alludes to the extraordinary heat and unusually heavy rains—"events which were surprising to the oldest inhabitant"—that had occurred up to the 9th of October.

Porto Sal Rey is, as we have already said, the chief town of the island: one of its districts is called Pao de Varella. Rabil is about four miles distant from Porto Sal Rey, and Moradinha about a mile from Rabil.

While the Eclair was at Boa Vista, "the seamen seemed to have resorted chiefly to the house of a man called Justinian da Silva Georgio, who keeps a spirit-store in Porto Sal Rey. It is remarkable that this man was attacked with headache and general fever on the evening of the day he was visited by the Eclair's people; he was attended by Dr. Almeida and seen by many of his friends during his illness, which lasted a fortnight; amongst the latter were two females, called Anna Gaspar and Rosinha San Antao, both of whom had slight fever shortly afterwards. It is but necessary to mention that both these women (prostitutes, we suppose.—*Rev.*) had been visited by people from the Eclair; they soon recovered, and their illness at the time attracted no notice whatever."

These were the only cases of sickness that occurred among the inhabitants of Porto Sal Rey, during the stay of the Eclair at Boa Vista. It should be also mentioned that, while the officers &c., resided on shore, several of them, the captain's cook, and some other servants were taken ill, and were, according to the strict orders of Capt. Estcourt, immediately conveyed to the fort at the entrance of the harbour.

Let us now see what occurred to the small military guard stationed there during, and immediately after, its occupancy by the crew of the Eclair. Dr. M'William's account is as follows:

"When the crew took possession of the Fort they found a guard of three soldiers there. They had been on this post already three or four days. Their names were—

Athanasio Perez, Acting Corporal, dark mulatto, aged 26 years;
Pedro Manoel, negro, aged 30;
Antonio dos Santos, private, mulatto, aged 25.

This guard slept in front of the upper room occupied by the sick, and often went into it. The corporal had a headache two days after the sick were landed, which at first yielded to cooling fomentations. In two days more he and his comrades were relieved, and he was still rather unwell, but making no complaint was posted as a sentry at the barracks in Porto Sal Rey. He was, however, soon taken ill, and sent to his house in Pao de Varella, where he was confined some weeks. His case caused no alarm, nor was he attended by any medical man, further than being seen once by Dr. Almeida during his convalescence. The other two

soldiers had also attacks of fever. the one three weeks or so after he left the Fort and the other not until fever was general in the barracks.

"The second guard at the Fort consisted of—

Vincente da Cruz Silva, dark mulatto native, aged 29;
Manoel Antonio Alves, negro native, private, aged 18;
Luis Briza, European Portuguese, private, aged 18.

"The guard was on duty at the Fort six or seven days; none of them were at all affected during this time. They however all had fever afterwards, when the disease was prevalent in the town, of which Luis Briza, the European, died.

"Third guard. In the routine of duty the second guard was relieved by

Corporal Joaquim Agostinho, European Portuguese;
Private Joao Alexandre Roque, European Portuguese; and
Private Miguel Barbosa, native negro, aged 30.

"By the evidence of Miguel Barbosa, the only survivor of this guard, it appears that he and his comrades were at the Fort several days before and after its evacuation by the sick of the 'Eclair.' They slept in a small cook-house under the rampart. The same day the 'Eclair' left, Miguel was ordered by the corporal Agostinho to sweep out the rooms which had been occupied by the sick, and that all three of them went into the rooms on this occasion. According to his account, on the day after the steamer sailed, the Corporal was attacked with fever, and died in three days, delirious, and vomiting a black fluid. The other European, Joao Alexandre Roque, was similarly seized on the day following, and died on the fourth day with fever, delirium and black vomit. Both these men were seen by Dr. Almeida, who told me they were in *articulo mortis* when he was called to them.

"Pedro Manoel, a native negro soldier, was sent by Major Mascarenhas, the Military Commandant, to attend the sick soldiers at the Fort; he was with them from an early period of their illness, up to the time of their death, and remained at the Fort for several days afterwards, in company with Miguel Barbosa. Before they left the fort some gunpowder was exploded in the rooms which the 'Eclair's' people had occupied; and it is said that they were whitewashed some time afterwards.

"Another negro soldier, called Manoel Antonio Alves, who was on duty with the second guard, was ordered by the Commandant to bury the bodies of Agostinho and Roque. For this purpose he proceeded to the small island accompanied with Jaoquim Farenga and Manoel Luis, both native soldiers. He left his comrades in the boat, and stripping himself, ran up to the Fort in a state of nudity, and there, with the assistance of Miguel Barbosa and Pedro Manoel, rolled the bodies in quilts, and carried them down to the boat, which transported them to the beach, about a mile and a half to the southward of the town, where they were burried in the sand.

"The fourth guard was constituted of this man (Manoel Antonio Alves), Lorenzo Samed, and Pedro Gonzalves, all native soldiers. The same boat, that conveyed them to the Fort, on its return brought Miguel Barbosa and Pedro Manoel to Porto Sal Rey. The Commandant, however, deemed it advisable not to admit them at once into the barracks, but sent them to a house in the upper and northern end of the town called Beira,* which is a part of the District Pao de Varella.

---

* "This is a row of four small houses, divided from each other by rough rouble partitions. The uppermost was occupied by a mulatto native woman of the name of Theresa Maria Jesus; the next by the two soldiers; the next to that by a Portuguese woman called Anna Gallinha, and a native woman named Anna Texeira; and the other by a Portuguese man called Jose Carlos da Lisboa, a writer."

"The soldiers of the fourth guard lived in a room next to that in which the sick of the 'Eclair' had been. Manoel Antonio Alves was taken ill in the course of two days, and, strange to say, was at once conveyed to the barracks in Porto Sal Rey. Pedro Gonzalves and Lorenzo Samed were in their turn in the course of ten or twelve days relieved from duty at the Fort. Gonzalves was taken ill with fever some days after he returned to the barracks, and died on the 26th of November. Lorenzo Samed was also attacked, and recovered after a dangerous illness.

"The fifth guard at the Fort, Andre Vass and Jose Sancha, two negro private soldiers, were sent to the Fort to replace the fourth guard. Jose Sancha was attacked with fever three days after he had been there; upon which the Commandant resolved for a while to withdraw the guard altogether from that fatal post. Both the soldiers therefore returned to the barracks, where Vass was also taken ill in the course of three days." P. 84.

From this statement it appears that the only fatal cases among the guard at the Fort were those of the two Europeans, who were there when the sick of the Eclair left on the 12th of September. Both of these men sickened a day or two afterwards, and died within the next three or four days; *i. e.* about the 20th of the month or so. Some, indeed, of the other soldiers also—who, it should be remembered, were either mulattoes or negroes—had been ill; but only in a slight degree.

Let us now follow the two men Barbosa and Manoel, when upon leaving the Fort about the 25th September, they went on shore to live at Beira. "It is of the utmost importance," says Dr. M'William, "that every circumstance connected with their stay here be carefully noted, as it afterwards turned out that the very first (fatal) case of fever in the town appeared in the room adjoining that in which these two soldiers were lodged." Although neither of them was so ill at this time as to be confined to bed, it is obvious that they had the germs of the disease about them.* After remaining in the house for a week or so, they went to the barracks where, it seems, both were laid up. Among their most frequent visitors at Beira were their neighbours Gallinha and Texeira; the former, indeed, had been in constant attendance upon them, and had cooked their provisions. She was attacked about three or four days after they left, and died with high fever, delirium, and black vomit. This was on or about the 16th of October—a little more than a month after the sailing of the Eclair. Up to this time, no apprehension had existed among the residents of Porto Sal Rey, although it was known that two soldiers had previously died in the fort, some weeks before; it was then thought that the disease would not spread to the town.

---

*"Their clothes were taken by the wife of Silvester Jose Romess, a negro mason, to be washed. Silvester himself had also been a visitor of the soldiers when in the house at Beira; he had never been on board the 'Eclair' or at the Fort, but was attacked with fever soon after the soldiers returned to the barracks, as were also his daughter, niece, and wife, but the latter not until the fever had become general in the town."

It may be as well to mention here, that of 17 women who washed the soiled linen brought from the Eclair, not one of them was seized with the fever before the end of October, by which time the disease already prevailed in the town; "so that in none of these cases can the occurrence of the fever be fairly attributed to infectious matter conveyed by the linen."

The next fatal case occurred in a mulatto man, Affonso by name, who lived only twenty yards from Anna Gallinha, and who had frequently visited her; he died two days after her. A native mulatto woman, who lived next door to this man, and who visited both him and Anna Gallinha, was attacked with fever on the 19th; she also died. The man, who carried the bodies of Affonso and of Gallinha to burial, was two days afterwards seized; he died. The persons who had waited upon him, straightway sickened: one of them died. The occupants of the other two houses in the Beira were taken ill about the same time. Theresa Jesus, and also Anna Texeira (who lived in the same room with Anna Gallinha) recovered; but Lisboa the writer, a Portuguese, died on the 27th or 28th. "Among those who visited this person, was the son of Senhor Carvahal, merchant in Porto Sal Rey; this youth, aged 18, had passed two nights in the house of his friend. He sickened the very day that Lisboa died, and died himself on the 4th November. He had not, as far as could be discovered, had any intercourse with any other infected person, save his friend. He was nursed by his mother, sister, and father, who, like himself, were dark mulatto natives; but none of them suffered.

"The disease had attacked several persons in Porto Sal Rey proper, or the lower part of the town, by the end of October; and some of the soldiers were sick in the barracks at this time. The force on the island is in general about forty soldiers, who, with few exceptions, are natives of the Cape de Verds. Before the late invasion of fever, there was one major and commandant, one lieutenant, and forty-one soldiers, including serjeants and corporals. Of the forty-one soldiers, thirty-two were negroes or very dark mulattoes, and nine were European Portuguese. The whole were attacked with fever, which proved fatal to eight of the nine Europeans; two of them died at the Fort, and the other six in the barracks at Porto Sal Rey. Of the native soldiers three died." P. 86.

Meanwhile, the Governor with his suite, several of the better class of Portuguese, and the greater number of the English, had left the island about the end of October for other islands of the group, where rigid quarantine was kept up against Boa Vista. "The whole of those," says Dr. M. "who left the island, have, I believe, continued perfectly free from fever, with the very notable exception of one family which returned to it in November," under the belief that the disease had nearly, if not altogether, disappeared. The particulars of this case are too interesting to be passed over:

"Mr. Pettingal and his family, consisting of his wife and daughter, a young lady about twenty years of age, were lodged for the first night on their return to Boa Vista, in the house of Mr. Macaulay, and on the following day removed to their own house. A day or two subsequently, two mulatto girls who, during the absence of the family at San Nicholao, had been in charge of the house, were both taken ill with fever; and although one was sent home immediately, the other remained in the house three days after she was attacked, and was during this time frequently visited by Miss Pettingal. Both of the mulatto women recovered; but on the 17th of November, or six days after the return of the family to Boa Vista, Miss Pettingal was seized with symptoms of fever, which in forty-eight hours were unequivocal, being marked by black vomit and retention of urine, and terminated fatally on the afternoon of the 24th November, or the seventh day from being attacked.

"Miss Pettingal was sedulously attended during her illness by Mrs. Learner,

"The soldiers of the fourth guard lived in a room next to that in which the sick of the 'Eclair' had been. Manoel Antonio Alves was taken ill in the course of two days, and, strange to say, was at once conveyed to the barracks in Porto Sal Rey. Pedro Gonzalves and Lorenzo Samed were in their turn in the course of ten or twelve days relieved from duty at the Fort. Gonzalves was taken ill with fever some days after he returned to the barracks, and died on the 26th of November. Lorenzo Samed was also attacked, and recovered after a dangerous illness.

"The fifth guard at the Fort, Andre Vass and Jose Sancha, two negro private soldiers, were sent to the Fort to replace the fourth guard. Jose Sancha was attacked with fever three days after he had been there; upon which the Commandant resolved for a while to withdraw the guard altogether from that fatal post. Both the soldiers therefore returned to the barracks, where Vass was also taken ill in the course of three days." P. 84.

From this statement it appears that the only fatal cases among the guard at the Fort were those of the two Europeans, who were there when the sick of the Eclair left on the 12th of September. Both of these men sickened a day or two afterwards, and died within the next three or four days; *i. e.* about the 20th of the month or so. Some, indeed, of the other soldiers also—who, it should be remembered, were either mulattoes or negroes—had been ill; but only in a slight degree.

Let us now follow the two men Barbosa and Manoel, when upon leaving the Fort about the 25th September, they went on shore to live at Beira. "It is of the utmost importance," says Dr. M'William, "that every circumstance connected with their stay here be carefully noted, as it afterwards turned out that the very first (fatal) case of fever in the town appeared in the room adjoining that in which these two soldiers were lodged." Although neither of them was so ill at this time as to be confined to bed, it is obvious that they had the germs of the disease about them.* After remaining in the house for a week or so, they went to the barracks where, it seems, both were laid up. Among their most frequent visitors at Beira were their neighbours Gallinha and Texeira; the former, indeed, had been in constant attendance upon them, and had cooked their provisions. She was attacked about three or four days after they left, and died with high fever, delirium, and black vomit. This was on or about the 16th of October—a little more than a month after the sailing of the Eclair. Up to this time, no apprehension had existed among the residents of Porto Sal Rey, although it was known that two soldiers had previously died in the fort, some weeks before; it was then thought that the disease would not spread to the town.

---

*"Their clothes were taken by the wife of Silvester Jose Romess, a negro mason, to be washed. Silvester himself had also been a visitor of the soldiers when in the house at Beira; he had never been on board the 'Eclair' or at the Fort, but was attacked with fever soon after the soldiers returned to the barracks, as were also his daughter, niece, and wife, but the latter not until the fever had become general in the town."

It may be as well to mention here, that of 17 women who washed the soiled linen brought from the Eclair, not one of them was seized with the fever before the end of October, by which time the disease already prevailed in the town; "so that in none of these cases can the occurrence of the fever be fairly attributed to infectious matter conveyed by the linen."

Number attacked : : : : : 8
" died : : : : : : 7
Ratio of deaths in population, 1 in 1.6
Ratio of deaths in number attacked, 1 in 1.1.

French—
Number exposed to fever : : : : 2
" attacked : : : : : 2
Spaniards : : : : : 2 not attacked

NATIVE POPULATION.—Free : : : : : 666
Slaves : : : : : 249

Total : : : 915
Died, 65 free, 3 slaves : : : 68
Ratio of deaths in native population, 1 in 13.4

So much for the outbreak and course of the fever in Porto Sal Rey. The reader must now have his attention directed to what was going on, in a very early part of its career, at the town or village of Rabil, which is divided into two districts, known by the names of Cabeçada and Boaventura. Here it is necessary to state, that of the men, upwards of 40 in number, who were employed on board the Eclair during her stay at Boa Vista, and all or most of whom returned every night to their homes, not one appears to have suffered either before or for a considerable time after the departure of the steamer, save one man from Cabeçada, Luis Pathi, whose case is thus given by Dr. M'William:

"He returned to his house when the vessel sailed, and on the following day went to Moradinha (or Santa Cruz), a hamlet on the south-east side of the Rabil ravine, to attend the feast of the Exaltation of the Holy Cross. This festival is observed on the 14th of September, and its celebration sometimes continues three or four days longer. While engaged in the dance on the third or fourth day of the feast, he was suddenly taken ill, and at once put to bed, where he remained several days; at the end of this time he was conveyed to his own house in Cabeçada, where he was confined three weeks longer. He had general fever, pain of back. &c., but no black vomit. His daughter, a girl of twelve years of age, was attacked on the tenth or eleventh day after his return to Cabeçada, and died in three days, with suppression of urine and black vomit. Another daughter, seven years old, was next seized, and died in four days; a boy eleven years of age was next taken ill, and died on the fifth day with symptoms similar to those of his sisters. His wife was attacked the same day the boy died; the disease in her case was protracted to fifteen days' duration, and was marked in its last stage, as it had been in the cases of her children, by black vomit.

"Such was the ominous commencement of the fever in the populous district, Cabeçada."* P. 89.

---

* It thus seems that the fatal case of Pathi's daughter occurred some days before that of Anna Gallinha in Porto Sal Rey. Here it may be right to state that although Pathi, as we have seen, sickened at Moradinha, and remained there in bed for several days before returning to his own house at Cabeçada, yet no other case of fever occurred in the former place until the month of December, when a woman came sick from Rabil, and died shortly afterwards.

It is scarcely necessary to follow the course of the pestilence in this place. All the evidence goes to show that it originated from the family of Pathi. His house was situated in the most crowded part of Cabeçada, and adjoining to it were the houses of Manoel Fachina, Joaquim Marques, Joaquam Pathi, and Manoel Rosa, who, after Luis Pathi's illness, were the first sufferers in Rabil. The disease soon appeared in the other parts of the town, more especially in the south-western direction.

The fever was most fatal in Cabeçada in the months of November and December, and in Boaventura from the middle of November to January. Some cases appeared in February and March. Dr. M. saw two as late as the beginning of April; but the disease was then in a mild form.

We shall not pursue the outbreak and spread of the fever in the other villages on the island. Suffice it to say that, in Dr. M'William's opinion, "the disease could in each case be clearly traced to one or two individuals, coming from an infected district, and affecting first the inmates of the house they remained at, next the visitors, and gradually extending on the whole of the inhabitants. In fact, it may be said that, in each town and village on the island, the disease first appeared in a single house, which became an irradiating focus for its dispersion in all quarters."

The fever was supposed to have ceased entirely throughout the island by the end of April; but, three weeks or so afterwards, it again made its appearance at Moradinha, the village where Luis Pathi was taken ill, but in which the mortality had been remarkably small during the prevalence of the epidemic. Dr. M'William at once proceeded to the spot, and he found two persons, a girl of 14 years of age and a man about 35, with all the symptoms of a most malignant fever. His description of these symptoms is so vivid that we must not fail to give it:

"The countenance of the girl, a dark mulatto, was of a dirty lemon colour, shining through the natural darkness of the skin, made it resemble very much that of a light bronzed statue. A very strong fætor issued from the body, which tainted the room and drove us back from the door until the window was opened. She had complained much of pain along the spine, and still pointed to her head as the seat of pain. She had been bled in the arm by one of the neighbours, and all around the wound was of a greenish colour, swollen, and putrid. In the angles of the mouth there was dark frothy blood. She had had black vomiting; but this symptom had for some hours ceased. The urine was black, as were also the fæces. The former had been voided in very small quantities. Pulse small, irregular. She had been ill seven or eight days.

"The man had nearly the same symptoms, but in a milder degree, and he had not been affected with black vomit." P. 94.

The girl died next day; the man gradually recovered. He appeared to have been benefited by the use of small and repeated doses of Quinine. Dr. M. learned that there had been two other fatal cases of the fever in Moradinha, some days before it was discovered that the disease existed there. Four fresh cases occurred between the 1st and 3d of June. "In none of them," says Dr. M., and we particularly invite the reader's attention to this remark, "was there black vomit; they soon passed from the *continued* to the *remittent* form, and all recovered under the use of quinine, mild aperients, and nourishing diet."

Nevertheless, it is but right to mention that Dr. M. has remarked, in his concluding observations, that "remittents and intermittents were much less common among the convalescent than I had expected."

The account, given of the re-appearance of the fever at Moradinha, is as follows:

"A girl called Maria dos Prazeres was the first person attacked at Moradinha on this occasion. She had visited Joao Gallego and the other eastern villages on the 15th of May, and returned to Moradinha, where she was laid up with fever on the 20th of the same month. Her mother who slept with her one night after she was taken ill, was also attacked on the 26th and died on the 29th, after three days' illness. The girl whom we saw was called Perpetua. She had visited Maria, soon after which she was seized with fever, and died with the symptoms which have been already described, on the morning of the 1st of June.

"The disease did not extend to Rabil or any of the other villages, and on the 13th of June the whole of the remaining patients were convalescent; and when I left the Island on the 15th of July last, there had been no case of fever for thirty-three days, and no deaths had occurred since the 1st of June, or during a period of forty-five days." P. 95.

The following table shows the total amount of the actual and of the relative mortality among the different classes of the inhabitants of the island of Boa Vista, during the prevalence of the fever.

	Total Resident Populat'n	Left the Island.	Exposed to Fever.	Died.
Europeans:—				
Portuguese	56	3	53	25
English and Americans	25	14	11	7
French	2	—	2	—
Spaniards	3	—	3	—
Total Europeans	86	17	69	32
Natives:—				
Free 3875	3875	—	3875	266
Slaves 434	434	—	434	13
Total Numbers	4309	—	4309	279*
Grand Total	4395	17	4378	311

Ratio of Mortality.

Amongst Portuguese, Spaniards, and French, who were exposed to the Fever	1 in 2·28
" English and Americans	1 in 1·60
" Native population—Slaves	1 in 33·4
" Free	1 in 14·6
Average mortality of native population	1 in 15·4

* To this number we must add 3 more out of the eight additional cases, which occurred at Moradinha between the 20th of May and the 3d of June.

Dr. M'William found it quite impossible to ascertain, with any degree of correctness, the number of persons attacked in the several villages; but, from the best information he could obtain, he believed that about two-thirds of the whole population of the island was affected by the fever.

When he left the island at the end of the second week in July, no case of fever, as we have seen, had occurred for upwards of four weeks, and no death for six weeks. The disease had not, in any one instance, extended to any of the other islands of the Cape de Verde group. A rigorous quarantine had been kept up, during the whole time, against Boa Vista. It was stated in Dr. Stewart's report that yellow fever existed at Porto Praya in San Jago, while the 'Eclair' was at Boa Vista. This statement, we are told by Dr. M'William, is not correct. The only fever at that time existing in San Jago was the usual endemic remittent, which prevails there every season during and after the rains; nor was it then more severe than ordinary.

There cannot be a doubt but that the fatality of the epidemic at Boa Vista was not a little aggravated by the great scarcity of provisions which had existed in the island for some months, and by the utter want of any medical assistance whatsoever during the greater part of the prevalence of the fever. The poverty and the destitution of the people, the filthy state of the streets and of the houses, the presence of a quantity of stagnant putrid water in the immediate vicinity of the town of Porto Sal Rey, in conjunction too with the very great heat of the weather, must all have contributed to render the febrific poison more fatally malignant.

The mortality occurred chiefly among persons of from 12 to 35 years of age, and death took place generally between the third and seventh days.

"Of second attacks," says Dr. M., "I could only find five cases at all clearly established; from the little appreciation that the people have of time, it is not possible to arrive at the exact intervals between the attacks."

Let us now see what are the general deductions, which the conscientious and enlightened reporter has drawn from a calm and minute examination of all the facts connected with the memorable history of the "Eclair fever" throughout its entire course.

"Under ordinary circumstances, the fever which attacks ships' crews on the African coast is the common bilious remittent. Seldom if ever has it happened that a disease characterized by black vomit has at once broken out in a ship. This malignant symptom rarely occurs, even on shore, except during some of the more severe epidemic visitations. Judging, therefore, from the result of general experience, as well as from the medical reports, it seems to me to admit of no doubt that the disease which invaded the 'Eclair' in the latter end of May and early part of June 1845, was the usual endemic fever of the African coast, which although a most fatal disease, is not considered to be of an infectious or contagious nature. The mental despondency which seems to have pervaded the crew rendered them indeed peculiarly susceptible of disease, not only while on detached service in the boats up the river, but also on board the ship (around which the commonly recognized causes of fever were in great abundance), and on this account it is probable that the fever was of a more aggravated type than usual.

"At Sierra Leone, the irksome and unwholesome duty of cleansing the hold of the 'Albert,' as well as that of their own vessel; the unwonted exposure in some cases by day and night; the irregularities committed by men who had for

some months been exposed to morbific influence; all combined to the development of a fever of a much more malignant nature than the usual endemic of the African rivers.

"Accordingly, several of the cases that occurred after the ship left Sierra Leone were marked by unequivocal black vomit—a symptom, as has been already mentioned, extremely rare in the endemic fever, and regarded by all who have served in hot climates, as a test of unusual malignancy.

"It is thus quite evident that the type of the fever changed materially for the worse during the passage from Sierra Leone to the Gambia and Boa Vista. There is also great reason to believe that it acquired still greater virulence while the crew were at the Fort. The house in which the sick were lodged there contains only one room at all well ventilated; and, judging from the evidence of Dr. Almeida,* they must have been much crowded; at all events, the fact is beyond doubt, that the accession to the sick list and the mortality became much greater at this than they had been at any previous period. In short, from the endemic remittent of the African coast, the disease had, from a series of causes, been exalted to a concentrated remittent, or yellow fever." P. 105.

Such is Dr. M'William's deliberate opinion; an opinion, it will be perceived, in entire accordance with the whole of the reasonings and statements adduced in the early part of this article, which, it is but fair to ourselves to state, was written some time ere we had received Dr. M.'s report. The following passage cannot fail to be read with interest, as an interesting comment on much that has gone before.

"Sir William Pym and those who espouse his doctrines will contend that the fever of the 'Eclair' was from the first a contagious disorder, differing essentially from the remittent of the African coast. If a disease such as that described by Sir William Pym be really endemic on the coast, surely we ought to hear more of it, considering the large squadron which is now kept there.

"If it be assumed that the fever on board the 'Eclair' was the Bulam, and therefore primarily contagious, I would ask where was it contracted? Not at Seabar, for in that case we would expect other vessels that visited that part of the station to be affected with a similar disease. Was it at Sierra Leone? For if so, why were not some of the many Europeans from the numerous shipping there, attacked with a fever manifesting contagious qualities? And we know that while the 'Eclair' was there, there was nothing unusual either in the amount or nature of disease in the colony. The same may be said of the Gambia. Assuredly the remittent is quite destructive enough of human life; and in the late Niger Expedition, where its fatal effects were manifested in a terrible degree, there was no reason to believe that its spread was due to contagion. The whole of the surviving sick were landed at Fernando Po, after we got clear of the Niger, yet none of the residents there suffered in consequence, although many officers and men died at Clarence Cove. Why did this malignant disorder rage on board the 'Eclair' and not in other vessels that were with her? Simply because her circumstances were peculiar, and it is entirely to this peculiarity and unwonted

---

* "You were, I understand, requested to see the people of the 'Eclair?'—Yes, I was requested by the Governor to go to the Fort and see the sick of the 'Eclair' there.

"How did they seem to be lodged there?—They seemed to be extremely crowded. I could hardly pass between them. In one small room there were about twelve sick persons.

"How many sick were there altogether when you visited them at the Fort?—I should say about thirty if not more."

combination of circumstances that the contagionality of the fever with which her crew was affected is due. To me there is no proof that the fever in question was in any degree contagious before the vessel reached Boa Vista, and we have a right to look for proof both at Sierra Leone and the Gambia, where the evidence is against contagion. At Boa Vista the reverse is the case; for the whole history of the progress of the fever subsequent to the landing of the crew on the small island proves it then to have possessed highly infectious qualities. I would say, then, that the contagious properties which marked the 'Eclair' fever at Boa Vista, were *acquired or contingent*, and not primarily or essentially belonging to it."—P. 110.

Dr. Mc William, in drawing his observations to a close, alludes to what he considers the injudicious reluctance of Sir William Pym to permit the removal of the sick from the Eclair, upon her arrival at the Motherbank, either on board a hulk of proper size, or to one of the wings of Haslar hospital, "to be appropriated exclusively for them," as was proposed at the time by Dr. (now Sir John) Richardson.

"This humane proposal might, in my opinion, have been acted upon without any risk of the public health being endangered; for the history of yellow fever in ultra-tropical countries shows that a continued high temperature beyond that of the summer heat of England is necessary to keep its poison in a state of activity; and that when it has prevailed at Gibraltar, Cadiz, Malaga, and Leghorn, it has always been preceded by a certain duration of high temperature. The Superintendent-General of Quarantine objected most decidedly to the sick crew being landed at Haslar, on the ground of 'the disastrous consequences to the mercantile interest of this country, in the event of the 'Eclair being admitted to pratique;' for this reason alone, 'and without entering into a discussion relative to the public health,' did Sir William Pym oppose the removal of the sick crew to Haslar. One would have indeed hardly expected that the Superintendent-General would have much dread of the danger of infection, from the crew of the 'Eclair' being landed in the month of October, in the commodious, cool, and well-ventilated wards of the finest hospital in the world, after reading in his work that the contagious powers of yellow fever 'are destroyed by cold, or even by a free circulation of moderately cool air.'"—P. 111.

The general conclusions of Dr. M.'s Report stand thus:—

"First, That the fever on board the 'Eclair' was primarily the remittent of the African coast, which is not a contagious disorder, but that the disease acquired contagious qualities in virtue of a series of causes.

"Second, That although there exists on the island of Boa Vista a physical cause capable of producing remittent fever, yet it does not appear that that cause was in action when fever broke out in September, 1845, and that the island was quite healthy when the 'Eclair' arrived there.

"Third, That the disease of which the Portuguese soldiers died at the Fort (Duke of Braganza) on the small island, was that which afterwards ravaged Boa Vista, and the same as that which prevailed among the crew of the 'Eclair.'

"Fourth, That the fever was propagated throughout the island almost exclusively by direct intercourse with the sick, there being only two cases in which there appears any probability of persons having been infected in any other way.

"Fifth, That although those who had passed through the fever were much less liable to the disease than those who had not, yet it would appear that a person having had one attack, possesses no absolute protection against a second attack.

"Sixth, That connecting the whole of the circumstances attending the arrival and stay of the 'Eclair' at Boa Vista, with those under which the disease appeared

on the small island, and afterwards on Boa Vista itself, leaves no doubt of its having been introduced by the 'Eclair.'

"Seventh, "That in all probability the mortality from fever on the island was much increased by the want of proper nourishment for the people, as well as by the total absence of medical assistance for some months.

"Eighth, That the disease had in no case spread to any of the other islands of the Cape de Verd Archipelago." P. 112.

In taking leave of Dr. M'William's report, we cannot but express our surprise that Sir William Burnett, in transmitting it to the Admiralty, should still persist in asserting, now that the complete circumstances of the case have been ascertained and made public, that there was no case of the fever at Boa Vista for a month after the departure of the Eclair! Such hardihood of assertion surely savours more of pertinacity than of calm conviction. Sir William Burnett himself, in his former communication upon the subject (vide the number of the Med. Chir. Rev. for July, 1846, p. 228) candidly admitted that "if it can be fully and satisfactorily shown that any person who had visited the ship or tents where the sick were placed, contracted the fever in question, and communicated it to others, and they to other persons in succession, who had never visited the ship or sick, then there can be no reason to doubt the infectious nature of the disease; but if nothing of this kind has taken place, then the conclusion must be that the disease is not infectious, and is therefore incapable of being communicated: in either case settling this long-contested question." Can any unprejudiced reader of Dr. M'William's report hesitate for a moment as to his opinion? We cannot but suppose that Sir William Burnett allowed himself to prejudge the question at first, and that he was afterwards unwilling to recede from the position he had taken up. His stinted and ungracious recommendations of Dr. M.'s report must surely have proceeded from the same feeling of disappointed expectation.

From one Sir William we pass on to another Sir William. He of the Quarantine board is, as might be expected, much more complimentary to Dr. M'William than the *chef* of the Navy Medical Department. The Report is declared by the former to be "a most valuable document," displaying "great judgment, perseverance and impartiality," and its author is said to "deserve well of his country for the manner in which he has executed his mission." Strange!—is it creditable?—that the very same act and work should be so differently regarded by men equally professing the same regard for truth and candour.

As a matter of course Sir William Pym points to the cases of Georgio the store-keeper, of Luis Pathi, and of the two Portuguese soldiers at the Fort, as affording incontrovertible evidence of the introduction of the fever by the 'Eclair,' and of its spreading by infection. We quite agree with him in this respect: the facts are too strong to be gainsaid, and too conclusive to be explained away; but we cannot see how the evidence brought forward by Dr. M. can anyhow be said to finally settle the question as to malignant Remittent and Yellow Fever being essentially distinct diseases. Dr. M'William evidently thinks quite otherwise, as we have already seen. Sir William then enumerates what he deems to be the distinguishing features between the two diseases. He makes the presence of the *black vomit* to be the characteristic or discriminating symptom of the true pesti-

lence, and this symptom he (unguardedly, it must be presumed) says "only shows itself in the persons of natives of a cold climate." He does not hesitate to declare that it is "a native of, and peculiar to the West Coast of Africa," and that it "has been at various times imported into different islands and countries; viz. to the different West India islands, the island of Ascension, and the different ports of Spain and North America, as in the year 1845 into the island of Boa Vista." With respect to the assertion that "it has a singular and peculiar character; viz., that, like Small-pox, it attacks the human frame but once," we shall only now say, in addition to what has been suggested in a former page, that the character or attribute in question is by no means so *singular* or *peculiar* as Sir William and some other writers imagine. All fevers of a continued type, (whether originally or only secondarily so,) and more especially when they have once occurred in a severe form, leave a greater or less degree of insusceptibility to a relapse or recurrence. Second attacks of true Typhus are comparatively of very rare occurrence. We have already seen that the same holds true of the Ardent fever, and, we doubt not, of *malignant* Remittent also.

The general assertion, too, of Sir William, that the endemic fevers of intertropical countries are not infectious, has been shown to be, when taken in an unqualified sense, not strictly correct: they are unquestionably liable, now and then, to become so under circumstances of crowding, want of cleanliness, imperfect ventilation, &c.

With respect to the statement that "it appears evident from the history of the 'Eclair,' that she had both diseases on board at different and distinct periods; viz., the marsh or mild during the months of April, May and June, and the yellow or Bulam fever with black vomit, from the 23d of July to the time of her arrival in England," we can only say that it is very far from being evident. Dr. M'William, as we have seen, interprets the matter very differently, and, as it seems to us, in the right way. Sir Wm. indeed, fixes the exact day when the first case in which there was black vomit occurred, viz., on the 27th of July, the fourth day after the steamer left Sierra Leone. But not only is there an utter want of evidence to warrant this statement,* but there is a very strong presumption that this symptom had been observed, in a partial degree at least, at a much earlier period; for (as quoted in our previous article) we read, in the account of three *post-mortem* examinations in the month of April, the following entries made by Dr. Maconchy:—"the stomach contained some reddish glairy matter"—"the stomach was distended with dark serous fluid, mixed with black flakes." What, pray, were those appearances, but evidences of black vomit having, either actually or all but taken place? and when it is mentioned in addition that, "not a clot was seen in the body, but fluid

* It is amusing to notice how Dr. Copland, in his narrative of the fever on board, seeks to reconcile Sir William's statement on this point with the lack of any evidence to prove it. While he admits that "no description or details are given in the official papers," he nevertheless tells us that "on this point it is impossible for Sir William to have been mistaken; seeing that his experience of this distemper in the West Indies and in the South of Spain, has been greater than that of any other physician whatever." What has his experience to do with the matter?

blood exuded from the nostrils and blistered back," can we resist the conclusion that already the fever on board the 'Eclair' had begun to exhibit symptoms of incipient malignancy? Moreover, it is left utterly unexplained by Sir William Pym, where his true yellow or Bulam fever was caught. It was not prevailing at Sierra Leone at the time of the departure of the Eclair; and all the other ships of the squadron were healthy at the time, and remained so. Both of these considerations appear to us to render it very much more probable that the already existing fever on board of the steamer *became*, from a variety of circumstances, all tending to the deterioration of the health of the crew, infectious and fatally malignant, than that an entirely new disease, *nova pestis*, was introduced on board immediately before the vessel left Sierra Leone.

It is curious to observe how apt a person, who has taken up strongly any favourite hypothesis, is to assimilate, and, as it were, incorporate, all facts that present themselves with that hypothesis. Sir William Pym, in his zeal to prove the great fatality of the real yellow fever, and consequently the danger of its being imported into this country, actually quotes from an anonymous newspaper a list of ships on the African coast, with their mortality, during a period of eight months in the years 1837–38. Out of 650 men, no fewer than 308 perished; a truly frightful loss! But how, may we ask, does Sir William know that every one of these fatal cases was his genuine yellow or Bulam fever? Is it not much more probable that a very large proportion arose from that very fever, viz. the endemic remittent, which he so emphatically declares to be essentially distinct from the former, and which we know to be abundantly fatal?

Sir William still adheres to the propriety of the advice he gave upon the arrival of the 'Eclair' in England not to land the sick, as was proposed by Sir J. Richardson, at Haslar Hospital, upon the ground that the landing of them at Boa Vista was the cause of the introduction of the disease into that island:—not a very valid reason, it must be confessed. His allusion too to the disease being arrested at Moradinha, in the beginning of June, by the measures recommended by Dr. M'William, is surely straining a more than doubtful point to the very uttermost; the fever was all but extinct upon the island at the time!

In concluding our notice of the "Eclair fever," we may remind our readers that although four of the officers of the "Growler," who went on board of the former steamer at Boa Vista, were attacked very soon after, there is no evidence to show that this disease spread to any other of the Growler's crew. But what is very remarkable is that, while this vessel lay at Woolwich, two of her men, engaged in clearing out her hold (which, it is stated, gave out a very offensive smell), were taken ill on the 10th of October—a month after leaving Boa Vista—and both died in hospital with all the characteristic cases of yellow fever: no other case occurred subsequently, either on board the Growler or among any of the attendants in the hospital on shore. Here, therefore, is another instance of the disease appearing to be connected with a foul and unwholesome state of the vessel's hold, and of the danger of men being exposed to the effluvia thence proceeding. It is much to be regretted that Mr. Carter, the surgeon of the Growler, has not given the public any particulars respecting the health of her crew, notwithstanding the appeal that has been made to him.

Already so large an amount of space has been occupied with the preceding details, that we have left ourselves only a few lines to allude to the principles which should guide a medical man in his endeavours to prevent the introduction and arrest the diffusion of the Yellow fever. And here, to prevent all mistake, let it be understood that we are alluding to a fever of hot climates, which, *in whatever manner it may have originated*, is accompanied with symptoms of a dissolved and putrescent state of the fluids, as indicated by the occurrence of black vomit, sanguineous discharges from the bowels and other passages, and by a luridly-yellow discolouration of the surface. That such a disease is liable, under certain favouring circumstances, to exhibit highly infectious properties cannot, we think, be fairly disputed; and is not this just what we might have *à priori* expected from its very analogy, we might rather say its close alliance, with other forms of malignant putrid fever? Cullen was quite right, we are convinced, in making it one of the several varieties or modifications of Typhus; its peculiar characters being the result of climatorial and other appreciable influences.

It would be easy to point out many features that are common to them all; and we verily believe that it has been from a neglect of this very fact that there has been so much discrepancy of opinion amongst medical men in reference to the one we have been treating of. That petechial typhus is apt to spread by infection will not be denied: and the same thing seems to hold good of malignant yellow fever.* The virulence of the infection, and consequently the risk of propagation, in both are proportionate to the degree of the dissolution and putresency of the fluids, and the corresponding malignancy and fatality of the disease.

There is but little occasion to entangle ourselves in controversies as to there being any essential difference between one intertropical fever and another, or as to the alleged original *habitat* of a peculiar species in this part of the world or in that. It is sufficient for the practical physican to recognize the simple and intelligible principle that, whenever a fever in a hot climate begins to exhibit symptoms which clearly indicate that the blood is "touched corruptibly," it is high time for him to have recourse to precautionary measures to mitigate or arrest its diffusion from the sick to the healthy. And what are these measures?—the very same that every prudent man would resort to, in this country, in the case of Typhus. Do not crowd the sick together, but keep them as much apart as possible; maintain the greatest cleanliness and freedom of ventilation in their chambers and abodes; separate the healthy, and prevent all unnecessary intercourse between them and the sick; remove all causes, as far as may be practicable, of existing insalubrity; be of good courage, be not afraid. It is always useful to have the healthy well-fed and actively occupied. Wherever the sick have been located, the cabin, room, or ward should not be occupied by persons in health until it has undergone a thorough cleansing and purification; and here we cannot but express our opinion that the use of disin-

* Much of what has been said on the Typhoid form of Dysentery, in a previous page of our present number, is strictly applicable to the history of the Yellow fever, and supersedes the necessity of further remark in this place.

fectant gases is not sufficiently appreciated in the present day on board ship, in barracks, and so forth, wherever numbers of the sick are apt to be accumulated.

While recognizing the importance, nay the necessity, of such measures as these, we need scarcely say that much of the present system of Quarantine restraint, in reference to Yellow fever, &c., is very needlessly vexatious and oppressive. The truth is, that the whole *regime* requires revision and remodelling. It should form a part of a general code of sanitary discipline; and the quarantine board should be but a branch of a general board of health. There has been a vast deal of most exaggerated and foolish alarm about the introduction of foreign pestilential diseases, while, all the time, we have been overlooking the more formidable pestilences that are continually at our very doors.* Much of this extravagant fear has arisen from keeping the subject of quarantine laws and enactments apart and distinct from the considerations of those measures, that should be had recourse to against the diffusion of our own home-bred fevers. The general plan of prevention, in both cases is the same; nor need there be any great difference in the particular measures to be adopted. To impose a fixed period of 20, 30, or 40 days upon suspected, or even upon infected, vessels, &c., without regard to the particular circumstances of each individual case, is manifestly absurd. No medical man would recommend such a practice in reference to the worst forms of European petechial fever.

As to the Yellow fever being ever imported by the *sound* cargoes of vessels coming from infected ports, there is scarcely an atom of evidence to show that such has ever been the case; and, even with respect to the bed or body-clothes of passengers, the risk of its introduction in this way seems to be indeed but very small. Nevertheless it will always be prudent to avoid this risk, especially as it can be so very easily done with little trouble or expense.

Much more attention ought to be paid to the condition of ships' holds than is usually the case. That a foul hold is a not unfrequent cause of bad fever on board, a fever that may eventually acquire an infectious character, is much more than probable. If masters and owners knew that the duration of their quarantine would in a material degree depend upon the state of their vessels, a great deal of the expense and delay to which they are at present subjected might, with perfect safety to the public, be avoided.

---

* How strikingly is this exemplified in the New York Report. The number of deaths, in the Marine hospital on the quarantine ground, is ten-fold and upwards greater from small-pox than it usually is from Yellow fever; and yet, during most of the year, there is no quarantine whatsoever against the former disease.

MENTAL DYNAMICS, OR GROUNDWORK OF A PROFESSIONAL EDUCATION. THE HUNTERIAN ORATION BEFORE THE ROYAL COLLEGE OF SURGEONS OF ENGLAND, 15th FEBRUARY, 1847. By *Joseph Henry Green*, F.R.S. late Professor of Anatomy and Surgery to the College: Professor of Anatomy to the Royal Academy: one of the Surgeons to St. Thomas's Hospital. Octavo, pp. 65. Pickering, London, 1847.

No one present in the Theatre of the College of Surgeons on the 15th of February last will soon forget the discourse to which they listened on that day. For an hour and three quarters Mr. Green commanded the earnest attention of a densely crowded audience, by a display of learning and eloquence, in an address delivered with consummate skill and brilliant effect. But on calmly reading this oration in the closet, we could not but be impressed, how greatly its success was due to the happy and effective delivery of the orator. It contains, indeed, metaphors and passages of great beauty, but at the same time much that is impracticable, unintelligible, and in the obscure style of Coleridge.

Mr. Green, after paying a brief but graceful tribute to the distinguished services of John Hunter, states that his present purpose is not that of dwelling on his merit as a physiologist and surgeon; but of considering how far his excellence may be deemed a pattern for the formation of a scientific character in unison with the requirements of our profession.

"We are here indeed, on the very threshold of our inquiry, met by a difficulty, which can neither be overlooked nor disregarded; it is this, that the predominant and characteristic trait of Hunter's mind was Genius. Now if, in accordance with almost universal belief, we admit without qualification that this attribute implies a perfection unattainable by human effort—that it is absolutely a special gift of Providence imparting to the mind of its possessor somewhat of the power originative and creative of the Divine Giver—we must humbly confess that Genius is not an imitable acquirement, and we shall be constrained to abandon as hopeless any search for the means of emulating Hunter's peculiar excellence by mental culture and self-exertion. Nevertheless, who will deny, that by educing and cultivating the powers, which any fairly endowed individual may possess, we may preserve the freshness, improve the vigour, and favor the originative faculties of the mind? And if, as cannot be doubted, the art and science of healing eminently require a mind relying on its own resources, it cannot be unworthy of our study how the mind may be best trained in order to elicit its inherent powers and native energies, and to render that calculable and regular, which otherwise would be but a happy accident." P. 8.

Hunter unquestionably possessed genius in its truest and highest sense, but in that sense we can scarcely regard this attribute as an "imitable acquirement;" nor should we exactly select him as a pattern for the rising generation except for his "unceasing application and marvellous industry," without which, as Mr. Green states, "we should find original power elanquesce, and exhaust itself in immature and abortive productions." Much as Hunter has accomplished, we can come to no other conclusion than that this self-taught genius might have achieved still

more, and at least have done fuller justice to the productions of his own intellect, had his powers been cultivated and his mind disciplined in early life; in fact, had he enjoyed the advantages of the training so forcibly inculcated in this discourse.

The object of the Oration is an attempt to define the intellectual discipline "which may best prepare the mind for the scientific cultivation of a profession, and aid the individual in forming his character in the light of a final aim." We cannot follow Mr. Green in the learned arguments by which he shows the value of a knowledge of letters, natural history, the history of man, mathematics, logic, &c. We must be content with the selection of a few passages which will enable our readers to form some notion of the style of the author, and of the elevated character of his sentiments.

Mr. Green describes Natural History as—

"A study, which offers food for every digestion, knowledge for every capacity, and which tends more perhaps than any other to people, to enlarge, and to tranquilize the mind. But where begin, or where end, in the vast living panorama of God's works, in which earth, air, sky and ocean, teeming with the multitudinous assemblage of their countless myriads of products and inhabitants, form the mighty canvas, and rise upon the overwhelmed sense of the youthful student, as they may have burst upon the view of our first progenitor, and still as fresh as in the morning of creation? The 'majestical roof of heaven fretted with golden fire;'—ocean and the watery world, as the workshop where the busy artist Nature prepares the first rude sketches of the living forms, which she afterwards brings to view in a higher and more perfected character;—mid-air, with its shifting scenery of light and clouds, its meteors and changeful variety of atmospheric phenomena;—and earth, with its hidden treasures of metal, crystal, and precious gem; its surface, diversified by river, lake and mountain, adorned with all the forms of living growth from the lofty forest tree to the lowly and lovely flower, and peopled by the animated tribes, that form the graduated scale of organic life. Here Curiosity is ever excited, Attention rivetted, and Memory bribed, by perpetual novelty, variety and beauty:—the Comparing power is ever kept alive by an endless succession of similitudes and contrasts, that now sustain the interest by inducing the pupil to note the like in the different and the different in the like, and now re-awaken the flagging attention by renewed excitement and gratification of the senses;—and the Reasoning Power is finally evoked in order to trace and explain the varying adaptation of means to proximate ends, displayed in Instincts which anticipatively rehearse the functions of that faculty, which when enlightened by Reason, and directed to ultimate ends, becomes Human Understanding. Thus, as the student watches the ascension of nature into mind, he shall learn that, up the whole ascent, nature is a prophetic-hymn, heralding the advent of man, and proclaiming the wisdom and goodness of the Creator." P. 19.

We may quote the following remarks on a subject much neglected in the education of medical men, viz. Logic. Its proper province is

"Reasoning or Discourse—the process by which we deduce from known truths all that they legitimately comprehend, by which we apply general rules to particular cases, by which we infer from some less comprehensive truth one of more comprehensive generality;—it is the process by which we weigh evidence, infer and prove by argument, and draw universal and necessary conclusions;—while in every judgment the presence of Reason is attested by the claim, which it asserts and vindicates to the unavoidable conviction of all rational beings.

And the results are the rules, maxims and judgments, which constitute our generalized experience.

It is not therefore the art of one

'Profoundly skill'd in analytic;
Who can distinguish and devide
A hair 'twixt south and south-west side;
On either which who can dispute,
Confute, change hands, and still confute:'—

BUTLER.

"Reasoning is the daily and hourly business of our lives, and, whatever our worldly calling—in the pulpit, at the bar, or at the bedside of the patient—we are unceasingly occupied in inferring or proving a something from what is already ascertained or taken for granted. And in the words of an eminent writer on Logic, 'To learn to do that well, which every one will and must do, whether well or ill, may surely be considered as an essential part of a liberal education.'" P. 31.

We shall conclude this notice with an extract expressive of Mr. Green's approval of the institution of collegiate establishments at some of the principal medical schools.

"I have only to express my unaltered conviction, strengthened indeed by the success with which the trial has been already attended,—that it is in Universities and Colleges that a medical education may be best grounded on those universal elements of science, which are the essential constituents of every liberal profession. I hold it indeed scarcely possible that any professional education can be fully accomplished except in such Institutions:—where discipline, both intellectual and moral, and a pledged direction and supervision of the studies, give the requisite security for its progress and completion;—and where the *Alumni* are induced habitually to regard themselves as members of one body, and to form among themselves a correspondent law of honour, of self-respect, and of respect for each other as fellow-collegians—with the cognate habit of despising the hollow, the tricky, and the ostentatious,—in short, to form that sentiment of honour and gentlemanly feeling, in which the moral life of the individual breathes as in its natural atmosphere, with an unconsciousness, which gives the charm of unaffected manners and conduct." P. 42.

THE NATURE AND FACULTIES OF THE SYMPATHETIC NERVE. By *Joseph Swan*. 8vo. pp. 55. London, 1847.

IN a former Number of this Review, we called the attention of our readers to a Pamphlet on the Nerves of the Uterus, by Mr. Swan—(Med.-Chir. Rev., April, 1846.) In the present and somewhat larger publication, the author takes a more general view of the anatomy and physiology of the ganglionic nervous system; and, in connexion with it, of the par vagum.

It need scarcely be remarked that the sympathetic nerve is that part of the nervous system concerning which, both as respects its anatomy and physiology, the greatest uncertainty prevails. In late years, however, much light has been thrown upon this subject; or rather it may be said on the anatomical structure; for although there has been no lack of theories, it can scarcely be affirmed that any positive and unquestioned fact has been

established with respect to the peculiar functions of the ganglia and their nerves. The following are the principal points which may be regarded as proved:—1. That in its ganglions the sympathetic offers the same elementary structures, especially gray corpuscles or vesicles, which are considered to constitute, in the case of the brain and spinal cord, an active nervous centre; 2, that it contains fibres proper to itself, the so-called gelatinous, gray, or organic nervous filaments, originating from, or at all events connected with, the above-named vesicles; 3, that it receives into its system tubular fibres from the cerebro-spinal system, these being derived from both sentient and motor nerves, as is seen in the case of the scattered cephalic ganglia, and also in that of the common spinal nerves where tubules come off from both the anterior and the posterior roots; 4, That these two classes of fibres, the gelatinous and the tubular, however much they may appear to be mixed up and confused together, constitute no exception to the one great law of the nervous system—the non-intercommunication of the primitive fibrillæ, that is to say, whether traced in the interior of the ganglia or in the branches, they are always observed to maintain an independent course; 5, that the various branches and nerves belonging to the sympathetic contain the gray and tubular fibrils in different but determinate numbers.

These are the anatomical facts, and of course they offer much suggestive evidence as to the physiological actions, upon which moreover there are some few positive but subordinate facts that have been ascertained, either by observation or by experiment. Thus it is certain that, although ordinarily the organs supplied by the sympathetic perform their functions without any known participation therein by the brain, yet occasionally impressions do traverse to and fro, sensation being excited, as for example, from irritation of the intestinal mucous membrane, and emotion, not volition, affecting the heart and other motorial organs. It is also further ascertained that impressions are mutually transmitted between the sympathetic and the true spinal system; if, for example, in a recently decapitated kitten, the spinal cord be irritated in the neck, the heart's action is immediately quickened; and, again, if irritation of an organ exclusively supplied by the sympathetic, as the intestine, be morbidly excited, convulsions may be induced. These few facts comprise almost all that is positively ascertained; and it is clear that, relating to the mutual connexions of the ganglionic and cerebro-spinal system, they throw little or no light on the special or true functions of the ganglionic system.

Mr. Swan's brief exposition consists of an interesting sketch of the structure of the sympathetic, as ascertained by the aid of comparative anatomy and of its physiology. The following passage will convey to the reader the general views entertained by the author. "The great object of the sympathetic nerve is to furnish the parts it supplies with an appropriate nervous excitement of such a quality as will insure their functions without disturbing any other portion of the nervous system. It connects in different degrees all the parts of the nervous system as an harmonious whole, but brings them in so slight a degree in communion with the sensory as to allow only a perceptibility that can appreciate and respond to impulses without permitting them to proceed beyond the viscera. By preventing sensation, it becomes favourable to the production of involuntary

motion, so that impulses on the lining membrane of the viscera, when sufficiently strong, are responded to, and the contraction of the muscular coat takes place. For these purposes it has a peculiar conformation, which differs more or less from the other parts of the nervous system. Although it communicates with several cerebral nerves, it assimilates most to the fifth and spinal nerves. From these and their centres it probably derives some of the essential or diffusive influence for fortifying its vital powers, but admits only just as much of their faculties as corresponds with the functions of the parts it supplies." P. 4.

The comparative anatomy discloses some interesting facts. In the first place the plan of distribution, making allowance for diversity in the form of the body, " preserves a very great similarity, because it corresponds almost entirely with the fifth and spinal nerves ;" the general principle is that, there is one ganglion to each pair of spinal nerves, but these may either remain distinct from each other, as in the cervical region of birds, where there is a small ganglion attached to each cervical nerve ; or they may be massed together, as in the superior ganglion of the neck of man, which comprises in reality several distinct ganglia. The presence of large and active extremities makes it larger, especially its ganglia. Then as to texture, in the higher animals, the ganglia are close and fleshy, whilst in other classes, as in many reptiles, they are thready or plexiform ; and Mr. Swan believes that the powers and excitability of the latter are inferior to those of the former.

As regards the " innate faculties " of the sympathetic, as Mr. Swan terms them, he conceives that there are two distinct powers—" a diffusive influence, and a perceptive and motive faculty ;" the former, " which forms the element of the material portion of the nervous system, the sympathetic has the power of deriving from, and through, the cerebral and spinal nerves. It can have its powers depressed or exalted ; they are depressed when those of the brain and spinal marrow are low, and especially when the spinal marrow has been permanently injured ; they may be exalted in any increased vital process, whether sound or morbid." As to " the perceptive and motive faculties, they are concentrated in, and have their specific powers limited to, any particular region by a ganglion or plexus ;" the quality of the perceptive faculty likewise depends on the structure of the ganglia, although its powers are capable of being further exalted or depressed by the state of the diffusive influence."—*L. c.*, pp. 17, 18.

As it would exceed our limits to enter into the details of this inquiry, we shall merely extract the summary which the author has drawn up of his views.

" The sympathetic nerve has several properties peculiar to itself, and some resembling the faculties of other nerves. It is, to a certain extent, an independent system, and is a source of peculiar power. It derives its general diffusive influence, or nervous element, from the other parts of the nervous system its branches communicate with. It has appropriate faculties, and is perceptive, and not sensitive, except in pain. Its ganglia have a power according to their structure, and a capability of receiving and answering impulses. It excites involuntary motion primarily. If the impulses are strong, but still amount only to a perception, the ganglia can transmit them to the spinal marrow, and thence directly to the voluntary muscles ; but if the impulses be stronger, and amount to pain, they may pass from the spinal marrow to the brain, and excite voluntary

motion; or they may pass from the ganglia to the stomachic plexuses, and from these by the par vagum to the oblong medulla, and thence to the spinal marrow and its nerves, and produce vomiting. It, and especially its ganglia, have a peculiar circulation of blood by minute vessels, derived from contiguous parts, which is quiet and gentle, and increased by the activity of the organs its branches supply; it may also be increased by excitement communicated through the nervous influence, or tracts, or nerves, from the brain, and other centres, the intellect, passions, and external senses. It presides over, and connects in action the whole arterial system, particularly in the brain, spinal marrow and nervous system generally. It belongs principally to the heart itself, but through connexions with branches of the par vagum, combines the nerves of this organ with those of the lungs, and those of the muscles of the chest. It combines the abdominal viscera, and these with the abdominal muscles. It animates and combines the reproductive organs; also the urinary organs, and connects these with sentient and voluntary nerves. It combines various other nerves and parts, through minute and intricate branches. It excites the organs it supplies, to separate from the blood and food such things as are prejudicial, whilst the absorbents retain the beneficial. As its ganglia have so much power over the circulation, they, with other nerves and processes, tend to the production of heat; they, also, through a somewhat similar influence, to that in the heart, tend to the persistence of action and want of weariness, even in the voluntary muscles of birds. It determines the effect that local injuries shall excite in the heart and blood-vessels, and the extent and power of constitutional irritation for their reparation." P. 47.

We must leave our readers to form their own judgment of the value of these conclusions; only observing for ourselves, that whilst concurring in several of them, which cannot however lay claim to much novelty, from others we entirely withhold our assent.

In connexion with this inquiry, we may state that Mr. Beck, in a late number of *The Lancet*, contends that the sympathetic system is distinct from, and independent of, the cerebro-spinal system; each having its proper anatomical structure, its distinct physiological acts, and being subject to its distinct diseases. In his former paper (see Med.-Chir. Rev. Oct. 1846) he endeavoured to demonstrate the perfect distinctness of the two systems in origin, course, and distribution; and the principal object of the present contribution is to show by the effects of ether on the uterus, that whereas the influence of this agent is upon the cerebro-spinal system, and that as the reflux functions of the spinal marrow are in abeyance, whilst the action of the uterus continues unimpaired, the inference is, that this organ must depend on some other influence than that of the cerebro-spinal system, on that namely of the sympathetic, which mainly supplies the uterus with nerves.

HOUSEHOLD SURGERY, OR HINTS ON EMERGENCIES. By *John F. South.* 12mo. pp. 340. Cox, 1847.

THE Translator of Chelius, late Professor of Surgery to the College of Surgeons, a member of its Council, and one of the Surgeons of St. Thomas's Hospital, here makes his appearance in a new character, that of

Gossiper on Surgery with the non-medical public, and in that capacity has sent forth a very objectionable book. Popular works upon medical subjects are produced under two different influences: the one a desire on the part of the author to write himself into notice as a remarkably successful practitioner in the class of diseases he has chosen for illustration; the other in a foolish belief that the public at large possess sufficient intelligence and discrimination to successfully apply knowledge which it has taken a long special training to acquire—that there is, in fact, a royal road to the diagnosis and treatment of disease which has only to be pointed out to be pursued. However different may be the moral position of these two classes of authors, the mischievous effects of their proceedings are much the same; and it may be safely said that the world has suffered as much from the blunderings of fools as from the machinations of knaves. We cheerfully acquit Mr. South of any of those puffing designs which have presided over the productions of so many Chest-books, Skin-books, Pox-books, and the like, and only charge him with a mere act of simpletonism—a charge which, small as is the space we have at our disposal, we doubt not being able to make good.

It seems the work originated in some popular lectures which its author delivered some time since to a rural audience. He says,

"Though the subject might seem an odd one to select for a village auditory, yet there was no difficulty in rendering it amusing as well as instructive, and I believe that I may justly say that I did not fail of attaining my object. On accidentally mentioning the circumstance to one of my friends, who by means of the printing press, furnishes food to a hungry and never-satisfied public, he said, 'Why do you not make a little book of it? Do so; and I will print it.' So we struck hands, and here appears my small volume."

We are not much accustomed to the admeasurement of the risible faculties of village boors and Lady Bountifuls; but if they found anything amusing in the lectures, it must have been derived from the manner of the lecturer (but no: certain dreamy recollections of the theatre of the College of Surgeons forbid this conjecture), or it must have been of so subtle a nature as to ooze out during the process of printing; for nothing of the kind do we discover. As to the instruction aimed at, a pretty wide field has been embraced, if not compassed. The author commences his work with enumerating the various contents of what he terms "The Doctor's Shop" with which a family in the country should be provided, and among the "few simple family medicines with which no house should ever be unprovided," we find the following: calomel, tartar-emetic, iodide of potassium, blue-pill, opium, and croton oil; and among the formulæ given, are those for making Goulard and Sugar of Lead Wash, Black-wash, Washes of Nitric Acid, Chlorine, and Oxide of Zinc; Croton-oil Embrocation; Red-precipitate, Iodide of Potassium, Lead, Gall, Zinc, and Sulphur Ointments; and these directed to be used "for the eyes," "sluggish sores," "sluggish sores with stinking discharge," "stinking ulcers, or sloughy wounds," "cracked skin of the legs," "scabbing of large ulcers," "stimulating wounds or ulcers," "to lap up the discharge," "for enlarged glands of the neck," "for piles," "as a certain cure for the itch," as the case may be.

When treating of *Bleeding*, the author judiciously enough recommends, in the case of obstinate hæmorrhage from a leech-bite, that the orifices be

taken up by means of a sewing-needle, but he adds that, in some constitutions, "even after the removal of the needle and thread the bleeding will continue. Nothing then remains but to thrust into the bottom of the wound a bit of thin iron wire heated white-hot, which I have never known fail to stop the bleeding." We would simply ask him whether he has ever met with a case requiring this, and if so, sufficiently often to justify his placing such a recommendation in such a book? Cupping, we are told, has "the advantage of being easily learned," and, we may add, as easily forgotten unless constantly practised. To enable his amatuer to acquire it, Mr. S. furnishes diagrams of the instruments and process. With regard to general bleeding, he does not consider the uninstructed should venture to bleed from the arm, but he may work away at the foot safely enough. The art and mystery of Tooth-drawing, whether by key or forceps, is dispatched in four pages (with appropriate diagrams), and then we come to a chapter upon "How to put on a Roller." We allow that the knowledge of this may be useful to many persons; but we would suggest that one practical lesson from the surgeon who recommends its employment would do more than all Mr. South's pages and diagrams; at all events we feel certain that we could never have learned to roll a leg from his description. The T bandage, "for keeping a poultice on the seat," is a simpler matter, and his lady-readers will understand his diagram well-enough. Of the mode of lancing the gums with a "gum-fleam" we have also an illustration: but, although we agree with Mr. South that the child will often in an hour or two afterwards derive immense benefit from the operation, we doubt whether the anticipation of this will induce it to hold its head erect and unsupported as here depicted. We do not know whether the following passage was intended for a moral hint to his rustic auditory, or as a specimen of dry humour.

"The greater number of persons who get a black eye, deserve it, and so far as I am aware there is no remedy save warm-bathing which will hasten its removal; but it is often a very tedious business. The only thing to be borne in mind is not to get a black eye; if you do, you must be content to bear the disgrace for a few days if you deserve it. But if it have been an accident, there is nothing to be ashamed of, and a small draught of patience will be a sovereign remedy."

Believing ourselves that a black eye may be got quite undeservedly, and that its persistence is to be deprecated, we recommend our readers to discard our author's temporizing procedures and continuously to apply slices of bryony root, or a coagulum formed of alum and white of egg between muslin, which will often rapidly dissipate it.

But we approach more serious matters. Thus we have detailed directions for treating *Wounds* of all kinds, whether a "clean stab," a "bruised cut," a "torn or rent wound," "pricks or punctured wounds," and wounds from serpents. For a bite from a mad-dog, the author is energetic enough, but not too much so, when there is no doubt of the animal's rabid condition. Let a probe or piece of stick be passed down to the bottom of the wound, and insist upon the operator cutting around the wound, and so deeply as to bring out the probe or stick covered with the part cut out as with the finger of a glove." "I do not think I am wrong in advising, where the finger has been bitten, and there is no possibility of medical assistance, to chop it off, which any one can do with a mallet and chisel."

The variety of wounds might have been supposed to sufficiently tax the attention of the amateur surgeon; but no: he is here taught, and shown by woodcuts, the modes of treating rupture of the tendo-Achillis, nearly all the varieties of *Fracture* (compound and simple), and the mode of reducing *Dislocations*, from that of the jaw to that of the hip. In respect to *Hæmorrhage*, we have directions for treating hæmoptysis (free bleeding among them) and hæmatemesis, for taking up arteries in wounds, and the compression of the large trunks. For operating for *Rupture*, the presence of a surgeon is admitted to be necessary, but, in the mean time, the taxis, the application of cold, bleeding to faintness, and turning the patient topsy-turvy may be resorted to.

We have enumerated nothing like the whole of the contents of the book. The various forms of "Stifling" by carbonic acid, drowning, hanging, &c., are treated of diffusely, and there are chapters on Things in the Gullet, Windpipe, Nose and Ears. A chapter is added upon the dress, diet, and exercise of children, in which these subjects—the only ones almost the public can be advantageously addressed upon—are most superficially treated; and another treats of the prevention of infectious fevers by ventilation and the non-crowding of the sick, in which nothing is said which has not been better said a hundred times before.

In fine, we may state of this book, that most of the subjects it embraces are utterly unsuited for general description and illustration: and those which admit of these the author has shown himself incompetent to treat of. It were unjust, however, to dismiss it without adverting to an excuse he offers for its publication. He explains the addition of the portions which relate to wounds and broken limbs, &c., by the suitableness of such information to persons at sea or in commencing colonies, unprovided with medical advice. We are not prepared to say that a pamphlet addressed to this comparatively small class of persons might not have its uses; but this book bears no designation of the kind, and was doubtlessly intended for circulation in this country, in no part of which is medical aid unattainable. The author has some misgivings of the reception which awaits him; for he says:

"It is very possible that some observations may be made on a Hospital Surgeon writing a book of this kind, intended for general use. I am very careless on this point, as I have had no unworthy object in view. The way in which, and the purpose for which the book has been written, are my apology, if any be needed, which I do not admit; and if I desired precedent I need scarcely remind my readers how many of the ablest persons in science and art have held it no degradation to their high standing to render their particular branch of knowledge accessible, not only to adults, but even to children, by cheerfully written works, in simple language, often accompanied with homely illustration."

We do not admit that good intentions form a sufficient excuse for injurious actions. If they exculpate the individual from moral reprobation it is at the expense of his capacity. Nor will the precedents here alluded to hold good. Many of the objects of science and art admit of familiar and popular illustration; and the worst result which can ensue from their partial comprehension is the generation of smatterers and ridiculous pseudo-savants; but it is a greater matter with regard to medicine. In the first place the facts are more difficult of summary communication and compre-

hension; and the ill-consequences of their imperfect acquisition are not limited to the individual dabbling with them, but may injuriously affect many other portions of society. Hence it is the duty of all members of our profession to discountenance instead of encouraging such pretensions; and we are of opinion that the egregious manner in which Mr. South has violated such a duty should, *ipso facto*, forfeit the responsible seat he holds at the board of one of its governing bodies.

Speech of Lieut.-General Sir Howard Douglass, Bart., M.P., for Liverpool, on the Army Surgeons' Bill, in the House of Commons, Monday, April 12, 1847.

We publish with real satisfaction certain passages in the speech of the gallant Sir Howard Douglas, on the ill-treatment of the Army Surgeons—a class of officers forming a considerable portion of the entire body of the medical profession, whose honour and reputation it has nobly upheld, under every circumstance of service—in peace and in war.

The friends of the Army Surgeons, *in the Army*, have at all times been of the class to which we know that Sir Howard Douglas belongs—those emphatically designated as of "*the heroic school*"—the school of Abercrombie, Moore, Hope, and Graham.

The honoured commanders named were sterling friends to our brethren in the army; but the two first were killed in action ere they had done more than record their sentiments on the merits and deserts of the surgeons whose conduct in battle had excited their admiration and gratitude. The others, owing to party or political circumstances, were impeded in their endeavours. Sir Howard Douglas, then follows in a noble wake.

After exposing the low rate of pay comparatively, causing surgeons "to cling to active service long after their physical powers were too much impaired to discharge sufficiently their laborious duties," the gallant General proceeded in detail to recite the grievance of "that learned, most important, and, as he thought, rather neglected class, entitled on every account to the first consideration and distinction."

"The army surgeon, it is true, does not purchase his commission; but the expense necessarily invested on his education must not be forgotten; and he, like the paymaster, cannot realize this by sale, for the benefit of his family. No one who knows anything of the severe and painful duties which a medical officer has to discharge, and the services in which he must be engaged, in the presence of disease of every kind, facing death in every shape, can doubt of the hardships which officers of that class must have undergone during a service of thirty years. The surgeon, too, let it be remembered, must be in full possession of all his energies, keep himself well up to the mark in every improvement in practical surgery and in medical science. Paymasters and quartermasters may, with rather diminished powers, cling to the service without any great detriment to it, or inconvenience to themselves, after these had somewhat faded; but there was a period of life beyond which the medical officer, however perfect his intellect, could not discharge effectually his duties on the field as surgeon of the regiment—a period beyond which vision becomes imperfect, the nerve unbraced, the hand too unsteady, in the difficult and delicate operations which a surgeon was called upon to perform. No man of the age of sixty or sixty-two could be expected to retain these faculties unimpaired; and as the great bulk of medical officers enter the

army at the age of twenty-four, thirty-two years' service would bring them near to that stage in human existence, beyond which it would be vain and unreasonable to expect that a medical officer of thirty or thirty-five years service in all climates should retain his efficiency. Officers so circumstanced are aware of this sad truth, but continue to serve, knowing that the half-pay to which they are entitled is insufficient for their wants; that, from bodily infirmities and deficiency of mental energy, they cannot add to their income by private practice, being unequal to compete successfully with younger and more recently-educated practitioners, and therefore cling to the service. It is for the interests of the service, then, that men so circumstanced should cede their avocations to younger individuals; but this they can only be tempted to do, or are justified in doing, by our increasing their rates of retirement. The medical officer, while on full-pay has no cause for complaint—this is sufficient to attach him to the service, but binds him too long, from the rates of retirement being so inadequate. The hon. and gallant Member then cited, from official returns, the cases of many deputy inspectors general of hospitals, and surgeons, who had been from thirty-six to forty-three years in the service.

* * * * * * * *

"Having explained the ordinary duties of medical officers, he would now beg to say a few words which he hoped would take these officers as a class out of the category of civil functionaries and non-combatants. The medical officer cannot be considered a non-combatant in the sense of personal exposure, nor in many cases, as he could tell, of personal military gallantry. The medical officer is suddenly called upon to discharge duties which require the vigour and powers of endurance of less advanced age; to partake of the fatigue, privations, and diseases incidental to actual service; to brave every climate, to witness death in every form, and to suffer it himself in the field, when administering to those whose lives he is endeavouring to preserve. Two surgeons of Her Majesty's regiment were killed on the field of battle in the operations of the Sutlez; three at Cabul. How many of the Honourable Company's service he knew not. He requested the House to let him endeavour to depict the duties of medical officers on the field of battle. The action is about to commence. The medical officers attached to the troops take post in the immediate rear of their respective corps, and then prepare the implements of their mournful and painful calling. The battle begins; the active combatants, unmindful and regardless of danger, buoyed above the terror of death or of wounds by ardour and excitement, which few can imagine, heed not the casualties that happen around them. Not so the medical officer. The fallen and the disabled that are not beyond the reach of his skill, become the subjects of his immediate care. The fiercer the fight, the more numerous these sad consignments. There, on the naked field, exposed to personal risk, and within reach of the bullets, which may have previously ploughed the ranks of the columns, or lines in his front, the medical officers, with unflinching eye, steady hand, and well-braced nerves, discharge their melancholy functions, and frequently lose their lives in endeavouring to save others. Is the battle won? The troops move forward with exultation to reap the fruits of their victory. The medical officers remain on the blood-stained field, amidst the havoc of war, to collect the mutilated victims, and administer to the sacrifices that victory exacts. Is the battle lost? or is the field, though won, abandoned, as ofttimes happens? The medical officers perform their still more painful duties on the forsaken field, and become themselves captives, in common with those who, by their aid, may survive. Then there was the assault of the fortress, and the storming of the breach, at which medical officers are invariably aiding. And this is the class—such the persons, from whom you withhold advantages enjoyed by the practitioners of a less exalted surgery. The horse is a noble animal; the veterinary science is an important and useful profession; but man is a nobler animal still, and a soldier, apart from other considerations of humanity, a more important

and valuable agent. He appealed, then, in the strongest terms, to the feelings and generosity of Her Majesty's Government, to the House, and to the country, against the exclusion of the class of officers whose case he had taken up, from advantages which are extended to others—from a boon which all public servants, civil and military, now enjoy. The hon. and gallant member stated that he would not enter further into detail, but leave the case to the consideration of the House and the Government, and he hoped that he had not appealed in vain to the noble Lord at the head of the Government, and would be consoled by the reflection that he had done his best in behalf of these officers."

I. Inhalation of Ether. By *J. Mason Warren*, M.D., one of the Surgeons of the Massachusetts General Hospital. Octavo, pp. 18.

II. Practical Remarks on the Inhalation of the Vapour of Sulphuric Ether. By *W. Philpotts Brookes*, M.D., Surgeon of the Cheltenham General Hospital, &c. &c. Octavo, pp. 68. Churchill, 1847.

It is a pleasing duty to have to record that the general result of the immense number of experiments upon the effects of the Inhalation of Ether, which have been repeated throughout entire Europe during the last few months, has been of the most satisfactory character—that expectations have not been disappointed, or fears and forebodings realized. We are not however aware that there are any new facts, whether as regards the *modus operandi* of the substance or the description of cases to which its employment may be extended, that call for notice. For some time to come the cautious observation and candid record of the various phenomena that present themselves will constitute the most important and responsible duty of those whose opportunities are numerous; for, at present, we are but on the threshold of the investigation. Amid the points towards which attention may yet be advantageously directed is, the conferring greater simplicity and portability upon the apparatus employed; for innumerable as are the varieties of those hitherto invented, all we have seen are defective in these essentials. We observe M. Roux of Toulon (*Gazette des Hôpitaux*, No. 61) speaks highly of a bag which he employs, shaped like a lady's reticule, and lined with a pig's bladder, which he says is very easily breathed from even by young children. M. Sichel (who discountenances ether in the more delicate operations of the eye), in some cases in which he desired to subdue excessive spasm of the eyelids preventive of the extraction of foreign bodies, and having no apparatus at hand, poured the ether into a teapot, and directed the patient to inspire through the spout, with success.

The two pamphlets before us contain, besides the detail of some cases, no observations with which the profession is not already well acquainted. That of Dr. Brookes is not at all to our taste. He says, in his preface, "I have never, in any way, endeavoured to shroud this question with mystery; for I have freely and willingly invited any professional or non-professional gentlemen to view the operations that have fallen under my notice. These few pages have, therefore, been written as much for the general as for the medical reader."

It is little to Dr. Brookes's credit to have converted the operating theatre into a show-box for all the idlers of Cheltenham. We are as little willing to advocate mystery in this ether question as can be; but we are still less disposed to countenance its being made a vehicle for puffery. His pamphlet has not been written "*as much* for the general reader," but *entirely* for him.

PRACTICAL OBSERVATIONS ON THE PATHOLOGY AND TREATMENT OF CERTAIN DISEASES OF THE SKIN, GENERALLY PRONOUNCED INTRACTABLE. By *Thomas Hunt*. Octavo, pp. 152. London, Churchill, 1847.

IF we are to believe our author, Arsenic, administered according to his method, is all but an infallible remedy in the worst case of Lichen, Prurigo, Lepra, Psoriasis, Pityriasis, chronic Urticaria, Purpura, Ecthyma, Eczema, Acne, and Sycosis, and also in Lupus and in non-congenital Nævus. All the varieties of these diseases, if not of a syphylitic origin, or not associated with feverishness or other derangement of the constitutional health, are declared by him to be under the dominion of "one antidote, which, under proper regulations, acts with such uniform efficiency as to leave nothing to be desired. That remedy is *arsenic*."

Let us first see what these "proper regulations" are. In place of the usual method of giving arsenic in small doses at first, and gradually increasing them until either some impression is made upon the existing disease, or certain toxical symptoms are induced, Mr. Hunt contends that it is much safer and better to begin with full or tolerably large doses at first, and reduce them by degrees as their *effects* become palpable. The most characteristic of these effects is a pricking sensation in the tarsi, and slight inflammation of the conjunctiva. On this subject the author makes the following remarks:

"*At this crisis* (the supervention of these opthalmic symptoms) *the disease is brought under arrest, and generally from this period appears to be shorn of its strength.* The return of healthy action in the cutaneous vessels often becomes visible, and is sensibly felt by the patient, on the very day on which the eyes become suffused with tears. The dose may now be reduced, and in some cases a very small dose taken with exact regularity will suffice to keep the eyes tender and the skin healing, until at length even the disposition to disease appears to die away under the influence of the poison. And as in the exhibition of mercury we are content with making the gums sore without distressing the bowels, so in administering arsenic we should never allow the mucous membranes to suffer: for supervening gastritis, and even cholicky pains, most clearly contra-indicate its use. Fortunately a slight degree of conjunctivitis, in about forty-nine cases out of fifty takes precedence of the more grave affections which indicate an over-dose. Both the safety of the patient and the prospect of his recovery will much depend upon the vigilance with which a knowledge of this fact inspires the surgeon. Ignorance of the existence of this safety-valve has caused many a cautious practitioner to repudiate the medicine altogether; and an acquaintance with this important sign would, doubtless, on the other hand, have checked the temerity which, in its results, has attainted with unmerited suspicion the reputation of a valuable remedy." P. 13.

Arsenic, when taken for a considerable length of time, is said to produce in some cases a discolouration of the skin:

"The trunk of the patient first, and subsequently all those parts of the body which are by the dress protected from the access of light and air, become covered with a dirt-brown, dingy, unwashed appearance, which, under a lens, reveals a delicate desquamation of the dermis, and is, in fact, a faint form of pityriasis. This may be considered as a secondary form of arsenicalization; for I have observed that when the primary dose is diminished on the appearance of conjunctivitis, the eye-lids may be allowed to get well, yet if the patient's skin be kept *brown*, the disease will vanish just as rapidly as though the conjunctiva were kept sore. The first and larger dose appears to knock the disease on the head, (so to speak,) and to exhaust its energy. Its less malign, or secondary form, being subjugated by the secondary or pityriatic action of arsenic." P. 15.

In old chronic cases of cutaneous affections, "the arsenical course should be protracted (in reduced doses) for about as many *months* after the final disap-

pearance of the disease, as had existed *years* before. This will prove the best security against a relapse, and will generally succeed in preventing it."

After narrating the details of upwards of 40 successful cases—we hear indeed of no unsuccessful ones—of various forms of skin-disease, including four of *lupus exedens* and one of *nævus*, in which a cure was effected by the use of arsenic, Mr. Hunt favours us with some general observations. From these we select the following passage as a fair specimen of his mode of handling his subject.

"It is now many years since I resolved to try what could be accomplished by arsenic in the treatment of the more unmanageable disorders of the skin, and the result has filled me with astonishment and delight. Whenever I have had a fair trial and a fitting case, I can truly say that for many years arsenic has never once failed me. In the few cases which have not done well, either there was irreparable organic disease existing, or else the irregular habits or whims of the patient, or some accidental interruption to the course, sufficiently accounted for the failure. It is possible that in *some* cases related as cured, relapses may have occurred unknown to me. I can only relate facts as far as they have come to my knowledge, and leave others to draw their conclusions. In the majority of the cases, I have ascertained that the patient has for a greater or lesser period, after the termination of the course, enjoyed immunity from the disease which had tortured him for years.

"Of other alteratives, as respects their efficiency in cutaneous disease, I know but little except from report. Of their very general *inefficiency* I had painful experience in the early part of my practice. I have long abjured medicated baths, and external applications generally; and, excepting where the antiphlogistic regimen was required, *I have placed no restriction whatever upon the diet of my patients.* I never could understand the principles on which the dietetic system of treating disease is founded. If the appetite is not depraved by gross intemperance, I look upon it to be the only safe indication of the proper quality and quantity of food required in a given case, both in health and disease. I do not say it may never prove false: and I will promise never to trust it, if any man will supply me with a more intelligent or more philosophical guide. Neither local treatment nor diet therefore, (with the above exception,) have had any influence in determining the results of my experiments. But in order to submit the agency of arsenic to a still more severe test, I have in almost every protracted case, interrupted the course again and again, and have found to my unspeakable satisfaction that, in every case unshackled by complications, I could as readily check the disease, and allow it to advance at pleasure, as the engineer can control the progress of his locomotive. The power of the medicine in these cases is therefore established beyond the reach of doubt or cavil. And its safety is not less demonstrable. By discovering the efficacy of its continued use in small and *decreasing* doses, and thus securing for the medicine an innocuous operation, I trust I have removed the only valid objection to its use, namely, its dangerous properties. And now, with the exception of its name, and the horrors associated with the idea of a poison, one can scarcely conceive a remedy less objectionable than arsenic. Sarsaparilla, besides being nearly useless, is expensive; the preparations of cantharides irritate the urinary organs; iodine, besides that it is 'no respecter of tissues,' is a nauseous medicine, and new patients could be prevailed upon to take it, were it safe to do so, for a lengthened period. But none of these objections apply to arsenic. A medicine which, besides being almost or quite certain in its operation, is safe, cheap, tasteless, and elegant—which may be taken at meal-times through a whole life, if necessary, without creating disgust or nausea,—which interferes (in curative doses) with no healthy function,—which gives no pain, and inflicts no inconvenience—has surely recommendations which are not easily surpassed." P. 134.

We must close our notice of Mr. Hunt's volume with the same words of hesitating belief with which we began it. His style of writing is not likely to beget

much confidence in his statements, among those of his professional brethren whose good opinion is most worth having. The practice of authors publishing patients' eulogistic and recommendatory letters cannot be too much censured. It always savors of the advertising school, to which too many of the medical authors in the present day give countenance by their own practice.

On Sir Charles Bell's Researches in the Nervous System. By *Alexander Shaw*, Surgeon to the Middlesex Hospital. 4to. pp. 40. Murray: London, 1847.

This Essay is appended to the posthumous edition of the well known "Anatomy and Philosophy of Expression, as connected with the Fine Arts." It contains a remarkably lucid exposition of Sir C. Bell's discoveries, written in a pleasing and very graceful style, and illustrated with many interesting facts drawn from the field of Comparative Anatomy. We must find room for one extract:—

"Man is distinguished above all other animals by his lower extremities having solidity and power sufficient to sustain his body without the aid of other members, and so as to be his sole organs of progression: hence his erect position. Again, as man's upper extremities are emancipated from the duty of assisting in locomotion, they are free to execute whatever rapid and varied movements may be called for, either for self-defence or for procuring nourishment. And, in correspondence with that freedom of action, a *Hand* is added, which, for the perfection of its endowments and mechanism, has been, in all ages, a constant theme of admiration.

"Now let us ask, what influence have the improvements thus shown in the construction of the organs of locomotion and of prehension, upon the structure of the Mouth? The chief use of the prehensile organs being to seize food for the supply of the mouth, it may be expected that, as they become more highly organized, the mouth will undergo a change in its form. What, then, is the effect of the improved organization of the prehensile organ, as seen in the hand of man, in allowing the mouth to be adapted for a vocal organ—an organ of Articulate Language?

"It has been stated that in all the vertebrate animals below man, the member analagous to the arm and hand, is an instrument of progression as well as of prehension. Whether we take the fin of the fish, the anterior extremity of the reptile, the wing of the bird, the paddle of the dolphin, or the fore leg of the horse or dog, the principal, if not the only use of the member is to assist in locomotion; only a few quadrupeds, like the squirrel, the feline animals, &c., besides using their paws for running, climbing, burrowing, &c., employ them to carry food to the mouth. Now the consequence of using the organ intended to convey the food to the mouth as one of progression, will be, that the office of appropriating the food will be thrown upon the mouth itself. Accordingly, in all animals below Man, the mouth is a prehensile, as well as a manducatory organ. If the animal be graminviorous, it must crop the herbage with its teeth before chewing and triturating it; if it be carnivorous, it must be provided with large, sharp fangs or tusks, to fight, seize, and tear its prey, before it can reduce its food to a fit state for swallowing. In short, the mouth, with its delicate sense of touch, its hairs or whiskers projecting from it as feelers, and its jaws armed with large teeth, is to be looked upon, in conjunction with the long, flexible neck commonly belonging to brutes, as combining the functions of the human arm and hand, with that of an organ of mastication.

"But it is obvious that a mouth of the large capacity and irregular shape of an animal like the horse, ox, dog, or lion, could never be adapted to produce articulate sounds. In a cavity such as the mouth of the horse, we can under-

stand how neighing may be produced; but we cannot suppose that, by any adjustment of the tongue or lips, the air, even if it were properly vocalized in the larynx, could be confined, and then be let suddenly free to give rise to explosive sounds; or be impinged against the palate, to cause guttural sounds; or be directed into the back of the nostrils, to produce nasal sounds. In short, none of those numerous, finely-varied changes in the shape of the interior of the mouth, produced by the combined action of the tongue, palate, cheeks, and lips, which give rise to the infinite modifications of sound in speech, could take place in such gross structures. For the vibrating air expelled from the larynx to be divided and modulated so as to produce words with proper tone and accent, it is necessary that the cavity of the mouth should be small, its boundaries regular and uninterrupted, and the communication between it and the nostrils, free.

"Now that is the very character of the mouth in Man." P. 35.

MEDICAL STATISTICS, THEIR FORCE AND FALLACY. By *James Duncan*, A.M.,M.D. 8vo. p. 42. Dublin, 1847.

INTRODUCTORY lectures are proverbial for their dullness, their repetitions, and the inordinate quantity of inanities they are the privileged medium for the communication of. Dr. Duncan has thought proper to break through the ordinary routine, and the consequence is the production of a very interesting exposition of some of the deductions derivable from the examination of the Registrar-General's and the British Army Reports, and of some of the errors to which an undue reliance upon figures may give rise to.

Dr. Duncan first adverts to *the different degrees in which the two sexes are affected by disease.* It is well known that a larger portion of males *are born.* The English (1838–41) Registrar and Irish Census (10 years) Returns, give 2,240,938 male for 2,142,272 female births, and yet at any period the actual number of the sexes living is reversed. The general population of Great Britain and Ireland in 1841 was 13,097,012 males and 13,742,873 females. This result can only occur from the excess of mortality in males, and the Reports above referred to register 1,318,853 male to 1,241,207 female deaths. Moreover, the difference does not occur at the advanced periods of life when the different habits of life might to some extent explain it, but in infancy, when every influencing condition of the sexes seems similar; so that for 406,684 male deaths under two years, there were but 344,935 females. After this period the sexes remain very similarly circumstanced until the period of puberty, when the difference is found to be in favor of the males until the 40th year. To about the 45th the deaths are nearly equal, after which until about 60, the males again form the excess—females constituting the majority of those who die in advanced years.

Dr. Duncan draws attention also to the *relative frequency of consumption in men and women.* Authors have been divided upon this point; but the Registrar's Report for 1838–40 gives us 90,611 deaths in a female population of 7,885,615, and 80,550 deaths in a male population of 7,668,245; but it is a curious fact, that while consumption is more frequent among *females* in the *general* and *rural* populations, it is more frequent among *males* in *cities* and *large towns;* for while in 100,000 of the general population there would be but 378 male and 408 female deaths, in that of London there would be 451 males to 377 females, and in Birmingham 526 males to 410 females.

We cannot follow the author in his examination of the proportion of deaths caused by consumption in our military service in different parts of the world, important as information derived from such considerations is in confirming or correcting the prevalent ideas upon the effects of climate in this dreadful disease; and we must content ourselves by very briefly adverting to some of his observations upon the abuse of figures. For example, the superiority of any given in-

stitution in the number of cures it produces should never be affirmed until the rules governing the admission, diet, &c. of patients are understood. The ratio of mortality without this may be apparently, but not really, much greater in a given disease in a workhouse than in a hospital, or in two institutions ostensibly similar. The author agrees with those who regard the numerical method as incompetent to aid in the treatment of disease.

"A number of cases of a disease, say delirium tremens, are treated in a particular way, and the result is compared with an equal number of cases treated differently, and a conclusion is drawn in favour of the plan which appears to secure the largest number of recoveries out of the entire. To this theory as thus explained there are many objections. Let us suppose, however, that the trial has been made; to what practical result, I ask, can it possibly lead? Are we to understand that, having found out a certain plan to be successful in a large majority of cases of any disease, that it is to be applied, with unbending exactness, to every other example of it we may meet with? Is there to be no regard to the stage of the disease, the constitution of the patient, the character of the prevailing epidemic? If there is, of what value is the supposed statistical announcement, to guide us in the varying circumstances in which we may be placed? If such distinctions are disregarded, in what is our conduct less absurd than that of the mere pretender to physic, who, with a boldness only unequalled by the credulity of the public, invents his specific, and declares that it is a panacea for all diseases—that it is suited to all constitutions, and, that while it will infallibly cure, it will do no harm?"

REMARKS ON THE DIET OF CHILDREN; AND ON THE DISTINCTIONS BETWEEN THE DIGESTIVE POWERS OF THE INFANT AND THE ADULT. By *George T. Gream*, one of the principal Medical Officers of the Queen Charlotte's Lying-in Hospital, &c. &c. Small 8vo, pp. 201. London: Longman and Co. 1847.

WE should have been quite at a loss to know why Mr. Gream should have published this book, if he had not been so obliging as, in the concluding paragraph of it, to assign his reasons for doing so.

"In bringing to a conclusion," he says, "these practical remarks, which have been almost extorted from me by the sufferings to which numbers of children are exposed, as I daily witness, through the ignorance or thoughtlessness of their attendants, I venture to express a hope that they may not only prove useful, in the domestic circle, among parents, and others who have the charge of infancy and youth, but that they may, as tending in however humble a degree, to advance and enforce their own views and principles, obtain a favourable reception from those of my own profession, to whom we are indebted for the establishment, on scientific grounds, of those practical and judicious regulations for the management of children, which are now more or less adopted by the most eminent and successful practitioners." P. 201.

We fear, from the loose, superficial, and common-place character of Mr. Gream's observations, that his most alluring motive was the not very disinterested one of being useful in the *domestic circle, among parents, and others who have the charge of youth*. We think it just possible that, in this respect, the book may answer the purpose; because the persons to whom it is addressed are not the most discriminating critics of a medical author's merits, and Mr. Gream has several very comforting precedents of similar popular books having puffed their authors into practice. After these remarks we need scarcely say that, to well-educated medical men, or even to pupils, Mr. Gream's book is not likely to be of use; and that it is more likely to be a vade-mecum for Mr. Gream himself, than to have any more extensive application

Periscope;

OR,

CIRCUMSPECTIVE REVIEW.

Selections from the Foreign Periodicals.

On Letting Blood from the Jugular in the Diseases of Children. By C. Hildreth, M.D. Zanesville, Ohio.

Dr. Hildreth, after observing that leeches, which are usually recommended in the acute affections of infancy, are sometimes not obtainable without great difficulty, goes on to state his opinion, that general bleeding in many of these is far preferable, and that the jugular vein is the one best adapted for the purpose at this period of life. "I would make this operation the rule instead of the exception in many of the acute affections of children under two or three years of age; and also in older subjects, in certain diseases of the brain and trachea. As the head in infancy is larger in proportion to the size of the body than in the adult, so also is the cerebral vascular system more developed, and hence we find the jugulars relatively larger and more prominent in the first years of life." The veins of the arm at this early age are found with great difficulty, and a sufficiency of blood hard to be obtained from them, while the near proximity of an artery has prevented practitioners opening a vein in cases where the loss of blood was urgently called for.

Dr. Hildreth considers the fear of air entering the vein as quite visionary, providing the operation has been properly performed; and believes this mode of abstracting blood as especially efficacious in inflammation of the larynx and trachea, cerebral inflammations, and congestion causing convulsions. "We also much prefer letting blood from the jugular in the acute inflammations of the thoracic viscera. One of its chief advantages is the great rapidity with which blood flows from a free orifice in the vein. A decided impression can thus be made upon the system in a very few moments, and *with much less loss of blood than would be required to produce the same effect* from a vein in the arm, or by the still slower process of leeching or cupping."

The following are Dr. H.'s directions for performing the operation in a more simplified manner than is usually recommended. "It is essentially necessary in letting blood from the jugular of a refractory child, *that the head and chest should be immovably fixed*, otherwise the flow of blood will be interrupted or stopped entirely. The nurse, having exposed the right shoulder of the child, seats herself upon a low chair, and in holding the child across her knees carefully confines its arms. The surgeon, seated at her side, receives and secures the child's head between his knees. With the thumb of the left hand he now compresses the jugular across where it crosses the first rib, while the remaining portion of the same hand is employed in fixing the chest of the child against the person of the nurse. The right hand of the operator being at liberty, he makes the opening into the vein. The blood is received in a cup, the edge of which applied a little below the orifice, likewise serves to compress the vein. From a robust child blood escapes with astonishing rapidity, particularly when he cries or struggles. No effort, therefore, should be made to keep him quiet, his cries being rather encouraged, as they expedite very much the operation. A sufficient quantity of blood is very soon lost, the colour of the lips and cheek of the child telling when syncope approaches much more certainly than

the pulse. The quantity of blood desired having been lost, a compress is applied to the orifice, and the pressure taken off from the vein below. After the child becomes quiet, the compress is removed, and the wound closed by a piece of court or adhesive plaster. This is much the best dressing, the compress and bandage usually advised being very objectionable. It not only obstructs the free return of blood from the head, but its presence irritates the patient, and if not well adjusted may promote the flow of blood or interfere with respiration. If, however, all pressure be removed from the vein below, blood will not escape if no dressing be applied, except the child cry or struggle. We therefore much prefer, if the proper dressing be not convenient, leaving the orifice uncovered, and directing the nurse to apply the compress for a moment, should blood escape during the cries of the child."

However young the child may be, Dr. Hildreth feels convinced that, in the above-named inflammatory diseases (he prefers leeches in inflammations of mucous membranes in general and of the abdominal viscera in particular) accompanied with high fever, a smaller quantity of blood so abstracted will suffice than if it had been taken in the ordinary manner—and in young children the jugular is the only vein of sufficient size to admit of the certain and safe abstraction of blood. He relates a few cases in proof of the prompt benefit occurring from the operation."—*American Journal Med. Sciences*, No. 26, pp. 369, 74.

[We feel great pleasure in giving an abstract of the above paper, as we feel convinced it draws attention to an important practical fact. We are no advocates for depletion of children to the extent sometimes recommended; but in cases wherein the prompt loss of a certain amount of blood seems indicated, we agree with Dr. Hildreth, that it may oftentimes be more advantageously abstracted by jugularotomy than by leeches. We shall not soon forget the instant and marked relief we once saw thus produced in a case of croup.—*Rev.*]

Observations Introductory to a Course of Clinical Surgery. By M. Malgaigne.

From the time of Celsus certain physical qualities have been expected of the surgeon, namely, that he should be young, adroit, and ambi-dextrous. M. Mayor of Lausanne would send him to study among the carpenters and joiners; in a word, his has been looked upon as a mere mechanical art, *quod in therapeiâ mecanicum*, according to the expression of Richerand. But is this the only employment of the surgeon? Is it as in the middle ages, when the physician sent for him to perform certain mechanical operations and then dismissed him? No! After having been so long a subaltern, the surgeon must now take that rank which is his due. The physician may be ignorant of surgery, but the surgeon should know medicine; for in how many instances are surgical affections preceded or followed by phenomena which involve the entire economy! That he should be adroit can but be of great utility to him; but as to being ambi-dexter, few persons can boast of this, and frequently those who can, as Rivarol observes, have only two left hands.

For our parts, what we demand of the surgeon before all things is, that he have a healthy, cultivated, and philosophical mind, and a high moral character, and that he should not be, as Celsus would have him, without pity. Surgical morality is of especial importance in respect to new operations—experiments to be tried on the living man. We must think more than once when human life is at issue. He should possess a philosophic spirit, in order that he may verify before he believes, not accepting the dictums of his teacher without examination. Faith in authority at the present day must yield to faith in facts. His mind should

be of a logical cast, so that he may not mistake analogies for proofs, or draw too hasty conclusions, such as inferring, for example, from the phenomena observed in frogs, the certainty of similar facts in man.

From the time of J. L. Petit, surgery and medicine have been based upon normal and pathological anatomy. This is an error. Normal anatomy is truly a great aid, for it teaches you the position of the nerves and blood-vessels, and of all the internal organs. It is essential for the mechanical part of surgery or operative medicine. But has not surgery just as much need of cutlery and pharmacy? and should we therefore not accord them as important a position as anatomy? The true basis of surgery is *Physiology;* but not the physiology which has been up to the present time almost exclusively taught you, and which has been termed the "Romance of Medicine." Of what use in practice is such physiological knowledge? How are your therapeutical procedures influenced by knowing that it is the heart which propels the blood, the lungs which respire, the liver which secretes the bile, or the kidneys the urine? Even the discovery of the circulation has not advanced the treatment of disease a step. Before Harvey's time Botal bled *coup sur coup*, and while recognizing what there is of beautiful in this discovery (for the truth is always beautiful), we may almost say that, from that epoch, therapeutics, as regards blood-letting, have pursued a retrograde course. In fact, experience seems to have led the ancients to bleed from a variety of veins; and upon what grounds is this practice abandoned?

True medical and surgical physiology, that which employs itself in discovering the manner in which Nature repairs the breaches made in the human body, dates from JOHN HUNTER. He in fact first investigated the manner in which she comforts herself in the healthy man for the resistance to causes of disturbance or ruin. Physiology, so understood, forms the basis of the rational part of surgery. It will direct us to the study in its origin, its cause, its progress, and its consequences, of that great phenomenon, inflammation. It will show us how to direct or repress, or to excite it, as the case may be. Reflect upon the import of this change of basis—physiology in the place of anatomy—and you perceive in it consists an entire revolution in the art of surgery.

There is something else besides his organs which appertains to man. The organs exist in the dead body. There is besides in the living body a power whose nature is unknown, a power which circulates the blood, generates motion and thought, and which offers resistance when an external agent deranges that order which constitutes health. It has been differently regarded; some considering it as dependent upon physical or chemical laws, others as mechanical. But can we in this way explain even the most ordinary fact? You cut your finger; the blood flows, and then stops after the formation of a clot. Oh! it is the heart, it is said, a hydraulic machine, which propels the blood until the obstacle produced by chemical decomposition closes the vessels. In nearly the same manner may the red circle surrounding the wound be explained. Exosmosis then suffices; just as endosmosis explains its disappearance. But why do not these phenomena thus come to pass in every case? Why do they occur at one period rather than another? Why do we not see them on the dead body, while it differs not as regards its chemical relations from the living man? It is because *vital force* exists no longer. What is this vital force, then? No one knows. We admit it just as we do attraction and chemical affinity. But what matters its nature? Sufficient for us to know its action. This knowledge is however of the highest import; for it is only by studying the phenomena by which it reveals itself, by penetrating the laws which govern it, that we can learn how to direct it. Let us examine well then how it acts in order to resist external agents; but, above all, let us study it in man, and preserve ourselves from the too common error, the abuse of experiments upon animals. These are useful only when they are made upon the nearest approaching species, or upon such as regards the point in question are identical. We could not, for example, describe inflammation in

man according to the phenomena observed in the dog, in which there is hardly any suppuration, or the rabbit, in which it exists not at all. Let us then investigate the mode of action of the vital force, and first let us observe it under the most simple circumstances. We have received a cut; the blood flows, then stops; the capillaries are injected, and inflammation is established. If we leave Nature alone, and trust to the vital force, we observe that it is essentially a *vis medicatrix*, and that in this case it alone suffices; whence the conclusion, that in *traumatic inflammation the surgeon's first duty is to do nothing*. Do nothing! An important dictum this that needs interpretation. Nature, at the commencement, we have said, is essentially a medicatrix, but upon the condition that her work is not interfered with. Wait to act until she wanders or deviates, and then intervene, to place her again in the right path. Watch like a vigilant sentinel, in order to remove whatever may interrupt her proceedings. To fulfil this great therapeutical indication, which of itself constitutes three-fourths of surgery, we are obliged to modify the treatment of the most obstinate as well as the most simple diseases. What has a surgeon to do in a simple fracture? Nothing. Nature alone will well repair the damage, provided she is protected from interruption during her labours. Among the most frequent obstacles is movement of the parts; and our care is limited to securing, by the best-adapted apparatus, the complete immovability of the part. As to bandages, what is their effect but to deprive the part of the vivifying action of the air, and etiolate it, even if they do not produce a local scorbutus. We may say as much of resolvents, such as white lotion, spirits of camphor, &c. What is there to resolve in a fracture? The swelling which accompanies the inflammation? Is it not necessary for the consolidation? The effused blood? Nature will remove that without your aid. If there is displacement of the fragments, the hand of the surgeon is required: and this is mechanical, or, if you like, operative medicine. Restore the form of the limb, fix it conveniently, and retire. Let Nature resume her place, and do not say that you have effected a cure. You have reduced the fracture; the cure comes from Nature alone.

Simple surveillance is also our essential and only duty in all those simple inflammations, which in all their symptoms and progress are analagous to traumatic inflammation, and which come on from unknown causes, or under the problematical influence of cold, draughts of air, &c. The same principles will apply, *e. g.* to white swellings, diseases whose cause is often traumatic, and which continue long in the condition of simple inflammation before acquiring the gravity which subsequently characterizes them. At the commencement, and indeed as long as the disease has not passed beyond the limits of simple inflammation, prevent the movement of the part, that so frequent obstacle to the efforts of Nature. Remove all causes of pain, and especially such as arise from faulty position, of such common occurrence when the disease has been of long continuance; and discard all that perturbatory medicine which only can make use of leeches, blisters and moxas. Of these means, the employment of which now seems the general rule, some may be useful in rare and special cases; but what shall we say to a blister put on for the relief of pain which only depended upon faulty position? The common result of such therapeutic procedure is to convert an ordinary synovitis into a fungous tumour, and to give the surgeon occasion to show his dexterity in depriving the patient of a limb, which, by proper care, he might have preserved for him.

The considerations we have entered into give rise to new views, which may serve to establish the superiority of surgery over medicine. The facility with which we may ensure immovability to a limb, and the impossibility of procuring the inactivity of the heart, lungs, &c. explain why surgical inflammations are generally so much easier of cure than medical ones. In conclusion, expectation in surgery cannot be too highly recommended; but an intelligent expectation. Nature is not always a *vis medicatrix*, and then it will not do to remain a simple

spectator. Direct the vital force, govern it, resist it if necessary by the most energetic surgical means; and even in certain desperate cases abandon it to itself. Resume your passive character, the power of diagnosis and prognosis alone remaining to you. It is better to deplore a case as desperate, and to let the patient die at least naturally, than hasten his end by an operation that nothing can justify. —*Gazette des Hôpitaux*, No. 57.

Treatment of Epilepsy.

While detailing some unsuccessful experiments he had been making upon the employment of Ether in epilepsy, M. Moreau delivered the following important observations:—"Every medicinal substance has in turn failed when employed against this terrible neurosis, as everybody knows. But what every one does not so well know is that all, or almost all, these remedies succeed during the first periods of their employment. That is to say, the attacks diminish, or cease entirely, and even suddenly. But just in proportion to the extent of the apparent amelioration, the more the attacks seem to have yielded, the more terrible do they become when they are reproduced. They may then even place life itself in danger, which they never did before. This fact is of great practical importance, and I have seen hundreds of examples of it during the now near seven years that I have sought, by all the remedies proposed at home or abroad, and some of my own devising, to combat this dreadful disease.—*Gazette des Hôpitaux*, No. 38.

Treatment of Burns.

M. Jobert and M. Malgaigne pursue very different modes of treating Burns at the St. Louis, where a great number of these cases are admitted, M. Jobert employs *refrigerants*, laying bladders of ice upon the parts, previously covered with simple dressing. From this treatment he states that he obtains, 1, prompt cessation of pain; 2, an abolition or diminution of inflammatory action; 3, a diminution of the suppuration which precedes or follows the separation of the eschars; and 4, a more rapid and prompt cicatrization; but it is, however, contra-indicated when thoracic complications exist, or when a predisposition to these is present; or when the burn, situated in the chest or back is very extensive, M. Malgaigne prefers the oleo-calcarous liniment and carded cotton; associating the two because the cotton alone does not so rapidly relieve the pain, and the liniment alone does not protect the surface from friction. Like M. Valpeau, he prefers equal parts of lime-water and olive-oil; but M. Miquel recommends 2 parts of the former to 1 of the oil. This produces a speedy cessation of the pain.—*Gazette des Hôpitaux*, No. 59.

[We have employed with much advantage the application recommended by Mr. Bulling, viz., covering the parts with lint soaked in a mixture of 1 part treacle, 3 water—using it at a temperature of 98°.—*Rev.*]

Ectrotic Treatment of Small-Pox.

Dr. S. Jackson states that he has found the free application of the Tincture of Iodine by means of a sponge, causes an abortion of the eruption, and prevents pitting or the disagreeable discolouration which usually follows the disease.—*Philadelphia Medical Examiner.*

NOTES OF CASES OF CHRONIC DIFFUSED ARTERITIS, CURED BY THE ANTIPHLOGISTIC METHOD, AFTER THE FAILURE OF DIFFERENT MODES OF TREATMENT IN THE HANDS OF OTHERS. By Dr. RININO of Turin.

The greater importance most physicians attach to mere symptomatic appearances than to pathological conditions in the diagnosis and treatment of diseases, especially those of a slow and chronic nature, is the frequent cause of the most pernicious errors, into which even practitioners otherwise of great excellence fall; errors which have often led patients to certain death, who, treated conformably to the maxims of modern experience (based upon facts examined by the light of a philosophical pathology and guided by pathological anatomy,) might have recovered their lost health, and lived for many years. Such errors are more easily committed in those cases in which certain chronic diseases are marked by nervous symptoms, as in the case of hysteria, hypocondriasis, believed by many practitioners to be dependent upon a totally different state to that of phlogosis. Add to this that, when treating patients of emaciated or cachectic habits, the subjects of hæmorrhage or discharges, women, children, and old persons, ill-fed individuals, such as are subject to prolonged mental employment or abuses of any kind, we are led to suspect their complaints are of an asthenic rather than of an inflammatory nature. And yet, if the symptoms be accurately examined, the indications of slow inflammation will be generally detected; whence it follows that the treatment pursued in these emergencies may be actually injurious, or if too bland and palliative, will allow the patient to die from the degeneration of tissue and other consequences unsubdued inflammatory action produces. What is true in a generel way in regard to morbid conditions dependent upon slow inflammation mistaken and badly treated by practitioners, applies to *chronic diffused arteritis*, of which I now publish a few of the more important cases that have come under my notice during a short space of time.

The *first* of these occurred in the person of a lady, who, having arrived at the time of life when the menses were leaving her, was tormented at the periods by various vague pains in different parts of her body, and vibratory pulsation at the præcordium, the neck and temples. Sleepless nights, bad digestion, and confirmed melancholy existed. Two practitioners, consulted in succession, observing the feeble state of the patient and the absence of fever, pronounced her ailments of a nervous and asthenic character, and prescribed the usual remedies for these conditions. Dr. Rinino was now called in, and finding the pulse frequent, "metallic," and vibratory, he pronounced the case one of arteritis, notwithstanding the absence of fever. Ten bleedings were practised in the course of a fortnight, leeches and then ice applied, a rigorous diet adhered to, and various contra-stimulants prescribed, and the lady entirely recovered her health, which she retained by taking the precaution of losing blood two or three times a year.

Case 2.—Signor Panizza had suffered for some years under the symptoms of hypochondriasis. Indigestion, epigastric pain, costiveness, emaciation, insomnia, a chlorotic aspect, and confirmed melancholy were among the symptoms which induced his attendants to consider his case one of asthenic hypochondriasis, and to treat it unavailingly by nutritious diet, pharmaceutical stimuli, and mental distractions. Afterwards, some congestion of the liver being suspected, leeches to the anus and purgatives were prescribed. By the first plan of treatment all his symptoms were aggravated, by the latter only temporarily relieved, and the patient seemed fast sinking into a hopeless state when the author was called in. Notwithstanding the feeble condition of the patient, he declared a chronic arteritis

to be the cause of his sufferings, and prescribed free depletion for its removal. He based his diagnosis upon a constant arterial vibration felt and heard at the epigastrium, and upon the sharp metallic character of the pulse—the patient being however apyretic. Nine bleedings and a leeching rapidly followed each other, a rigorous diet and contra-stimulant remedies being simultaneously employed, and in forty-five days the patient was convalescent.

Case 3.—Signor Sasso (ætat 60) was seized, in 1845, with a complicated condition of ill-health, the chief feature of which was diagnosticated by his attendants as ischiatic neurosis, and treated with palliatives, without effect, until at last the patient seemed to be in a state of great danger. He was of a very sanguine temperament and weak constitution, accustomed to good living and little exercise. He complained exceedingly of an epigastric pulsation, which tormented him whatever position he assumed, and, although he had no fever, he presented the rapid, metallic, vibratory pulse observable in chronic arteritis. He suffered acute pain in the hip-joint, which the slightest movement exasperated. Nine successive bleedings and a free leeching with a spare diet were resorted to, and the various contra-stimuli, especially kermes, extract of aconite, and secale cornutum, alternating with purgatives, were prescribed. During a month convalescence was satisfactory: insomnia was exchanged for refreshing rest, and the supposed sciatica, which the author believes to have been an arthritis, gradually disappeared.

Case 4—Occurred in the person of a girl of about four years of age, who for two months had had the eyes closed in consequence of an obstinate psorophthalmia, the supposed residuum of a crusta lactea, and against which various demulcent remedies had been in vain employed. Dr. R. was called in, in consequence of a profuse epistaxis, and was struck with the marked arterial vibration in spite of the loss of blood, which amounted to almost two pounds (Austrian). He informed the parents that the epistaxis was in fact a fortunate occurrence, and eventually persuaded them to allow him to extract yet more blood (which was cupped and buffed) on three separate occasions. Having thus, in some degree, quieted the arterial vibration, he prescribed kermes in union with the extract of aconite and Dover's powder; and in a fortnight the child was able to open her eyes, the psorophthalmia and arteritis gradually yielding to a persistence in the medicines.

Case 5.—No one, says the author, accustomed to the examination of these cases could be deceived in diagnosing chronic arteritis as the cause of the serious and prolonged illness of Signora Pinta. Her countenance was the colour of virgin wax; vigorous pulsations prevailed at the præcordium, accompanied by a sense of weight and anxiety; the cardiac pulsations imparted a very vibratory sensation to the ear, and the pulse was strong and metallic. The arteries of the neck and temples also pulsated rigorously. There was no fever. A sense of universal uneasiness tormented the patient, and in turns violent pains attacked the abdomen and head. She was absolutely sleepless, had become much emaciated, and laboured under an inexpressible sense of prostration of strength. Her illness dated four months back, when Dr. R. saw her, and at first putting on a febrile aspect, her attendant had bled her four times with temporary relief, and other four times after she had again relapsed. Other assistance being sought, her yellowish aspect and wasting away, and the duration of her illness led to the belief of the existence of a passive congestion of the liver, dependent upon an asthenic diathesis. After leeching the anus, stimulating remedies were employed with the effect of plunging the patient in a worse state than ever. Our author, called in, quickly corrected the error of diagnosis, and proceeding more actively in the same course of the more timid practitioner first consulted, in a very few days had bled the patient thirteen times to the extent of a pound, besides leeching her. In a month she had entirely recovered.—*Annali Universali*, Vol. cxviii. 29.

[*Other cases are adduced, but these are the principal ones, and considering that whenever Dr. Rinino specifies the amount of blood taken, he states this to have been a pound, and that his bleedings were sometimes repeated five, ten, or even thirteen times, and that for not very well characterized chronic inflammation, he proves himself to be a worthier follower in the footsteps of Guy Patin than is often met with in these degenerate times. The lavish use of the lancet in fact prevails, as we have observed in our notice upon Tommasini (p. 171), to a greater extent in Italy than in any other European country; but we imagine that, even there, Dr. R. will make few proselytes. That cases may every now and then be met with which may be benefited by such treatment we do not doubt, and the placing them on record has its utility by exciting attention to the important fact indicated by the author, and certainly too frequently overlooked, that the essential inflammatory character of a disease may be masked or concealed by a train of pseudo-asthentic and nervous symptoms, which have supervened. Infinite mischief would however result, if this exceptional case were generalized into a principle.]—*Rev.*

On Intercostal Neuritis and Neuralgia. By Dr. Beau.

Since the researches of MM. Bassereau and Valleix *intercostal neuralgia* has taken its place among the acknowledged affections of the economy; but with it other cases, for which the term *neuritis* would be more applicable, have been confounded. Dr. Beau's attention was first directed to the subject while contemplating the nature of the painful sensations in injuries of the ribs. Of two such cases, in the one case a severe contusion of the thorax, and in the other actual fracture, took place at the junction of the posterior and middle thirds of the ribs; and in both cases, while some degree of pain existed at the precise seat of injury, that of an intense character was located anteriorly near the sternum. It was the latter that became intolerably increased by coughing, sneezing, or other respiratory efforts. In these cases the pain was explicable only on the supposition of an inflamed state of the intercostal nerve consequent upon the injuries, the severest suffering being referred to the periphery in consonance with a well-known law.

Neither of these patients dying, the positive proof of the existence of such neuritis was wanting; but these cases led to the consideration of others of much more common occurrence, in which the existence of the peripheric pain and means of proving its dependence upon an inflamed state of the nerve alike exist. Such are cases of *inflammation of the pleura, whether simple or complicated with pneumonia.* It is familiarly known that the "pain in the side," so constantly present in these, is seated in the great majority of cases near the breast. It is, in fact, but the expression of pain at the peripheric extremety of the intercostal nerve, induced by inflammation of the portion of this nerve which is in contact with the inflamed pleura. The posterior portion of the nerve alone is inflamed, and yet the severe pain is excited at its periphery.

The intercostal nerves during the posterior portion of their course, that is from the articulation of the ribs to their angle, are in immediate contact, on the external side, with the external intercostalmuscle, and, on the internal side, with the parietal layer of the pleura. From the angle of the ribs to their termination, the nerves cease to be in immediate relation with the pleura, being separated from it in all the rest of their course by the internal intercostal muscle. It seems scarcely possible for the nerve to be so closely in relation to the inflamed pleura

---

* For an account of similar cases by Tommasini see Med.-Chir. Rev. N. S. I., 300.

without its participating in the diseased action; and, in point of fact, at post-mortem examinations we always find this portion of the nerve more or less inflamed during the whole portion of its course that is in contact with the inflamed pleura, such inflammation not extending beyond the angle of the ribs, where the nerve becomes separated by the muscles from the pleura. There is frequently a somewhat intense injection, not only of the neurilema, but of the nerve itself, with enlargements of its substance, as may be seen by comparing it with the uninflamed nerves in contact with uninflamed portions of pleura. The inflamed nerve has not seemed more friable than the others, but is sometimes slightly adherent to the contiguous pleura. It is to be remembered that pleurisies, and pleuro-pneumonias, are situated in the great majority of instances at the posterior portion of the chest, and yet the pain is felt at its anterior portion, as already observed. If this statement be correct, the pain induced at the anterior extremities of the intercostal nerves should vary in its longitudinal direction according to the height in the thorax at which the pleuritic inflammation is seated, and this is precisely what takes place; for, accordingly as the pleurisy affects the four or five first, or the four or five lower intercostal nerves, so is the pain felt at the anterior portion of the corresponding intercostal spaces. And, as the anterior extremities of the five last nerves, instead of turning up with the cartilages, proceed downwards and forwards, between the muscles of the abdominal parietes, towards the median line, the pain proceeding from the inflamed pleura is then manifested in the abdomen. It results from these details, that the seat of the peripheric pain of the inflamed nerve may serve as an excellent guide to the exact seat of the pleurisy, as all we have to do is to trace directly backwards along the course of the affected nerve. If local bleeding, applied to the seat of pain instead of the seat of the neuritis, readily dissipates the pains, it does so because it operates derivation at a certain distance from the inflamed part upon the intercostal vessels feeding the inflammation—just as, in orchitis, we place leeches over the cord, and not upon the scrotum.

Ordinarily all the nerves in contact with the inflamed pleura are equally inflamed, but all are not equally painful at their extremity. It will be found in general, that that nerve is most affected which corresponds to the rib possessed of most extensive movements. This is why, in most cases, the patients refer the most vivid pain to the anterior portion of the sixth or seventh intercostal space, because in most patients, and especially in men, the seventh rib is that which executes the greatest amount of movement. The patients will generally complain of pain at one of the intercostal spaces, but it is rare for only one nerve to be thus affected; and if we compress the spaces adjoining that at which the sensations of the patient seem to be centered, we find that others are similarly affected, though in different degrees. The difference in the intensity of suffering is very great; for, while some nerves are excessively painful, others, equally inflamed, give signs of scarcely any pain. Differences in pathological susceptibility analogous to this are however familiar to attentive observers; and it is the entire absence of such susceptibility in certain individuals that permits *latent* pluerisy and pluro-pneumonia to become developed without the manifestation of pain in the side, or any other symptom of the disease.

We have hitherto laid down as a law, that the posterior inflamed portion of the nerve *only* manifests pain at its anterior extremity; but there are some exceptions to this. We have observed, in the most careful manner, cases of pleurisy, in which pain existed simultaneously at the extremities of the intercostal nerves, and at the portion of the spinal column corresponding to the affected nerves. The latter pain is not however spontaneous like the former, but for its induction requires slight pressure to be made on the side of the spinous processes corresponding to the inflamed nerves, and then as many painful points will be recognized posteriorly as anteriorly. Every one is aware that, during percussion of the posterior portion of the thorax in pleurisy, pain is produced.

This is always referred to the inflamed pleura, but in fact is a posterior radiation of the inflamed intercostals. This pain at the posterior portion of the thorax is not fixed, as the anterior pain in the intercostal branch properly so called, but in the branch which terminates in the muscles and skin of the back; and yet in necroscopies we are enabled to show that this dorsal branch is no more inflamed than is the anterior extremity of the intercostal nerve, the pain being, in the one case as in the other, a distant result of inflammation affecting the portion of nerve in contact with the inflamed pleura.

These pains of the side, then, commonly termed *pleuritic*, are justly so called, on account of their relation to pleurisy. But pleurisy does not produce them *directly*, inasmuch as they result from the inflamed state of the proximal extremity of the nerve. The pains which continue to be felt after the cessation of a pleurisy, and which are usually referred to adhesions, are, in point of fact, produced by the neuritis become chronic. When there is inflammation of the lung without inflamed pleura, we have then no pains in the side, no neuritis capable of producing them having been generated. There is another form of pleurisy, in which the intercostal nerves are liable to become inflamed—that which is consecutive to pulmonary tubercle, and which is then seated at the upper part of the chest. The pain resulting from this is felt at the anterior part of the first intercostal spaces, but is much less severe than that of acute pleurisy. Those dull pains existing just under the clavicles, and which, according to pathologists, are a frequent symptom, and an immediate result of the presence of tubercles, are in fact, produced by the development of pleuritis consecutively to the tubercle. Besides these pains, phthisical patients occasionally suffer from others in the supra-clavicular region of a far more intense character, forcing cries from the patient, and requiring the endermic use of morphia for their relief. These, in all probability, depend upon a neuritis of the first intercostal nerve, which sends one of its branches to anastomose with the brachial plexus. This last is in communication with the cervical plexus, and we can understand how the neuritis of the first intercostal may in this way induce pain in the region of the neck; and even down the arm.

In comparing intercostal neuritis with *intercostal neuralgia*, we should first distinguish the varieties of this last. The most important of these is that described by M. Bassereau as "commonly sympathetic of an affection of some viscus, whose suffering is transmitted to the intercostal nerves by means of the anastomoses of the great splanchnic." M. Bassereau believes the uterus and its appendages to be the seat of the irritation thus propagated, inasmuch as women are much oftener the subjects of intercostal neuralgia than men, and that the women so affected, in the majority of cases, are suffering from some disturbance in the uterine functions. M. Beau demurs to this latter conclusion, believing that disorder of the digestive organs is the point of departure of the neuralgia; for—1, the great splanchnic is in communication with the semilunar ganglions and lunar plexus; 2, although these females are suffering from derangement of the uterine functions, they are so in a much more marked degree from that of the digestive organs; and, 3, that in all the male patients liable to this neuralgia, the number of whom is greater than M. Bassereau believes, there is a marked disorder of these. Dyspeptic symptoms need not be excessive, and yet the disorder they indicate may have a pathogenic influence upon the various organs. So connected with dyspepsia has M. Beau long considered this neuralgia, that he always terms it in his clinical lectures the *dyspeptic neuralgia*. Wherever such neuralgia disappears completely, the digestive functions have recovered their normal integrity; and to combat the neuralgia effectually we must attack the dyspepsia—all means directed to the relief of the former, without attention to the latter, being merely temporary and palliative in their operation. This *dyspeptic neuralgia* affects principally the nerves corresponding to the ganglions which furnish the constituent branches of the trisplanchnic nerve, that is to say

the intercostal nerves comprised between the 5th, 6th, and 7th intercostal spaces. As in neuritis, there is always one nerve more affected than the neighbouring ones, and that corresponding to the rib possessed of the most extensive movements. Generally five or six intercostal spaces are simultaneously attacked, although in different degrees. The neuralgia, as shown by M. Valleix, also frequently presents three painful points: one at the termination of the intercostal branch, another where the middle perforating branch is given off, and the third over the dorsal branch, near the spinous processes. Its duration is generally chronic, like that of the dyspepsia upon which it depends, and during its progress it exhibits sometimes regular, but generally irregular intermissions.

The second variety of intercostal neuralgia is that dependent upon rheumatism, *rheumatic neuralgia*, commonly termed *pleurodynia*. Very frequently only one of the intercostal nerves is affected, but the pain is very intense, especially if excited by pressure. It sometimes reaches the extent of preventing the patient laying down, and impeding the respiratory movements, which become short, irregular, jerking, and accompanied by interrupted exclamations. It is worse at night than by day, the maximum of its intensity being seated at the anterior portion of the intercostal nerve. It may be sometimes excited posteriorly by pressure over the dorsal branch of the nerve, but it never spontaneously arises there, as it so frequently does in neuralgia of a dyspeptic origin. This acute form only continues for some days, and may be accompanied by fever, when it puts on the greatest resemblance to neuritis. It affects men as frequently as women, while dyspeptic neuralgia, just as dyspepsia itself, most frequently affects women.

In comparing neuritis with these neuralgias, we observe that their symptoms have much resemblance, especially as regards rheumatic neuralgia. The pain of this, as of neuritis felt towards the anterior portion of the intercostal space, is very intense. It is less so in the dyspeptic variety, and the patient in the latter frequently complains of pain over the dorsal branch of the nerve, which in neuritis or rheumatic neuralgia is generally only produced upon pressure. The dyspeptic form especially affects the nerves between the 5th and 7th rib, while the seat of pain varies in the others according to that of the pleurisy, or the part affected by the cold, which has induced the rheumatism. Dyspeptic neuralgia is liable to frequent intermissions and exacerbations, which neuritis and rheumatic neurosis rarely are.

"The ideas, so long since considered as classical, respecting the vivid sensibility of the pleura and the pungent kind of pain resulting from its inflammation, ought, I believe, to be discarded, seeing that the acute and pungent pains of pleurisy do not proceed immediately from the inflamed pleura, but from the intercostal nerves, which the inflammation of the pleura has invaded."—*Archives Generales*, T. 13, pp. 161—181.

[This paper must be regarded as containing an interesting and ingenious suggestion on the part of Dr. Beau rather than a demonstration; for he particularises no anatomical inspections that he has made in corroboration of his views. The accuracy of the conclusions it contains will however doubtless be soon sufficiently tested.—*Rev.*]

On Exostoses and the Operations they require. By M. Roux.

Pathologists have comprehended under the general term Exostosis affections of an entirely different nature. In this way have been confounded together—1, Simple, chronic, partial or general hypertrophy of a bone: 2, Osteosis with circumscribed swelling of the affected bone, so often observed as a symptom of constitutional syphilis: 3, Aneurismal tumours of bone; 4, Sarcomatous swelling;

5, The degeneration termed by A. Cooper fungus or medullary exostosis: and 6, those tumours, organized like the bony tissue itself, which spring from the bone like natural apophyses, or exhibit themselves as great tubercles or excrescences within the external layers of the bone, upon which they seem as if implanted. These last are, properly speaking, exostoses, and it is to these alone attention is directed in this paper. I distinguish them carefully from those supernumerary apophyses which the bones of some subjects present, and which are almost always multiple and often very numerous in the same individual, and are more or less similar in form to the natural apophyses. The exostosis, properly so called, is, on the contrary, almost constantly solitary; an anormal condition confined to one point of the osseous system. Once, however, I saw two very compact exostoses situated on the maxillary bones, one on each side the nose; and quite recently I have seen a young man, having an exostosis on the thumb of the right hand, and another on the index finger of the right. This was the first time I had seen true exostoses, and those voluminous ones, on the phalanges.

I was sometime since about to collect my various observations upon this affection together, when the following case presented itself to my notice. A man, æt. 28, consulted me for a hard nipple-shaped tumour, firmly attached, like an appendix or apophysis, to the lower part of the femur, at the uppermost border of the ham. Although seemingly the size of a child's fist, it barely raised the skin beneath which it was placed. By careful examination, and separation of the muscles among which it was placed, a narrower base or kind of thick pedicle attaching it to the femur could be distinguished. The growth had commenced some years before, and after continuing somewhat rapidly it remained stationary for about eighteen months or two years. Quite recently, a collection of fluid had formed between the culminant portion of the tumour and the external soft parts, and this latterly has given the tumour the appearance of increasing. I did not hesitate to regard this fluid as a little synovial sac, a sort of hygroma, just like that which so frequently forms in the bursæ of the patella, olecranon, &c. under the influence of contusions or continued pressure; and my opinion was, strengthened by the prompt disappearance of the fluid upon the application of a strong solution of muriate of ammonia. As the exostosis itself had been for some time stationary, caused no deformity and little inconvenience, notwithstanding that the vessels and nerve must have undergone some change of position, any operation for its removal was out of the question.

This made the seventh time that I had observed a pediculated exostosis of this kind, seated at the lower third of the femur, and constituting an isolated affection. In most of the cases the tumour was seated anteriorly, in the immediate vicinity of the capsule. Whence comes this disposition of the lower part of the femur to be especially affected? I know not: but if I may judge from three or four cases I have met with in the humerus it is the *upper* part that is most constantly affected. According to my experience, too, the different parts of the skeleton vary much as to their degree of liability to exostosis. The femur seems so far beyond any other bone: then comes the humerus, the maxillæ, and then the phalanges, especially the last phalanx of the great toe, raising the nail, distending its matrix, and inducing a more or less acute pain. Dupuytren met with these cases of exostosis of the toe several times, and his directions for their removal are excellent. The nail is to be removed after longitudinally dividing it, and the tumour, which is rarely larger than a large pea or the point of the little finger, cut away with a saw, or better still, on account of its spongy texture, with a pair of cutting forceps.

Whatever place they occupy, or upon whatever place they become developed, these tumours have certain general characteristics which it is useful to be aware of. 1. A very remarkable one is that, although they may be found in adults, these persons were first affected by them at an earlier period of life. It is for the

most part individuals from 12 to 20 years of age that consult us concerning them. 2. They do not possess an unlimited power of increase. Arrived at a certain size they cease to grow, and I would risk stating that their growth ceases with that of the bones. 3. In comparing them with each other after they have acquired all the development they are susceptible of, their size seems in relation to that of the bone affected. They are generally larger in the femur than the humerus, greater still upon the bones of the pelvis, while they continue for a very long period small at the last phalanx of the great toe. So, too, with reference to certain tumours of the soft parts, such as lipoma and encysted tumours of various kinds, their power of growth seems very much dependent upon the size of the region of the body they occupy. 4. These tumours are not united to the bone by a pedicle properly so called, but only a certain degree of narrowing of their base. The principal part of the tumour is usually irregularly spherical, having an irregular and semi-cartilaginous surface. The central part of its substance may be somewhat spongy, but the tissue becomes much more compact as we approach the narrower base. 5. The connexion of these tumours with the parts that surround and cover them is various. When the exostosis is covered in part by a muscular layer, it is sometimes separated from it by a slightly condensed cellular tissue which yet allows the muscles to slide over the exostosis; while, at others, a small sac is formed containing fluid. In some cases, again, the exostosis and the muscles become firmly joined together, so as to render all motion of the latter over the former impossible; and, if an operation were undertaken upon parts so situated, the pedicle being divided without the tumour having been previously exposed, it might happen that this could not be removed without removing a portion of the substance of the muscle.

Considered as changes of organs, these osseous tumours assuredly form the simplest of diseases. They give rise to no inherent phenomenon or symptom; they have no particular tendency to degenerate; each is, as it were, as a little bone attached to the larger, and at most only susceptible of undergoing the same changes as this last. But as the exostosis approaches the surface it produces more or less deformity, and where it compresses or distends the soft parts it may give rise to more or less suffering, while situated within a cavity (*e. g.* the orbit or pelvis) it may induce most important functional disturbance or obstruction. In the limbs, the action of the muscles may be rendered painful or imperfect by them. One thing has always struck me in respect to these tumours seated beneath the muscles, namely, that the inconvenience and pain they occasion diminish, doubtless from the effect of habit, in proportion to the length of time the exostosis has existed. It is a good reason for exhorting our patients to have patience, and not yielding too readily to their desire for undergoing an operation."

M. Roux furnishes at some length the particulars of the various cases calling for operations and the mode in which he performed these. These are too long for extract, and we can only subjoin his conclusions upon this point. "1. With the exception of some whose position renders them inaccessible, it is almost always possible to remove these tumours. 2. In almost all cases such ablation is indicated, either for the removal of a great deformity, the relief of continual suffering, or the re-establishment of the deranged functions of certain organs. 3. In the great majority of cases we can proceed to the ablation without a preliminary exposure of the tumour, and without having any great difficulties to overcome. 4. Such an operation may lead to unfortunate results, in consequence of some peculiarity in the seat or relations of the tumour, or from the bad constitution of the patient; but generally it is crowned with success."—*Revue Medico-Chirurgicale*, Nos. 2 and 3.

[The above extract from the pages of our new contemporary and namesake is taken from a paper furnished by M. Roux from among the materials he is now engaged in arranging for his long-expected work on "Clinical Surgery." His

well-known candour in stating the results of his experience, whether favourable or unfavourable; and the long period during which he has been accumulating facts furnished him by an extensive private and public practice, naturally give rise to the highest expectations.—*Rev.*]

On Iodide of Potassium in Syphilis. By Dr. Aran.
(Archives, Vol. 13, pp. 77-161.)

In this paper Dr. Aran enters into a critical examination of the most remarkable writings which have recently appeared upon this subject, as those of Dr. Moij-Sisovics, of Vienna, Dr. Hassing of Copenhagen, Drs. Gauthier, Payan and Bassereau. (*Med. Chir. Rev.* No IX. p. 267). His appreciation of these seems to us to have been very conscientiously and carefully made, and we really hope that it will be for some time to come considered as final; for, important as the subject is, it seems to us to have been at least sufficiently insisted upon of late. The following are Dr. Aran's conclusions.

"1. Frequently repeated experience has indubitably established the fact, that iodine and its preparations taken internally, possess valuable anti-syphilitic properties. 2. The iodide of potassium is far preferable to the other preparations, and deserves a preference to them in almost every case. It is easily given, does not nauseate, frequently increases the appetite, accelerates nutrition, and is endowed with valuable curative properties. 3. It is not indiscriminately applicable to all the periods of syphilis, or the different symptoms which characterize it. As a general rule we may say it especially succeeds against what Wallace has termed the *pustular or deep-seated form of chancre:* it is of remarkable efficacy, and nearly always heroic, against tertiary symptoms, and those of the secondary ones which are tending to tertiary. 4. It is precisely in those cases in which mercury shows itself powerless, or of only feeble efficacy, or when the symptoms are of very long standing, that the substitution of the iodide for mercury best succeeds, and that especially when these symptoms occur in a more or less shattered constitution. In this way the iodide fills up a lacuna which was heretofore painfully evident in the therapeutics of syphilitic diseases. 5. The utility of the union of the mercurial and iodic treatment is at present an undecided question. At all events, this mixed medication seems to be without inconveniences. 6. The iodide is equally efficacious in whatever vehicle it be administered. Experience has shown that from 10 to 15 grains per diem suffices at the commencement, and that this may be gradually increased to from two to four scruples. 7. The occasional inconveniences which this medicine gives rise to (such as cutaneous irritation and inflammation of the conjunctival, nasal, or buccal mucous membranes), must not prevent our employing it, especially as they may be in part prevented, or moderated, by careful administration."

We may here add the account M. Aran furnishes of the treatment pursued for several years by M. *Moij-Sisovics*, senior physician to the Vienna Imperial Hospital, and described by him in his work upon "A rapid and certain Mode of treating Syphilis by preparations of Iodine;" and which, if the author's statements can be relied upon, certainly surpasses all others in prompt efficacy. "Half-a-drachm of iodide of potassium, dissolved in water, and divided into three doses, is given the patient daily, the quantity being gradually increased until four scruples are reached, beyond which it is never extended. Baths of iodine and marine salt are simultaneously employed, (salt 2 lbs., iodide of potassium 6 scruples, pure iodine, added when the patient is in the bath, 4 scruples) for an hour at a time, the patient lying in a hot bed afterwards until transpiration is excited. These baths are continued for three days, until some irritation of the skin is induced, the quantity of iodine being then augmented. On the 10th or 11th day a febrile state comes on, itching of the skin is felt, and a scarlatinoid eruption,

or one resembling zoster, is produced. On the 15th, 17th, or 21st day, a general desquamation occurs, which, with the previous eruption, indicates the saturation of iodine has reached its maximum, and the author declares he has never seen a relapse in a case in which the eruption and desquamation have pursued the regular course. For the exostoses, condylomata, and pustules, Dr. M. employs a strong local application formed of iodine, the iodide and water. He orders alimentary substances of easy digestion, taking care to avoid those of an amylaceous nature, and keeps the patients warmly clad in a room having a mild temperature. This treatment he employs against every description of syphilitic disease, primary or secondary; and he declares it is followed by a cure in from fifteen to twenty-two days."

ON THE USE OF ASSAFŒTIDA, AS A MEANS OF PREVENTING THE DEATH OF THE FŒTUS IN UTERO, DURING THE PREGNANCY OF DELICATE WOMEN. By Dr. LAFERLA of Malta.

The author, from an early period of his professional career, has directed his attention to those cases in which the fœtus having reached its period of development dies prior to birth, the mother in this way sometimes bringing forth a succession of dead infants. In reflecting upon the subject, he felt disposed to attribute the occurrence to debility or inertia of the uterus, and in searching for means to invigorate the condition of this organ without inducing its contractions, he remembered the commendations bestowed by Sydenham upon *assafœtida* in hysteria, and especially in cases of debility of the womb. His employment of this in several cases has led to excellent results, which it is the object of the present paper to detail.

The fœtus may die at three different periods of pregnancy—before the sixth month, during the two first months of the third period, and when arrived at full term, independently of the nature of the labor. When a pregnancy is so menaced there are several symptoms which should excite attention. At first there is a feeling of languor, general uneasiness, restlessness, horripilation, and a sense of great cold. The countenance becomes pale, the head heavy and confused, and the tongue loaded, vomiting or nausea even sometimes occurring. The spirits are low and the nervous system very excitable. The movements of the fœtus are languid, and at last only its cardiac pulsation can be perceived. There is numbness of one or both legs, and severe pains of the loins, belly, and pubis come on. The child feels a heavy weight in the pelvis, and a few days prior to its expulsion, a whitish-yellow or sanguinolent discharge takes place from the vagina. The *causes* of this state of things are usually depression, passions or frights, venereal diseases, a lax constitution, amenorrhœa, leucorrhœa, menorrhagia, hysteria, or a neglected miscarriage.

In apportioning the medicine the temperament of the woman and the period of her pregnancy must be taken into account. "To those of sanguineous or bilious temperament, from two to four grains per diem less should be given than to others; and I always take care that a pregnant woman, before arriving at the period at which she has already lost a child, shall have taken from 10 to 15 scruples of the medicine in all. At first I employed the tincture, but finding the pills more easily taken, I have since combined the powder of assafœtida with extract of chamomile, so as to make 2-grain pills. Of these I give one fasting, and another in the evening, five hours after dinner. This dose may be gradually increased as we approach the critical epoch: so that, if in prior pregnancies the child died during the first period, this should be done every second day; if on the second period, every four or five days; and if at term, every six or eight days. We have best chance of succeeding when we commence the treatment

even before the pregnancy has begun. I then prescribe from 6 to 8 grains per diem, divided into two doses, until certain signs of pregnancy manifest themselves. After these are present, I then give but the simple 2-gr. pill night and morning, and if the movements of the fœtus are of normal strength, and the pregnancy does not offer the same deviations as the former, I continue the same dose to within about a month of the period at which death usually takes place. The continuous use of assafœtida gives rise sometimes to a burning sensation in the stomach. In this case I suspend it for some days, substituting some decoction of gentian until the heat is dissipated. Great attention to moral agents is requisite, and the strength must be well maintained by a succulent diet."

Dr. Laferla has met with about twenty cases in which he has tried this medication with success, and seven of them are here detailed. In some of these, three, four, five, and even eight unfortunate pregnancies occurred prior to the successful use of this drug.—*Revue Medico-Chirurgicale*, No. 3, pp. 129-137.

[M. Laferla has directed attention to a very importrnt point, and one too much neglected. We cannot agree with him in his opinion that a special corroborative power is exerted by the assafœtida upon the uterus, but believe that it may frequently be useful in counteracting that nervous susceptibility which women prone to this accident so commonly manifest. In another and a large class of cases the loss of successive children is referable to a syphilitic taint existing in either parent, which is removable by a mercurial course.—*Rev.*]

Clinical Observations upon the Treatment of Squamous Diseases of the Skin at the Hôpital St. Louis. By M. Devergie.

Arsenic.—Although the various squamous affections of the skin present sufficiently distinct characters to require that they should be nosologically distinguished from each other, yet, in a therapeutical point of view, they may be considered in a more general manner. Arsenic may be given in the form of *pills* or of *solution*. The Pills, known under the name of *Asiatic*, are thus composed: *Arsenious Acid* 1 gr., *Black Pepper* 12 gr., *Gum* 2 gr., *Water* q. s. to form 12 pills. One pill, or at most two pills, should be given daily for 6 or 8 weeks; but this mode of administration is so uncertain, and so frequently induces irritation of the digestive organs, that M. Devergie now never has recourse to it. Among the *Solutions*, that of *Fowler* (*Liq. Potassæ Arsenitis, Phar. Lond.*) furnishing a very soluble arsenite of potash, is best known. When I first came to St. Louis it was customary to give only one drop of this every 24 hours, for a certain number of days, increasing this at very long intervals to 2, 3, 4, and even to 12 or 16 per diem. I found, however, that by giving 1 drop the first day, 2 the second, and thus increasing by a drop daily, until the maximum (12 to 16) was attained, the duration of the treatment might be much abridged without any injury to the health. We have accounts of as much as 40 drops of this substance being given, but this must be erroneous, and a careful investigation has convinced me that we cannot give more than 20 or 22 per diem, without producing injurious effects, and that 14 form the ordinary limit. This substance exerts its action both on the skin and upon the general economy.

Action on the Skin.—After a certain time the scales fall off, and although the ointments which are used detach these, it is the arsenic which prevents their reproduction. The skin gradually assumes its proper level, and the affected parts, from being red, become brown and smooth. This is of great importance, for it is only when we have obtained this *brown color of the skin* we can be assured of the cure. It is the proof of the efficacy of the medicine and the measure of

the quantity required. So certainly is this brown colour of the spots a sign of cure, that if there is a relapse of the disease from a recurrence of its causes at the end of a year, or even of three months, it never re-appears upon the portions of the skin originally affected, but always on one side of them. The disease is cured when these spots are seen; but they require some months or even a year to disappear.

Action upon the Economy.—The first effect of arsenic is a general falling away of the whole body, and a brown leaden coloration of the face: from which it results that patients who have completed the course of treatment, and feel themselves very well in all respects, have just the appearance of convalescents from serious disease. With no pain or other disorder of the economy, and possessed of a good appetite, they continue so thin as still to seem very ill. Arsenic would seem to *exert a modifying power upon the formation and secretion of fat*; and on this account I have prescribed it for the resolution of adipose tumors, and several times with success.

Arsenic sometimes gives rise to *certain accidents* which it is important to bear in mind. All persons cannot bear the same dose. In some, eight drops will produce anorexia and other disorders of the system, and in such cases we must suspend its use for some days, and only resume it very cautiously. It sometimes happens that when we have again reached six drops all the ill-effects are reproduced, and in this case we must definitively renounce it, for not only may it do much mischief, but it will not cure the disease against which it is employed. There are other subjects in whom 12 drops produce accidents which show that the drug has acted sufficiently, and must be left off. These are symptoms which have not been hitherto indicated by authors. Some patients will tell you they suffer from dyspnœa, others from partial loss of strength, as in a limb, others from colic or diarrhœa, from numbness of a limb—in a word, from one of a varied series of more or less strange nervous phenomena—none of these being symptoms characteristic of poisoning by arsenic. Of all these, dyspnœa is the most constant. Symptoms of true poisoning may, however, occur, these arising when too large a dose has been given. The presence of this medicine in the urine would prove that the system is saturated with it, and that its therapeutical action had reached its limits.

The arsenic is best given in two doses per diem, in a julep or sugared water. M. Devergie employs a far weaker solution of arsenic than Fowler's, so that 22 drops of it represent 1 drop of the latter. He does this because the quantity can thus be more easily measured out; and accidents are less liable to occur from a mistake in the number of drops. *Pearson's Solution*, which contains an arsenite of soda, is less easily supported by patients than the above.

Antimonials.—These may be also administered in two manners. In a pill, (*Plummer's Pill*), each pill containing one grain of calomel and of the sulphuret. Of these we may give from two to six daily; but, as with respect to arsenic, the pilular form is a bad and uncertain one, on account of the insolubility of the materials; I therefore much prefer *tartar emetic*. I mix half-a-grain of this with from half-a-drachm to a drachm of *cream of tartar*, and give them mixed up in preserves, the patient swallowing a glass of sugar and water directly after. It is remarkable that, given in this way, these medicines produce no vomiting, and scarcely any purging. The toleration is indeed surprising, and it is as much as there is a little nausea the first few days. In those cases where vomiting or purging is caused, it must be left off. This treatment requires to be continued in general for two months, and is not evacuant but alterative in its operation.

As M. Devergie considers *external treatment* of these diseases as of comparatively little use, we need not detail the various means at any length. Of these he seems to think the application of *tar* (melted down with at first 40, and then

20, 30, 10, or 5, parts of lard, night and morning, as one of the best, when used in combination with baths, the patient always sleeping in the same shirt and bed-clothes. The ointment has to be employed for several weeks, and if it is to prove efficacious, a white line will be seen after awhile to form around the circumference of the spots, produced by the decoloration of this. In the varieties of psoriasis termed *guttata, gyrata, orbicularis*, &c., it is from the centre to the circumference that the whiteness and the cure proceed. *Alkaline* ointments are best formed of *carb. sodæ* (from 2 to 8 parts previously dissolved in water to 30 of lard,) and will sometimes cure a sporiasis which has resisted tar. Indeed, *lard* will occasionally do this, and should be generally employed for children either alone or feebly medicated. *White precipitate* (2 to 6 parts to 30 lard), is not unfrequently successful, but it may, if the surface is an extensive one, produce salivation, a result less likely to follow the use of *calomel ointment* mixed up with a few grains of camphor. *Vapour* or *alkaline baths* should be conjointly employed with the above means, the latter being formed with carbonate of soda, to which salt is added, if a tonic, or gelatine if an emollient effect is desired.

In choosing among the various modes detailed several circumstances have to be considered. 1. If the disease is *hereditary*, whatever treatment we adopt we shall rarely produce a complete result. 2. If the patient possesses a strong constitution, we must not fear employing perturbatory medicines, which would be improper for weakly habits. 3. We must learn the duration of the disease, and whether it is a relapse. If it has never yet been treated, we must use every endeavour to procure a radical cure; but if it has been already treated we must fear its re-appearance, and, for its temporary dissipation in this case the external treatment is especially indicated. 4. Whenever the lepra or psoriasis is of recent origin, especially if occuring in a robust patient, from the internal treatment (arsenical or antimonial) we may expect a radical cure, providing always that the patient afterwards takes as much care as possible of the skin. If the patient is weakly, sickly, or of a bad constitution, we must be content with the external treatment.

In young, delicate, and feeble subjects, the *hydrotherapæic method* has a corroborant effect. By its aid I have cured cases of very long standing. The patients in general, too, bear it well, and under its use, in spite of the sweating induced, they gain flesh, probably in consequence of the more vigorous appetite that is produced. In most of the cases I have so treated, however, relapses have occurred in the course of a few months; so that it seems to possess no more power of *radically* curing the disease than the other external measures.—*Gazette des Hôpitaux*, 1847, Nos. 1, 13, and 22.

Observations upon Itch. By M. Devergie.

M. Devergie believes that some of the generally-received opinions concerning this disease possess a very insufficient foundation, and after long and attentive observations in the wide field at his disposal, he has arrived at the following conclusions.

"1. There is a very general opinion prevailing in the world, that the itch is the cause of the skin diseases which may subsequently occur. Without attaching too much importance to this, does it not rest on more or less probable foundations? 2. Although itch is said to be essentially contagious, it may yet frequently arise spontaneously in individuals placed in similar conditions under which it was primarily developed. 3. There is no proof that it has been transmitted from animals to man. 4. When medicines cure the itch, it does not follow that this is by destroying the acarus. The cure of the pustules may be rather said to lead to its death. 5. This insect may just as well be considered

as a morbid product as a cause. 6. The experiments undertaken to show that the contagion is operated by means of the insect, offer insufficient proof of this being the sole means of infection. 7. In the hypothesis of contagion being due solely to the acarus, as the contact of individuals with each other does not always take place in the same manner, we must suppose that the acarus for a certain period crawls over a large portion of the surface of the body to the place of election (the hands and feet), to the neglect of many other portions, which a few days after will be covered with itch-pimples. This regular and simultaneous development is much more likely to be dependent upon a general cause acting upon the entire economy, than upon a local one, arising from an insect transplanted from one individual to another. 8. If the acarus is the cause of the itch-pimple, it seems difficult to understand how it escapes from the central point of the pustule while digging a deeper gallery beyond it, scarcely ever having any communication with another pustule. This fact, which may be daily observed by the naked eye, is much more in harmony with the hypothesis of a morbid generation. 10. There is nothing fixed in the incubation of itch, which is much more in accordance with what takes place in other cutaneous diseases, than with the idea of infection by means of an insect. 11. It is in the severest form of itch, the pustular, that we find the fewest acari. This form is cured most easily, and seems the least contagious. 12. It is singular to find the same insect producing three different forms of eruption; and not only are these forms different, but so are their contagious properties and the number of insects found in them. 13. Statistics show us that itch is the most common antecedent of impetigo, lichen, and eczema. 14. The itch may disappear for a greater or shorter period under the influence of a general affection of the economy, and, like the other cutaneous diseases, remain latent, and then re-appear with renewed energy after five or six weeks, without any new infection having taken place."

From these corollaries M. Devergie draws the following conclusions:—

"1. If the acarus is one of the phenomena of itch, its existence as a morbid product is as admissible as an agent of transmission. The known facts agree much better with the former hypothesis than with that which considers the insect as the exclusive agent of transmission and only cause of contagion. If even the acarus, by being transported to another individual, may communicate the disease, the products of secretion, the itchy atmosphere, and the clothes impregnated with this may also produce it. 2. The principal therapeutical consequence deducible is, that we must treat the itch, and not the acarus, contrary to what is generally the practice. We should treat the disease like other cutaneous affections upon general principles and not by mere local applications. Indeed, is it not reasonable to take into consideration the two well-marked symptoms, whose sudden suppression is so mischievous in other cutaneous diseases, viz. the *itching*, become habitual in proportion to the duration of the disease, and the *secretion* in the pustular form? In the place of searching for means to cure the itch in the shortest possible time, should we not endeavour to do so gradually in proportion to its duration? Should we not, after its cure too, prescribe in some subjects the prolonged use of simple baths, to reduce the morbid sensibility of the skin and re-establish its functions, or even vapour-baths to produce a sedative effect on the nervous system, as well as a depurative cutaneous secretion, if I may be allowed such an expression? So, in regard to pustular itch, should we not act revulsively upon the intestinal canal by means of purgatives, to compensate for the suppression of secretion over so multiple a surface? This is my practice, and I do not feel disposed to abandon it, because it seems a rational means for the prevention of the ulterior development of other cutaneous diseases, of which I believe itch to be one of the predisposing causes."—*Gazette des Hôpitaux*, No. 32.

[M. Devergie seems to us, in the above observations, to have raised a very

important point for consideration. That the doctrine of the exclusive propagation of the itch by the acarus is baseless, we have long been convinced; but we must confess that we have not hitherto viewed the rapid suppression of the disease in any other light than a very desirable circumstance, seeing the great amount of bodily and mental irritation its persistence keeps up. The employment of baths and purgatives, however, after the cure, as recommended by M. Devergie, is a most rational procedure, and one far too much neglected.—*Rev.*]

A Sketch of the Life and Works of James Tommasini,* read before the Royal Institute of Venice. By Professor Giacomini.

It is very remarkable, I may say almost marvellous, that among so many justly celebrated men in Italy, so many *savants* desirous of renown; amidst such a diversity of principles as prevails in our peninsula, it is agreed on all sides, without even excepting his adversaries, that Tommasini may be considered as the foremost, the most experienced, and the most profound clinical observer of our epoch in Italy. The true reason of this general fact is found in the immense services which this great man has been rendering for more than half a century to his art and to society, whether as a professor, a writer, or a practitioner. To him in fact we owe the happy direction which medicine has received throughout Italy from the commencement of this century, the greater part of our best clinical observers having been formed in his school. Elected at an early age to the professorship of physiology and pathology in the University of Parma, he exhibited great intellectual power and practical tact in the first work he published (*Critical Lectures in Physiology and Pathology*, 1802.) Even now we admire in this work deep sagacity, healthy criticism, practical deductions of great importance, and an erudition alike rich and in good taste.

A short time after a wider field of observation presented itself to the investigating spirit of Tommasini. We allude to his clinical labours. This epoch was full of events of the highest import to Italy. Natural philosophy, restored by the labours of Bacon and Galileo, had produced so much embarrassment among the prevailing medical doctrines, that physicians gifted with more than ordinary intelligence, acquired a melancholy skepticism respecting practice. And thus no sooner had Brown's seductive doctrine appeared above the horizon than it was welcomed with avidity, and in a short time medicine in every country was found to be modelled on the views of the Scotch philosopher. Rasori was the first who made known the principles of the new school in Italy, by translating Brown's "*Compendium*:" and he it was who first, towards the end of last century, clinically demonstrated its errors. The Scotch physician had laid it down as a principle that all agents applied to the living tissues exerted upon them a stimulant effect, and that diseases emanated from defective vitality, as was made apparent from the loss of strength which was observed in all diseases. Rasori exposed the errors of this doctrine by proving, as Hippocrates had already established, that the greater number of diseases which lead to death induce in the organism a condition of excess of the vital powers; and demonstrated by "solemn" experiments at Genoa and Milan the law of morbid capacity and the existence of medicinal substances which act positively and directly by depressing the organic forces, and modifying the organism in a manner directly opposed to that of the action of stimuli, and which for this reason he has termed *contra-stimulants*. From these very simple principles, based upon experience, the medical

---

* Tommasini was born in Parma in 1765, and died there in 1846.

reform operated by the powerful genius of Rasori in Italy, took its origin. This great observer, however, carried away, after the discovery of these fundamental facts, by the whirlwind of political events, was compelled, for a long time, to leave his official post at the hospitals, and live in a kind of obscurity, after having gone through the severest trials during the course of those revolutionary times: so that the primary basis of the medical reform might have remained unimproved, and without a worthy interpreter, if Tommasini, who was the fellow-citizen and fellow-student and rival of Rasori, had not taken possession of them, to develop and confirm them by new experiments, and place them in the full light of day, with all the importance they deserved. Tommasini was in fact the first who exhibited in relief the immense practical consequences which flowed from the new facts discovered by Rasori, and he occupied himself by means of numerous observations upon a great number of medicinal substances and diseases, in proving their reality. He has in this way really enriched Italian medicine with a great number of novel facts, which have thrown the greatest light upon its practice, and led our physicians upon a way of investigation entirely different to that which they had hitherto followed; and this assemblage of facts has constituted so compact and immovable a phalanx, that he has been able, since its accumulation, to defy his adversaries with impunity, and to resist, with a brilliant success, all the attacks which the advocates of retrograde opinions have incessantly directed against him.

Consequently it is to Tommasini, beyond all others, that we owe the new reform in Italian medicine—a reform which has continued progressive from its commencement, and which is incessantly extending itself in the medicine of all countries in which practitioners take for their guide true experience and the precepts of natural philosophy. The entire proof of the truth of what I advance is to be found in the numerous works themselves of our illustrious countryman. It will suffice if I refer to those most directly bearing upon my subject; and it will be easy for me to prove, that the new facts with which he has enriched science, have influenced the practice of our art among all civilized nations, not excluding even that of his own adversaries, who have adopted them, masked under a different phraseology. We may first refer to his experiments and observations upon various remedies, especially digitalis, commenced in 1804. Any one reading these, and the long and varied discussions they gave rise to on the part of his celebrated adversaries, will find wherewith to become edified upon the subject of experimenting and the true logical method of drawing deductions from experiment. He will see that the experience which every one invokes with a sort of ostentation, I may say to a tiresome extent, in support of his own opinions, is itself, in the absence of philosophy and criticism, the first source of error. At the present day, when time has pronounced its verdict upon the famous question which cost Tommasini and his adversaries so much pains and labour, viz., *whether digitalis exerts upon the economy a stimulant or sedative effect*, we feel astonished that so evident a fact as the contra-stimulant power of this drug should have had so much difficulty in making its way. And yet the fact in relation to the action of digitalis, which is now generally recognized, is no less evident in respect to a crowd of other contra-stimulant remedies, concerning which observations, just as numerous and conclusive exist—observations which the obstinacy of some adversaries persists in neglecting, substituting with a desperate constancy preconceived opinions to the material facts which alone should be invoked in questions of this nature.

In the middle of the polemics which overturned Brownism in Italy, Tommasini published his monumental work, *Pathological Researches on the Fever of Livorno, the Yellow Fever, and other analogous Diseases*, 1805. In this were discussed and resolved the most important questions in relation to fever, and the true pathology of gastric and bilious fevers, and the doctrine of the diffusion of local inflammations established. He therein, too, signalized the true material condition of

continuous fever, and liberated for the first time the numerous family of fevers from the empire of the abstraction and the essential nature. He demonstrated that the phlogistic process was always the same in its forms and consequences, in its acute as in its chronic progress, and he laid down general precepts, which have become inviolable practical laws in Italy, for the exact appreciation of diseases in clinical practice—precepts which recall to mind those contained in the works of Hippocrates, of which Boerhaave said *Quo plus evolvo eo ditior in praxi fio!*

The various works published subsequently by Tommasini are but amplifications, based upon a much more extensive practical experience, of the principles expounded in his "Researches;" so that we must refer to the epoch of that work, and of that published upon the Genoa epidemic in 1799 by Rasori, the commencement of the great reform in Italian medicine—a reform really based upon Hippocratic principles, or in conformity with these principles and with those of experimental philosophy which the doctrines of Brown had caused to be completely forgotten in Europe. This work of Tommasini's went through a great number of editions in a short time, and was translated into several languages; and I must here advert to a fact of importance as regards the history of the medicine of our times. While this work was in the hands of all our practitioners, and justly appreciated by them, the celebrated Broussais was then with the French armies in Italy, and sojourned for a long period with us. Returning to France, this illustrious physician published, in 1808, his first work on the *History of the Chronic Phlegmasiæ*. This work is but the echo and copy of the doctrines of Tommasini concerning chronic inflammations and the nature of gastric fever and of fever in general; with this sole difference, that while the great Italian physician placed the seat of fever, according to the symptoms, in a phlogosis of this or that viscus, and frequently even in a diffuse inflammation of the entire sanguineous system, the celebrated Frenchman located it exclusively in the gastro-intestinal canal. Broussais could not be ignorant of the opinions of Tommasini upon this subject, nor have omitted the careful study of his works; and the just reclamations which were raised in Italy upon the appearance of his work, obliged him to offer an explanation in his "Annales de Medecine Physiologique." He acknowledged his acquaintance with the works of Tommasini while in Italy, and admitted that he had been preceded by him both in his researches and his conclusions. In the mean time French pathology (not so therapeutics, for unfortunately Broussais had not availed himself of the studies of the Italian school upon the medicinal substances, but retained all the old aberrations of Brown respecting these) received from the labours of Broussais a vigorous impulse. This physician created for himself an European reputation, and his sarcastic language in the end obliged his very adversaries to change their principles. And strange to say, Broussais, who had only borrowed his pathological principles from Tommasini, acquired partisans in Italy among the very persons who had rejected the conclusions of Tommasini!

In all the various works of Tommasini, whose titles even I cannot enumerate, there will be found pervading each page a judicious and practical spirit, founded upon the works of Hippocrates, full of sagacity and wisdom, enlightened by a prodigious erudition, and enlightened by that probity and love of truth so delightful to find in men of science. We can, therefore, in no-wise feel astonished that, as a practitioner, a writer, or a *savant*, he has always been considered, both in Italy and elsewhere, as the true patriach of Italian medicine, just as he was really its regenerator. In his clinical histories, published in so many of the Italian journals, he was most especially careful in the relation of unfortunate cases, accompanying them with considerations and warnings of great practical interest, and concluding with valuable remarks upon the causes which had led him or might lead into either error or doubt as to accurate diagnosis or treatment. It was in these cases that his enlightened circumspection and great prac-

tical sagacity were so conspicuous. I do not so term that impassable inertia, which, decorated with the name of prudence, allows dangerous diseases to proceed on to their fatal termination, and then washes its hands of them; so that the disease may bear all the blame and the practitioner be protected from all reproach; the vulgar being always disposed to blame as culpable the active intervention of the physician when this proves insufficient to save the patient's life! In these most serious and difficult cases, Tommasini employed an unheard of zeal in combating the disease with boldness and vigour until the very limits of art were reached, and he frequently succeeded in triumphing over it in spite of the desperate circumstances amidst which he was employed. Tommasini was always in the habit of preparing statistical tables of his clinical practice, and first taught the true manner of turning these to practical account, protesting against, as a vain and inconclusive practice, the simple accumulation of figures.—*Gazette des Hôpitaux*, No. 27.

[The fate of Brownism in Italy constitutes a curious feature in the medical history of our epoch. Rejected in this country and in France, it was welcomed in Germany, and especially in Italy, with the most flattering reception. Rasori constituted himself one of its earliest and most ardent interpreters; but his practical sagacity soon taught him that, however seductive from its simplicity the system of the Scotch theorist might prove; that so far from the vast majority of diseases being of an asthenic nature, proceeding from debility of tissue, and requiring stimulant remedies, the very contrary is far nearer the truth. Hence the re-action described by Professor Giacomoni in the above paper, which has since pervaded Italy, as it did France, under the *prestige* of Broussais. In both countries the other extreme was run into, and a far too indiscriminate bloodletting was the consequence: but while in France a more rational state of things now prevails, in Italy the antiphlogistics school seems still to exert undisputed sway. The charge of plagiarism brought against Broussais will be new to some of our readers, although it seems well-founded.—*Rev.*]

Practical Observations upon Foreign Bodies introduced into the Eye. By M. Petrequin, of Lyons.

M. Petrequin divides these bodies into three categories, those which are arrested on the surface of the eye, those which become imbedded in the cornea, and those which, perforating the cornea, penetrate more or less deeply into the chambers of the eye. In a manufacturing town like Lyons these accidents are very common.

1. *Foreign Bodies arrested upon the Surface of the Eye.*—These often become hidden under the upper eyelid, and are of difficult removal by the usual plan of raising the lid upon a probe, &c. M. Petrequin employs a camel's hair pencil, and introducing it beneath the eye-lid, he passes it from the one commissure to the other, so as to sweep any foreign body into the nearest angle of the eye. If he has not a pencil at hand he employs the feather of a pen. It acts best when used dry, the tears sufficing to moisten it. Extraction in children is difficult in consequence of the convulsive contraction of the eyelid, which often occurs, so that even opening these at all may be impossible. M. Petrequin throws in an injection of rose-water to dislodge the body from the corner of the eye where he supposes it placed, and brings it thus easily to the free edge of the eye-lid. Practitioners should always recollect that the sensations induced by a foreign body may continue after it has been removed, so that they may not needlessly make painful researches.

2. Workers in iron or stone may have particles fix themselves to a greater or less depth in the substance of the cornea, and these are sometimes so small as to require a lens to see them in the little depressions in which they are seated. If the particle is metallic, we should try the effect of a magnet for their removal. M. Petrequin considers a forceps a bad instrument, for, however delicate it may be, we may try 20 or 30 times without being able to seize the body with it, irritating the eye more and more, and increasing the difficulty of the extraction. The case is rare for the body to be large enough to have a free portion sufficient for seizure. The bistoury is preferable, but it should be a rounded one, for with a sharp-pointed one, we might risk piercing the eye during one of its spasmodic movements. M. Petrequin prefers a large lancet, the front and edge not being too sharp, and its form resembling that of an abscess lancet. The patient should be seated in a chair, the arms of which he grasps. The light coming from above, he is directed to throw back his head until its long diameter becomes very oblique from above downwards, and from behind forwards. The surgeon placed behind supports it against his chest, and directs the patient too look upwards. After having raised the upper eyelid he directs the cutting edge of the lancet very gently and carefully upon the foreign body, and slightly scrapes the cornea. The eye immediately becomes convulsed and is retracted beneath the lid. It is brought down again by looking at a fixed point, and the scraping again commenced. In this way the eye becomes accustomed to the contact of the instrument, and the foreign body is easily removed. It must always be shown to the patient, since, on account of the continuance of the pain for some hours after, he may doubt the fact of its extraction. After the operation, a plastic effusion takes place into the little cavity, which becomes filled up by its organization, no trace of cicatrix remaining if the operation has been adroitly performed.

3. These cases are always grave, as the contusion pruduced may give rise to traumatic amaurosis, and at all events to inflammation. Two or three cases of this accident are related, one of which we abridge. A countryman, æt. 50, was admitted into the Hôtel Dieu on account of a foreign body which had entered his eye while cutting stone. This occurred some time before his admission, although how long is not stated, and the patient had been submitted to means for the relief of the attendant inflammation with little success. The cornea was entirely healed and offered no trace of cicatrix, and through it the bit of stone, not larger than a millet seed, was observed to be lodged in the anterior chamber between the cornea and iris, at its superior external part. The conjunctiva was much injected, and the iris inflamed. It acted imperfectly, and some adhesions to the lens had occurred. Vision was defective, and attended with great pain in daylight. M. Petrequin made an incision four lines in length, into the upper and external fourth of the cornea. The aqueous humour flowed out, and the iris presented itself. He passed in a pair of Sichel's cataract forceps closed, and opening one blade above and the other below the body, he seized it with some difficulty on account of the little support the iris furnished it, this receding from the instrument. A small portion of the iris which was prolapsed was removed by the scissors. The patient was bled after the extraction, and he went on quite well, perfect vision having been recovered. A small hernia of the iris was removed by cauterization with the nitrate of silver.—*Annales d'Oculistique*, T. 17, pp. 14-20.

On the Dentition of Children, from the Clinical Lectures of M. Trousseau.

The most recent works upon Odontology contain some errors respecting this

process, which it is the duty of the clinical teacher to indicate. The *evolution of the milk teeth* is not usually accurately described as respects the order of sequence. M. Trousseau represents it as occurring in groups, separated by pauses. 1. The first group is formed of the two *lower median incisors*, which appear nearly simultaneously, after which there is a pause for one or two months. 2. The two *upper median incisors* pierce the gum from the 10th to the 12th month. 3. The third group follows the second in a fortnight or a month, so as often to be confounded with it, and consists of the two *upper lateral incisors* (not the lower ones as usually stated.) 4. Again is there a pause, and we then find the *lower lateral incisors*, and the *four first molars* appearing almost simultaneously; first a molar generally showing itself, then an incisor, a molar again, and then the other incisor, and lastly the two molars. 5. After a pause of from two to five months the four *canines* appear; the 6th group, consisting of the *second molars*, scarcely ever appearing until five or six months still later. This is not a matter of mere curiosity; for the occurrence of these pauses should be borne in mind, *e. g.* in weaning. For this we should choose the intervals between them, and above all avoid attempting it just at the period of the evolution or the 4th group. The two first teeth generally appear from the 7th to the 9th month; the upper median incisors from the 10th to the 11th; the upper lateral ones at the end of the 1st year. About the 16th or 18th month the child should have 12 teeth; 16 from the 20th to the 24th month, and upon the appearance of the second molars, generally about the middle of the third year, twenty.

In the course of the 6th year, and even sometimes not till the commencement of the 7th, but rarely before the 6th, we see a large quadricuspid persistent molar appear; so that at seven years the child usually has 24 teeth; and it is at this time the milk teeth begin to be shed, generally in the order of their evolution. Thus the lower median incisors fall at 7 years; the upper ones at 8; the first molars in the course of the 10th year; the second molars towards the 11th; and the canine between the 11th and 12th—all these teeth being replaced by the corresponding persistent ones. At 12 we see the second large molars appear, and the child has then 28 teeth; and towards the age of 20, 25, or even later, the wisdom teeth appear and the number found in man (32) becomes complete.

It is commonly said that the first persistent molars appear at the fourth year; but this is an assertion contradicted by daily observations; as neither their eruption or the shedding of the milk teeth scarcely ever commences prior to the 6th year.

The teeth whose eruption is least frequently attended by derangements of any kind are, beyond all others, the incisors, which is generally attributed to their cutting form; and the most difficult of all to cut, notwithstanding their conical form, are the canine. They are deeply seated, and, as it were, wedged in between the lateral incisors and first small molars, which they are obliged to push on one side. The eruption of the molars is rarely attended with much inconvenience. We should always discourage weaning during the period of the eruption of the canines.

Four or five days prior to the eruption of a tooth there is usually a slight catarrh, cough, some dysphagia. Frequently its cutting is accompanied by diarrhœa, loss of appetite, and great pain. The gum is usually inflamed over the tooth; and a little later this inflammation is also seen around the teeth already cut. Some children also do not cut any teeth without paroxysms of convulsion; and in some cases these form the first indications of what is taking place. It is a general practice to attempt relieving the inflammation of the gums by lancing them; but in 24 hours the incision heals, and the subsequent cutting of the teeth becomes more difficult and painful in consequence of the cicatrization. The tooth in fact, often appears far nearer the surface than it really is, and very erroneous views prevail respecting its mode of arriving there. They come forward so slowly that they have time to modify the tissues. They do not distend the gin-

gival membrane, but inflame it. Frequently at the summit of a tooth which is not yet cut, we may see a little projection, which disappears after a while. It does not depend upon the pressure of the tooth, but upon an inflammation which renders the gingival tissue more permeable, and its absorption more easy. If this slight tumefaction was the result of pressure, the tooth would make considerable progress in a short time; while, after the gum has subsided, it is hardly apparent. If convulsions are present, we should employ immersion in cold water, antispasmodics, narcotics, or laxatives, according to circumstances. Diarrhœa is a more serious symptom, acquiring great obstinacy during dentition; and is especially to be feared, if weaning has been performed just at the period of the eruption of one of the groups.

In young children the teeth become carious more easily than in adults, and the caries sometimes give rise to most severe pain, and to the detachment of the dental follicles, rendering the caries of the persistent teeth also more easy of occurrence. Should we extract such teeth? If we extract milk teeth in a child of two years old, the persistent ones appear sooner, but the alveoli contracting, the arch of the jaw becomes diminished in extent. The persistent teeth have then little room, and lap over each other. If we want the second set to appear regularly, therefore, we must not draw those of the first—at least only in cases in which the caries give rise to very serious symptoms.—*L'Union Medicale*, No. 33.

[Although accustomed to attach much weight to the opinions of so sensible an observer in the enjoyment of a large field of practice as M. Trousseau, we should feel very indisposed to abandon the practice of free lancing the gums during any of the disorders of dentition.—*Rev.*]

Albuminuria in Pregnant Women.

M. Devilliers recently communicated to the Society of Medicine of Paris, the results of the investigations which M. Regnault and himself have been engaged in making concerning the Albuminuria of Pregnancy. In this condition the affection presents much less distinct and well-defined symptoms, most of which may be readily confounded with those of other diseases of pregnancy. Sometimes no morbid phenomenon manifests itself; and at others, there may be heaviness, headache, general uneasiness, some characteristicless derangements of the digestive organs; but no febrile action belonging to this disease in particular. Lumbar pains are so common in pregnancy, that we are unable to distinguish them either by their seat or nature, from those which are developed during the acute stage of albuminuria, As to the dropsy, sometimes it does not exist, which however is rarely the case; sometimes it is limited to the lower extremities, resembling the simple œdema of pregnant women; and sometimes it is general, and may be mistaken for anasarca dependent on affection of the heart, which, during gestation, is frequently increased to a great extent. There remains then the albuminous condition of the urine; and, notwithstanding what various writers have stated, these inquirers have never been enabled to detect albumen in the urine of the pregnant women who have been presented to their notice under ordinary circumstances. In examples of the disease, the quantity of albumen has been found very variable, increasing sensibly during notable disturbance of the circulation. *e. g.* during the development of intercurrent febrile affections, the period of accouchment, puerperal diseases, or the approach of death.

The affection which especially presents itself as a frequent and important complication of albuminuria, is eclampsia. Most of the subjects of puerperal convulsions exhibit evident marks of serous infiltration. In some, however, this

symptom is absent; but the authors of this memoir have *constantly* found albumen in their unrine. Of course all the women exhibiting albumen, do not become the subjects of convulsions; and the authors have found these absent eight times in twenty cases; but they believe that more than a mere coincidence exists between the two diseases, founding their opinion upon the fact, that in ordinary albuminuria various cerebral and nervous phenomena are exhibited, and that during pregnancy these last acquire a special development.

Contrary to what is observed in ordinary cases, the albuminuria of pregnancy may terminate in a rapid and complete cure after confinement, however grave the case may have appeared. It may pass into the chronic state, but this is more rare. It predisposes to the same affection in future pregnancies; it may induce abortion, seems to favour the development of puerperal diseases, and at the very least must be considered as a powerful predisposing cause of eclampsia; but of itself, and alone, it does not seem capable of inducing a fatal termination. The prognosis for mother and child is therefore only grave in proportion to the amount of complications. The authors observed 11 deaths in 20 albuminuric women.

Besides the various cadaveric lesions derived from the complications, the authors almost always found renal lesions of various kinds, and not exclusively such as are described as characteristic of Bright's disease; and they feel much disposed to accord to these organic lesions much less importance than is ordinarily attributed to them, and to regard them rather as the effects than the causes of the disease. Indeed, in pregnant women its causes seem to be different to those generally admitted in ordinary albuminuria.

The researches of the authors have proved to them that the blood of women during gestation exhibits a remarkable diminution of albumen, especially in the latter months; a condition which Andral has shown to be favourable to the production of dropsies in general, and one which bears the relation of cause to effect in respect to the present disease. The albumen dissolved in the blood is, in the normal state, combined in certain proportions, with a certain number of saline materials, which allow the blood to traverse the canalicules of the breast, to be there submitted to depuration, without the transudation of a single trace of albumen; but if from any general cause, from a disturbed condition of the assimilatory powers, the static equilibrium is destroyed, the renal parenchyma may become traversed by abnormal elements, and albuminuria induced.—*Revue Medicale*, Vol. 1., 1847, p. 449–52.

Hygienic Rules to be observed in Myopia. By Dr. Sichel.

Congenital short-sightedness exists seldomer in an advanced degree than in a condition of simple predisposition or slight commencement; and experience proves that it generally diminishes with the progress of age, if nothing be done to increase it, and if, so far from opposing the physiological actions of the organ, we favour their evolution. To this end the power of accommodation of the eye should be continually exercised, by removing the objects upon which it is habitually employed to as great a distance as possible. Especial care should be taken not to fix the sight for too long a time upon too small objects, or to employ it in reading too small letters, whether printed or written. These negative conditions are not sufficient. The sight must also be much exercised on large distant objects, the use of spectacles must be delayed as long as possible, or those of the weakest powers chosen, so as to give merely a greater clearness to objects without altering their size. They should also, even when the myopia is much advanced, be employed exclusively for seeing at a distance, laying them aside for reading, writing, or working. They are not required in the house or in

any locality sufficiently known to pass along without difficulty. If the nature of the occupation or the degree of the myopia compels their use, their power should be much lower than that of those which are employed for seeing at a distance. Frequently we are obliged to allow concave glasses to young persons for the purpose of deciphering music or of following the demonstrations which are made upon a black ground in the schools and colleges; but it is very important that the number of the glasses should only be such as just to allow of vision at the desired distance, and their employment should be limited to this. Far better is it, indeed, to do without them, and endeavour by frequent exercise to gradually increase the range of vision. We must not attach too much faith to the alleged impossibility of seeing without glasses; for abundant resolution and perseverance will frequently overcome every obstacle. If, during writing, the chest becomes fatigued by the bent position the state of vision obliges the young person to assume, a movable desk may be employed, which may be raised or lowered as desired, and by increasing the visual distance a very slight determinate extent every week or fortnight, we can gradually increase the range of vision. The same contrivance may be employed for holding the music at the piano, and the injurious bending forwards thus prevented. Such means are applied with difficulty to large commercial books, and to the open books employed while learning the violin; and in these occupations, if the myopia is considerable, glasses are required. If the individuals are very young, we should postpone the period of their so employing themselves, and in the mean time exercise their eyes in such a manner as may tend to the diminution of the myopia. When this is impossible, let the glasses be as feeble as possible, and accustom the wearer at intervals to lay them aside and cast the eyes on distant objects, which will at the least have the effect of preventing the sight getting worse. These short-sighted persons must very seldom look through dioptric instruments, such as magnifying glasses, microscopes, &c.

"I may cite my own case. I was myopic from birth to a considerable degree; but I have, by my frequent exercise of the eyes, and by my care to allow myself the use of spectacles as seldom as possible, which in my youth exposed me to much privation and frequent ridicule, been enabled to render my sight twice as good as it was; for, in fact, I now read at a distance about as great again as I did twenty-five years ago."

The myop then should exercise his faculty of exercising the eye at different distances as much as possible, or he will lose the power of doing so, and find his affection gain upon him more and more. It is especially necessary to prevent young children approaching objects too near their eyes, and not to give them too small toys, which will oblige them to do so. Not only will this cause an increased myopia, but it may produce strabismus. The visual axes naturally converge when we fix our eyes upon near objects; and the more this act is repeated, the more habitual does the convergence become, until it ends in becoming permanent and passing into strabismus. It is on this account that this deviation of the visual axes is so easily produced between the ages of four and six, the period at which children are generally first taught their letters; and it especially occurs when there is an inequality in the focus of the two eyes.—*Annales d'Oculistique*, Tom. XVII., p. 200.

Practical Remarks on Bleeding, and upon its Utility in the Treatment of the Diseases of the Uterus. By M. Tanchou.

On Bleeding in General.—It is not always easy to decide upon the institution or the repetition of bloodletting, and I have long been in the habit of consulting the condition of the blood itself for clearing up any doubts. Generally it is *black*, and that colour, in conjunction with its consistence, justifies its abstraction. Of-

tentimes, however, it is red at the commencement of the bleeding, and becomes black at the end of this. Again, it is at first black, and then becomes red; in a fourth case, the two threads of the spiral it forms may be of the two different colours, and lastly, the blood may be red from the beginning to the end of the bleeding.

Whence is this last due? There can be no doubt that when the individual is weak, and exhausted in some degree by preceding bleedings, it arises from the rapid and freer passage of arterial blood into the venous system; but when the patient is strong and young, the circulatory system full, and it is observed at the first bleeding, I believe it arises from the contact of the air, which, traversing the skin, acts upon the blood; for I have made this remark especially in hot weather, when the patients have been several hours in bed, upon young subjects affected with inflammatory fever, and young healthy pregnant women—whenever, in fact, the skin is supple, elastic, and perspirable, conditions favourable to the absorption of air; or in individuals suffering from exhaustion from any cause, and in whom the organism always consequently manifests an avidity for whatever may maintain life and recruit strength. I may remind you that the blood of infants is generally red, that of old persons black; and that the skin, under these two circumstances, is in quite opposite perspirable states.

However this may be, I have been led to the following practical inductions. If the blood is black during venesection we may allow it to flow, or repeat the abstraction if the symptoms indicate this; while, if it is red, we must absolutely abstain from doing so, or we shall only aggravate the symptoms for which we employ it, and increase the fever. Thus, in pneumonia the breathing becomes more laborious, and the oppression greater, as if the pulmonary congestion increased as we diminished the general resistance by loss of the blood. So in hæmoptysis the blood in the expectorations becomes redder and more abundant; and in metrorrhagia the flow is increased. In typhoid fever bleeding is of use when the blood is black, and constantly hurtful when it is red. The remark should not be lost sight of by those who practice bleeding *coup sur coup*. In all these cases the pulse is accelerated in proportion as the blood flows and the individual becomes enfeebled, the artery seeming fuller and harder, so that the most experienced may become embarrassed. In threatened miscarriage, wherein bleeding is often so necessary, if we practice it when the blood is red, we inevitably induce abortion. I therefore always recommend those pupils who bleed for me to be guided in the quantity they draw by the colour of the fluid, and they have often been in a condition to prove the correctness of the rule. It has happened to me sometimes to close a vein immediately after having opened it, and to suddenly stop the flow upon this simple indication, and in all these cases I have never had cause to regret it. * * * * * * When a patient is about to enter upon convalescence, the pulse often becomes full and frequent, the cheeks are flushed, the eyes shining, and the skin hotter—in a word, there is fever; and it is not rare for the practitioner to hesitate whether he should abstract blood or content himself with dieting. I have always been safely guided by the colour of the blood. Black blood coagulates and reddens after it has left the vein; when it remains black on the surface it is diseased; and when it does not coagulate it is dead, as is seen in that flowing from old ulcers, uterine and other cancers. In the bleedings practised on young pregnant women red blood at first flows out and then black; the former, I believe, coming from the superficial, and the latter from the deeper-seated veins. In the treatment of a disease, then, if on opening the vein the blood is black, let it flow; if it is red, be certain that the disease is vanquished, and the point of tolerance reached. Great thirst also, which almost always follows excessive bleeding, is not a symptom to be overlooked. In organic disease the blood is generally black; but it may not always be prudent to abstract it, while if it is red we shall but increase the gravity of the disease.

Another remark, which I do not remember to have seen, is, that the blood in a diseased part may be different to that in the economy at large. I have seen it flow black from the general circulation, and red from leeches or cupping-glasses, and *vice versa*. It is this that revealed to me the importance of bleeding in the treatment of cancer, or in tumours threatening to become cancerous; but that always in relation to the general vital powers; and in such a manner as to prevent general infection by absorption, which bleeding hinders or favours, according to the circumstances under which it is undertaken.

M. Tanchou goes on to state, that in the inflammatory and congestive *Diseases of the Uterus* repeated bleeding may be very useful, as long as it is not carried to excess; and that proofs of its being so are derived from the fact of the resistance of the disease, and the signs of general enfeeblement of the economy. Most of these effects of the too-great loss of blood are familiar to our readers; but he mentions two which are probably less so, viz. *constipation* and the *condition of the urine*. The stools are inodorous, infrequent, and lumpy, intestinal absorption having extracted all that was possible from them. The urine is high-coloured, but without any deposit, and emits a peculiar penetrating odour when passed, which disappears on cooling. This odour is at once recognized by the experienced observer as the most infallible sign of inanition, frequently appearing before any of the others.—*Gazette Medicale*, No. 20.

[The author, in detailing his practice still farther, recommends general bleeding in uterine affections to a far greater extent than would be countenanced in this country, and seems to much fear discontinuing it otherwise than gradually. He cautions his readers, however, against the abuse of the lancet, and not without reason, for he says, "We have seen women who have been bled 80, 95, 100 times, or even more, in one or two years!" Shade of Guy Patin, arise, and testify that thy descendants are no degenerate phlebotomists! We have quoted the paper however on account of M. Tanchou's remarks on the indications derivable from the colour of the blood while flowing, which are new to us, and are so opposed to what is generally accepted, as to require confirmation.—*Rev.*]

The Pneumonia of Old Persons.

Of ten cases we observed at Salpétrière, seven were well-marked, and three doubtful. The term *doubtful* pneumonia may surprise some readers accustomed to meet with the well-known signs of the disease. But if we waited for the usual train of symptoms met with in the adult before we commenced treating pneumonia in the aged, we should often begin to act only when art had become powerless. For physicians accustomed to the aged, and who know how frequently in them it is *latent*, it suffices to observe *malaise*, fever, accelerated respiration, and a dry tongue, to direct their attention to pneumonia, especially in winter or spring-time. If we find in books that the pneumonia of the aged is more commonly than that of the adult preceded by precursory symptoms, and that it seldom shows itself in old persons suddenly, this is because too frequently the period of the commencement of pneumonia has been dated only from the appearance of stethoscopic signs. The earliest of these, and often the only one throughout the disease, is the feebleness of the respiration, or sometimes its complete absence over a certain extent. This sign, joined to bronchophony, constitutes for the experienced observer a certain indication of the presence of the disease.

The digestive and cerebral functions are much oftener disordered in the aged than in the adult. A dryness of the tongue, which alone may lead us to fear a pneumonia, is complicated with a yellow or brownish coating, billious vomiting,

and even delirium. An icteric colour of the face has not excited sufficient attention, notwithstanding its so constant co-existence with pneumonia of the apex.

Most practitioners are very circumspect in bleeding in these cases; but their fears are based upon no sound appreciation of facts. We see old pneumonic persons bled two or three times without any ill-effect, the blood retaining its normal characters and the characteristic buffing. M. Fauvel, after having bled the patient once, twice, and even three times, according to their strength and the amount of fever, and generally after having on the first day given antimony as an injection, administers from five to twelve of the following powders:—Calomel 8 grs., Opium 2 grs., Tartar emetic 1 gr. Mix for eight Powders. Salivation does not generally take place until the 3rd or 4th day; but as soon as that is perceived, the patient's condition ameliorates; but the powders are continued until resolution has commenced. He formerly gave tartar-emetic in large doses; but was obliged to renounce it in consequence of the inflammations of the throat and bowels it gave rise to. It is to be observed, that in the individuals who have long lost their teeth, salivation does not take place, or only with great difficulty, the gums remaining unaffected amidst the general buccal inflammation. In some of these patients an intestinal flux, coincident with the resolution of the pneumonia, is substituted for the salivation.—*L'Union Medicale*, No. 39.

Anesthesia in Saturnine Affections.

M. Beau has remarked that all the subjects suffering from saturnine affections, manifest more or less insensibility of surface. We do not allude to that saturnine anesthesia described by authors, when it exists in a marked degree, and which is not seen above six or eight times in a hundred cases. M. Beau declares, and he has shown us several cases now in his wards, that whatever may be the saturnine affection under which the patient is labouring, severe or slight, there is always *insensibility of the skin*, in some regions oftener than in others, and in proportion to the intensity of the saturnine disease. We saw him prick the skin of the arms and hands of several of these patients without their feeling any notable pain; so that lead may be stated to blunt sensibility. The fact is a new and important one; for it may aid in the diagonosis of some doubtful cases.—*Gazette des Hôpitaux*, No. 54.

Alum as an Emetic in Croup.

Dr. Meigs, relating some cases of true croup, takes the opportunity of confirming the accuracy of the account furnished by his father long ago of the great value of alum as an emetic in this disease. A *teaspoonful* given in honey, treacle, or other vehicle, acts more speedily and certainly than any other substance, and that without inducing prostration of the system. It is rarely necessary to repeat it.—*American Journal of the Medical Sciences*, No. 26.

Cod-Liver Oil.

M. Bretonneau states that he has observed effects just as markedly beneficial to result from the use of the common whale oil as from the cod-liver oil: an important fact, seeing the great expensiveness and the frequent adulteration of the latter.

GRAY'S SUPPLEMENT TO THE PHARMACOPŒIA; being a concise but comprehensive Dispensatory and Manual of Facts and Formulæ. Entirely re-written, re-arranged, and considerably enlarged. By *Theophilus Redwood*, Professor of Pharmacy to the Pharmaceutical Society of Great Britain. Octavo, pp. 1118. London, 1847.

MR. REDWOOD has added a great deal of interesting and very useful information to this new edition of a well-known work. The introductory chapters, containing a chronological history of Pharmacopœias and Dispensatories, an account of weights and measures, description of the methods of taking specific gravities, tables of thermometrical equivalents, &c., a pharmaceutical calendar, and various other matters, have been carefully and correctly prepared. We observe some omissions in the more strictly pharmaceutic department, that Mr. R. will do well to supply in his next edition. For example, we happened to look into the work for the composition of Ruspini's styptic, but found no notice of it. Such a well-known formulæ, too, as *vinum ferri*, *vinum antimonii*, ought surely to have had a place. The addition of some toxicological tables also would be of service. To find room for these and various other matters unnoticed, much of the chapter on animals might be left out without any detriment to the usefulness of the work.

BIBLIOGRAPHICAL RECORD.

1. The American Journal of Insanity. Edited by the Officers of the New York State Lunatic Asylum, Utica. Parts I. and II. Vol. III. 8vo., pp. 192. Utica, 1846.

2. Statistics of the Royal Infirmary of Glasgow. 3d Series. For 1846. By *R. S. Orr*, M.D. 8vo, pp. 36.

3. Thoughts on the Nature and Treatment of several severe Diseases of the Human Body. By *Edward J. Seymour*, M.D. Two Vols. Vol. I. 8vo, pp. 264. London, 1847.

4. The Transactions of the Provincial, Medical, and Surgical Association. Instituted 1832. Vol. III. New Series. 8vo, pp, 437. London, 1847.

5. Remarks on the Diet of Children, and on the Distinctions between the Digestive Powers of the Infant and the Adult. By *G. T. Gream*. 8vo, pp. 201. London, 1847.

6. The Cyclopædia of Anatomy and Physiology. Edited by *Robert B. Todd*, M.D. Part 28. London, 1847.

7. Erect Vision from an Inverted Image. By *B. F. Joslin*, M.D. From the New York Journal of Medicine for November.

8. A Treatise on Fractures in the Vicinity of Joints, and on certain Forms of Accidental and Congenital Dislocations. By *R. N. Smith*, M.D. 8vo, pp. 325. Dublin, 1847.

9. Inhalation of Ether. By *J. Mason Warren*, M.D. 8vo, pp. 18. London, 1847.

10. The Surgeon's Vade Mecum. By *Robert Druitt*. Fourth Edition, 156 wood engravings. 8vo, pp. 640. London, 1847.

11. The Construction and Government of Lunatic Asylums and Hospitals for the Insane. By *John Conolly*, M.D. With Plans. 8vo, pp. 191. London, 1847.

12. A Case of Large Secondary Prostatic Calculus removed by Perinæal Incision. From the "Transactions" of the Association. Vol. III. New Series. By *T. Herbert Baker*, M.B. 8vo, pp. 12. London, 1847.

13. Observations on the Connexion between Fever and Famine in Ireland and elsewhere. By *Henry Kennedy*, A.B., &c. 8vo, pp. 50. Dublin, 1847.
A very able pamphlet, well deserving of attentive perusal.

14. A Treatise on the Human Ear, with New Views on the Physiology of the Tympanum. By *J. W. Moses*, M.D. 8vo, pp. 18. St. Asaph, 1847.
Creditable to the ingenuity of the author. His parallel between the cavity of the thorax and the tympanum is cleverly worked out.

15. Practical Observations on the Pathology and Treatment of certain Diseases of the Skin generally pronounced intractable. By *Thos. Hunt*, M.R.C.S., &c. 8vo, pp. 168. London, 1847.

16. Remarks on Medical Organization and Reform (Foreign and English). By *Edwin Lee*. 8vo, pp. 163. London, 1847.

17. A Treatise on the Structure, Diseases, and Injuries of the Blood-vessels, with Statistical Deductions. Being the Jacksonian Prize Essay for the Year 1844. By *Edwards Crisp*. M.R.C.S., &c. 8vo, pp. 370, with Plates. London, 1847.

18. The Physiological Anatomy and Physiology of Man. By *R. B. Todd*, M.D., and *W. Bowman*, F.R.S. Part III. 8vo. London, 1847.

In our next.

19. A System of Surgery. By *J. M. Chelius*. Translated from the German, and accompanied with additional Notes and Observations. By *J. F. South*. Part XVI. 8vo. London, 1847.

20. A Treatise on Diet and Regimen. By *William Henry Robertson*, M.D. Part II. 4th Edition. Rewritten and enlarged. 8vo. London, 1847.

21. Observations on Aneurism and its Treatment by Compression. By *O'Bryen Bellingham*, M.D. Edin. 8vo, pp. 189. London.

22. Hydropathy and Homœopathy impartially appreciated, with an Appendix of Notes, illustrative of the Influence of the Mind on the Body. By *Edwin Lee*, Esq. The third Editions combined. 8vo, pp. 143. London, 1847.

23. Report of the Health of London Association on the Sanitary Condition of the Metropolis. 8vo, pp. 68. London, 1847.

24. Report of the National Philanthropic Association, instituted March, 1842. 8vo, pp. 30. London, 1847.

25. Vaccination, considered in relation to the Public Health, with Inquiries and Suggestions thereon. A Letter addressed to the Lord Visc. Morpeth. By *John Marshall*, Surgeon. 8vo. pp. 34. London, 1847.

26. The American Journal of the Medical Sciences. Edited by *Isaac Hays*, M.D. No. 26, for April, 1847.

27. On Pulmonary Consumption, and on Bronchial and Laryngeal Disease, with Remarks on the Places of Residence chiefly resorted to by the Consumptive Invalid. By Sir *Charles Scudamore*, M.D. 8vo, pp. 278. London.

In our next.

28. On Dyspepsia, with Remarks, submitted in support of the Opinion that the Proximate Cause of this, and of all other Diseases affecting the general system, is Vitiation of the Blood. By *John Burdett Steward*, M.D. 8vo, pp. 116. London.

29. On the Pathology and Treatment of Dysentery, being the Gulstonian Lectures, delivered at the College of Physicians, in February, 1847. By *William Baly*, M.D. From the London Medical Gazette. 8vo. pp. 33. London.

Contain much interesting and valuable information, more especially upon the Dysentery that is endemic in Millbank Prison.

30. Researches on the Chemistry of Food. By *Justus Liebig*, M.D. Edited by *Wm. Gregory*, M.D. 8vo, pp. 176. London.

In our next.

31. On Sir Charles Bell's Researches in the Nervous System. By *Alexander Shaw*, Surgeon of the Middlesex Hospital. Small 4to, pp. 40. London, 1847.

32. Table of Urinary Deposits, with their Tests for Clinical Examination. By *Ray Charles Golding*, M.D.

Will be useful to the student.

33. Practical Remarks on the Inhalation of the Vapour of Sulphuric Ether. By *W. Philpott Brooke*, M.D. 8vo, pp. 68. London, 1847.

34. Gazette Medicale, April to July.

In exchange.

35. L'Union Medicale, January to July.

In exchange.
Nos. 58 and 61 *have not arrived.*

36. Dublin Medical Review for May.

In exchange.

37. Edinburgh Medical and Surgical Journal for April.

In exchange.

38. British and Foreign Medical Review for April.

In exchange.

39. Edinburgh Monthly Journal of Medical Science, for April, May, and June.

In exchange.

40. Medical Gazette, April to July.

In exchange.

THE

MEDICO-CHIRURGICAL

REVIEW.

OCTOBER, 1847.

THE HUMAN BRAIN; ITS STRUCTURE, PHYSIOLOGY, AND DISEASES. With a Description of the Typical Forms of Brain in the Animal Kingdom. By *Samuel Solly*, F.R.S., Senior Assistant-Surgeon to St. Thomas's Hospital, and Lecturer on Clinical Surgery, &c. &c. Second Edition, with numerous Wood Engravings. 8vo. pp. 684. London: Longman, Brown, Green, and Longmans, 1847.

WE had occasion, at no very distant period, to call the attention of our readers to the existing state of knowledge in reference to the Nervous System (see Med.-Chir. Review, July, 1845). The several publications which were then noticed, valuable as they undoubtedly are, have by no means precluded the necessity for further and more varied cultivation of this deeply interesting branch of science; a consideration which causes us to hail with much satisfaction the re-appearance of a well-known and successful labourer in the field of neurological research. It is now some years since the first edition of Mr. Solly's work was published; and it is but just to this gentleman to state, that his treatise on the Brain was the first which, in this country, combined all the elements requisite for a scientific investigation; that is to say, in which comparative anatomy, embryology, experiment and pathology were brought to bear upon the anatomy and physiology of the human cerebrum. This enlightened mode of procedure is deserving of all commendation; and cannot fail, when generally applied, in stamping a philosophic character on Medical literature. In the present edition, the author has pursued the same course, and by omitting some details of structure, in themselves of no great moment, he has been enabled without objectionally enlarging the work, to present the medical public with the most complete account of the anatomy, physiology, and pathology of the brain that has hitherto appeared. Mr. Solly has enriched the descriptive portion of the volume with a large number of wood-engravings, many of them the product of his own pencil; and which, being intercalated with the text, give an increased interest and clearness to the details they so admirably illustrate.

It is satisfactory to us to perceive that a writer so well qualified to pronounce an opinion as the author of this work, has fully recognised and

ably supported those principles which, although they have been occasionally lost sight of or even directly opposed, we have ever regarded as the clue by which alone the complex organization and intricate actions of the nervous system can be successfully studied and interpreted. Two of the most fundamental of these principles are thus stated :—

"The revelations of the microscope regarding the ultimate texture of these different kinds of neurine are most deeply interesting, and quite determine the correctness of the view advocated in the first edition of this work, of their relative function. This view of the subject is now almost universally admitted, but in the year 1836 it was by no means an established point in physiology. The view to which I refer is this: that the cineritious neurine is the source of power, and the medullary neurine merely the conductor of it. The importance of establishing this position will be best understood when we come to the dissection of the human brain and spinal cord, and endeavour to discover the office of their component parts. Until this point was established, (and even now it is not considered to be so by all,) the study of the anatomy of the brain was barren and fruitless." P. 2.

A third fact, equally essential, is the strict independence, structurally and functionally, of the primitive nervous fibrillæ—"nerve-tubes never branch like blood-vessels and never inosculate with one another;" and, consequently, a "nerve-tube always performs one and the same office; it always conducts in the same direction, and the same kind of nervous power; not at one time carrying impressions which, on reaching the brain, become sensations, and at another time conveying orders to a muscle to contract." (P. 12.) The power of the primitive fibrillæ being thus limited, the varied actions of the nervous system necessitate the provision of distinct classes of conductors, which the author thus, correctly as we conceive, sets forth :—

"We find tubular neurine performing various offices :—

"1st. As a conductor of an impression from the surface of the body to the brain, —*a nerve of sensation.*

"2ndly. As a conductor of an order to act, from the brain to the voluntary muscles —*nerve of volition.*

"3rdly and 4thly. As a conductor of an impression from the surface of the body to the spinal cord, which is reflected thence down another set of conductors to the muscles whereby they are called into action, independently of volition;—the *excito-motory nerves* of Dr. M. Hall.

"5thly. As a conducting medium between the centres of power—the *commissures.*" P. 7.

Mr. Solly assumes, as the basis of all inquiries into the nervous centres of man, the facts revealed by comparative anatomy. In the Preface we find these judicious remarks: "The only philosophical method of simplifying and giving a character of general interest to the anatomy of the human brain, is by commencing with the structure and functions of a nervous system in the lowest and simplest forms of animal existence, rising by degrees to the highest, carefully observing each addition of parts, and the relationship borne by these to an addition of function. By pursuing this course we shall be rewarded by finding that the encephalon, this apparently most complicated organ in the human being, is but a gradual development from an extremely simple fundamental type on one uniform and

harmonious plan, and that the seeming complexity of the cerebro-spinal axis in man really arises from the great concentration, as opposed to the extreme diffusion of its component parts in the lower order of animals; for in no particular are the higher orders more strikingly distinguished from the lower than in the concentration of function within circumscribed spaces." (*Preface*, p. x.) In interpreting the evidence derivable from this source, the author applies as a realised and fertile principle this fact—that "one of the most important functions of a nervous system, as regards the vital existence of an animal, is to receive impressions, and to re-act on such impressions, independent of the consciousness or the will of the individual: this fact will be found universal in its application." In other words, the author unreservedly adopts the views of Marshall Hall on the physiology of the nervous system; and those of Grainger, Carpenter, and Newport on its anatomy.

We must pass by, though reluctantly, the details connected with the invertebrate animals, and proceed briefly to notice the interesting facts illustrative of the conformation of the brain among the vertebrata. In order to seize upon the fundamental organs, in the midst of the intricacies of apparent confusion of the cerebral parts, the author assumes three leading facts:—1, that every nerve of sense, whether it be of the sense of smell, sight, hearing, taste, tact, or of simple sensation, has, at its central extremity, a collection of cineritious neurine, or a ganglion; 2, that there is an organ for directing and controlling the actions of locomotion, corresponding to the cerebellum; 3, that there are parts, analogous to the hemispheres of the human brain, which constitute the instrument of the mental operations. We have no doubt that, so far as the anatomy is concerned, this is the true principle in accordance with which the encephalon must be studied—that it is the only mode of escaping from that obscurity, in which, as Mr. Solly justly observes, writers on comparative anatomy have involved this deeply interesting subject. As it is quite clear that the brain of fishes must serve as the basis for the comparison of the higher vertebrata and man, it becomes a point of primary importance to fix upon the signification of the curious series of ganglia which are seen on exposing the cranial cavity, in some of the simplest animals of this class. There is one point connected with this inquiry, which, if not truly determined, throws the whole subject into inextricable confusion—we refer to the just appreciation of the *olfactory ganglia*. These bodies, whether placed close to the hemispherical ganglia, as in the eel, or removed from them to a considerable distance, as in the whiting, constitute invariably the most anterior pair of the encephalic masses. To the author belongs the merit of having given the true homology of these ganglia more than ten years ago, when the most eminent authorities, among whom we may adduce Serres, had evidently no clear idea of their signification. Placed next in order behind the olfactory ganglia or lobes, are the two hemispherical ganglia, as Mr. Solly proposes to call the hemispheres; then follow the two optic ganglia; and, lastly, the cerebellum. There are several other subordinate masses, for an account of which we must refer our readers to the work before us, and to the Hunterian Lectures of Professor Owen. Thus, among fishes, are to be recognised the typical elements of the human brain, the vast distance between them being filled up by

the multitudinous forms displayed in the ascending animal scale, and by the no less exact evidences of developmental anatomy.

We regret that our limits will not permit us to follow the author through his clear and comprehensive account of the successive perfectioning of the cerebral organization; and especially of the laws in obedience to which the convolutions are developed. The whole of this part of the work, illustrated as it is with a great number of admirably executed wood-cuts, is deserving of careful perusal, and will, we feel assured, receive general commendation. Before quitting this subject of the comparative anatomy, it may, however, be desirable to extract the following passage, in which several errors are corrected respecting the existence of that interesting structure, the *rete mirabile*, a rich network of vessels into which the carotid artery divides after it has penetrated into the skull, and lies beneath the dura mater by the side of the sella turcica:—

"Rapp found this plexus in the stag, roe, the fallow-deer, chamois, the goat, sheep, and calf, and oxen. He considers that the arrangement of the foramina in the base of the skull in the camel indicates its existence in this animal, but he has not had the opportunity of seeing the parts in a recent state. It exists also in swine, but it does not occur in other Mammalia besides the Ruminantia and swine. Cuvier's statement certainly differs from this; he says that this vascular arrangement appears to occur in most of the Carnivora, but is absent in the elephant and beaver. According to Carus it is present in most Mammalia, and Willis says it exists in the dog, the fox, cat, &c.; but this is a mistake, for it does *not* occur in the dog, fox, badger, weazel, otter, or hedgehog, or in the domestic cat. But it has been found by Mr. Quekett in the leopard. Neither is it found in man, the apes, horse, elephant, or the Rodentia."—P. 137.

It is curious that in that remarkable ruminant the Giraffe, notwithstanding it feeds on the tops of trees and with the head therefore elevated, there is a well-developed rete mirabile, a fact which has been lately ascertained in opposition to the commonly-received opinion, by Mr. Quekett; and which proves, what is shown by so many other instances, that unity of structure is a more predominating law in the animal kingdom, than that of final causes or design.

In the Section on the Spinal Cord, Mr. Solly expresses his conviction of the accuracy of Mr. Grainger's account of the origin of the nerves; and he has given a most interesting figure of a minute dissection, made with the assistance of that gentleman, in which the fibres of the anterior root are represented as becoming partly continuous with the longitudinal fibres of the spinal marrow, and partly as running into the gray matter of the cord and there terminating. These results correspond with those obtained by Dr. Budge, noticed already in this Journal (Med.-Chir. Rev. 1845, p. 14): and, as both examinations appear to have been made with much care by the aid of the microscope, they may be received as correct.

The author has given a most minute and detailed account of the composition of the medulla oblongata; of the signification of the various nuclei of gray matter there observed; and especially of the distribution and physiological endowments of the several fasciculi or strands of white fibres connected with this vital segment of the nervous centres. Although some of our readers may suppose that these anatomical intricacies concern rather the student than the practitioner, they will, on well considering

these details and on scrutinising the excellent figures by which they are brought before the eye, find that a knowledge of this complex organization is indispensable to the satisfactory investigation of cerebral pathology. The reason why the descriptions of the anatomist have, as regards this part of the body, become so distasteful, is because they have been unfruitful; because, in the midst of a detail which is discouraging and redundant even to professed teachers, not a glimpse of those relations upon which the actions of the brain, whether healthy or morbid, repose, is to be caught; because, in fine, the authors of France and Germany, and herein too often imitated by our own countrymen, have broken up the brain into an infinitude of fragments, insignificant in themselves and utterly obstructive to those general and connected views of structure, which can alone subserve an enlightened physiology. The account given by Mr. Solly is opposed in toto to this barren topographical school: the several parts, instead of being considered piece-meal, are invariably viewed in reference to those other structures with which they are allied as members of an active living organization: if complex bundles of fibres are to be considered, the tedium of tracing them is obviated, by making the anatomical process reveal some interesting physiological action; or, if nodules of the vesicular matter are to be described, the question is not what is the precise shade of colour or the exact number of indentations, but what part does it play in the mysterious operations of conscious existence.

But we must allow the author to speak in his own language.

"According to the plan generally pursued in treating on the brain in systematic works of anatomy, the information conveyed amounts to little more than a vain catalogue of names applied to parts, without reference to their structure, their functions, or even their analogies in the nervous system of the lower orders of animals. Such a barren prospect as a list of names holds out but little to attract the most zealous among students, while the dryness of unconnected detail, and the obstacles to clear conceptions engendered by the absence of everything like arrangement, almost certainly deter him from attempting to learn more than is required to prepare him for examination for the diploma. It is unfortunate, indeed, that candidates for this honourable certificate are still very generally required to describe the appearances presented by the brain dissected, or rather destroyed, by the old method of slicing; a method most unphilosophical in its conception, and totally inadequate to impart any real information in regard to the structure of the organ. And I do not hesitate to affirm that this mode of examination has contributed essentially to retard the diffusion of sound knowledge in regard to the anatomy and physiology of the most important system in the body."—*Preface*, p. vi.

We had hoped that the mode of examination here so properly condemned, had been long since abolished: if, however, any remnants of the old and barbarous system still linger in high places, we trust the scientific works on the brain, which have lately issued from the press of this country, will speedily induce the examining bodies to place themselves on a level with the present state of knowledge.

A few extracts will serve to justify the very favourable opinion we have expressed of Mr. Solly's work; and, also, to illustrate what we conceive to be the true method of pursuing the anatomy of the nervous centres. The question respecting that somewhat unpromising subject—the fissures and subdivisions of the spinal cord, is thus discussed:—

"*Line of demarcation between the tracts of sensation and motion.*—Although the different offices performed by the anterior and posterior roots of the spinal nerves have been, I think, clearly ascertained, and as it is also evident that the spinal cord consists of tracts of neurine whose office is the same as the nerves which are connected with them, and therefore that there are portions of the cord which perform functions as distinct from each other as the arteries and veins, still anatomists are not yet agreed as to the line of demarcation between them. Sir Charles Bell, for instance, in a paper published in the 135th vol. of the Philosophical Transactions, states that he regards the lateral portion of the antero-lateral columns as a part of the tract for sensation, and I have no doubt of its correctness. The circumstance of there being no decided anatomical line of division between the two columns is not of itself an argument against the correctness of this view; for it is quite possible that perfect distinctness of parts, as regards their function, without any visible line of separation between them, may exist. We must always bear in mind that the neurine which composes the cord is supported and clothed by a perfect though delicate membrane, which, pervading its substance in every direction, is undoubtedly as capable of separating masses of neurine endowed with distinct powers, and ordained by nature to execute distinct offices from each other, as any fissure however wide, or membrane however thick. The presence of such gross and palpable partitions, it is true, would save us some trouble in discovering the line of demarcation, but would not necessarily make it in any way more efficient. They are not the less distinct organs because of our ignorance of their respective limits, any more than a nerve of motion is one of sensation because we are incapable of unraveling the fibres of each from their common investing membrane.

"That the boundary line between the two organs of sensation and voluntary motion comprised within the spinal cord cannot be formed by the posterior peak of grey matter, is very decidedly proved by the fact that a portion of the fifth pair of nerves, which we know to be a nerve of sensation from the beautiful experiments of Mayo and Sir Charles Bell, is not connected with the posterior, but with the lateral columns."—*L. c.* p. 223.

This is as it should be; the physiologist refusing to be bound by the apparent evidence of anatomy, when that evidence is over-ruled by superior considerations; but availing himself of a significant structural arrangement, to explain a difficult point of function. We are reminded by this quotation, that, as far as our memory serves us, the merit of seizing upon the precise attachment of the portio major of the fifth pair, as an indisputable indication of the extent of the sensory column, in this and other parts of the work, is due to the author.

There is no point of greater interest to the practitioner, than to have a clear conception of the mode in which the cerebellum and the cerebrum are respectively brought into connexion with the motor and sentient organs of the animal frame; for without this knowledge no satisfactory investigation can be made into a single case of paralysis, apoplexy, or convulsion. There is one fundamental truth which is the sole clue in all these cases; and which, although it may appear to be sometimes contravened by peculiar and inexplicable symptoms, is nevertheless recognisable on a careful scrutiny as a real principle in cerebral pathology: it is this—that the morbid effects, as evidenced by disturbance of the organs of sense and motion, are to be understood by following the tracts of fibrous matter proceeding from the spinal cord to the cerebrum and cerebellum. In this point of view, the physiological character of the fibres composing respectively the corpus restiforme going to the cerebellum and the crus cerebri running to

the cerebrum is a point of primary importance. The credit of explaining the true constitution of the former of these cords belongs exclusively to Mr. Solly; for although some preceding writers, and especially Rolando, were in part acquainted with the fact that some fibres of the anterior cords of the medulla spinalis passed towards the cerebellum, the author was the first anatomist who discovered that the corpus restiforme receives fibres from the anterior or motor column of the spinal cord. He remarks, with justice, that "the fibres just described as connecting the antero-lateral columns of the cord with the cerebellum are peculiarly interesting when viewed in relation to the functions of the cerebellum. For although it is true that its functions have not yet been clearly ascertained, the experiments of Flourens, Bouillaud, Magendie, and others, and the numerous cases on record in which disease of the cerebellum has been followed by paralysis, all tend to prove that the cerebellum is in some way or other connected with the regulation of muscular action, most probably, as before hinted at, that it has the power of combining the action of individual muscles so as to effect an harmonious result, such as is necessary to enable us to stand, walk, &c."—*L. c.* 231.

Our readers will agree in the truth of these observations, when they recollect that some most distinguished authorities, being ignorant of this connexion between the lesser brain and the motor tract of the cord, and equally uninformed as to the true formation of the crus cerebri, have not hesitated to enunciate theories, which, if received, would involve the whole pathology of the encephalon in hopeless confusion. Thus, at no very distant period, Foville, assuming that the posterior columns of the spinal medulla are alone prolonged into the cerebellum, and the anterior or motor as exclusively into the cerebrum, inferred that the former is more especially the organ presiding over the sentient phenomena, and the latter, or cerebrum, as exclusively the agent predominating over voluntary motion; an hypothesis which, notwithstanding it was opposed to all the evidences of physiology and pathology, received a considerable portion of assent, and was even admitted as a probable opinion by that eminent writer. Dr. Pritchard.

As in addition to the motorial fibres, the corpus restiforme is well known to receive threads from the posterior aspect of the spinal cord, or the sentient tract; and as moreover there is evidence to show that the restiforme fibres form a part of the decussating apparatus of the medulla oblongata, all the anatomical relations required to explain the phenomena of disease are realised. The subject thus elucidated, there is no longer any difficulty in comprehending how irritation of the cerebellum may disturb the muscular actions; how effusion of blood into its substance may cause a paralysis both of motion and sensation; and, lastly, as so many cases, especially those recorded by Andral, prove, how extravasation or ramollissement of one of the hemispheres will, as in the case of the cerebrum, induce hemiplegia on the *opposite* side of the body. A reference first of all to the graphic illustrations of Mr. Solly (especially to figs. 88, 89 and 90), and then to the sections on the pathology of the cerebellum, will remove all obscurity on this very interesting and difficult subject.

The true anatomical structure of the crus cerebri is a point of equal importance with that of the restiforme body; as this, however, is so well

bnown, we will only call attention to the circumstance of there being, altogether distinct from the decussation of the pyramidal bodies, a distinct decussation of the fibres forming the sensory tract of the cerebrum. This interesting disposition takes place where these sensory tracts form the floor of the iter a tertio ad quartum ventriculum; and, as many anatomists have overlooked its existence, the following account of the mode of exposing this structure may not be unacceptable :—

"The best mode of demonstrating this interlacement is first to separate the medulla oblongata, with the pons Varolii, crura cerebri, and optic thalami, from the rest of the brain. Secondly, divide the pons Varolii anteriorly, in a longitudinal direction, through the centre to the depth of half an inch; divide the pyramidal decussation; then take the two lateral halves of the cord and split them upwards, tearing through the floor of the fourth ventricle. When the rent passes the roots of the auditory nerve, fibres, the size of ordinary ligature silk, may be seen running obliquely across the mesial fissure, from one side to the other, decussating with their fellows. This decussation may also be demonstrated anteriorly, though it requires more care and some dissection."—P. 243.

We need hardly observe that this intercrossing of the sentient fibres, regarded in connexion with the decussation of the motor tracts or corpora pyramidalia, offers a perfectly satisfactory explanation, why, in cases of effusion into one hemisphere, sensation as well as motion is lost on the opposite side of the body.

As it is one principal object with us to lay before our readers, with the assistance of the work before us, some of the fundamental facts connected with the organization of the nervous centres, we would now call their attention to two points, which, although they are perhaps of greater importance than any others relating to cerebral anatomy, have been either denied or doubted by some late writers, and especially by Foville. The first of these questions concerns the ultimate destination of the motor and sentient fibres of the crus cerebri. That these fibres, after traversing the corpus striatum and optic thalamus, continue their course, and run finally into the very substance of the cerebral convolutions, is one of the most fertile truths for which anatomical science is indebted to Gall and Spurzheim, but especially to the former. Mr. Solly has always contended for the correctness of this view of the subject; and, as it affords a satisfactory explanation of some of the most common cases of cerebral disease, we cannot do better than follow the example of our author, who has quoted the following familiar but truthful description from the great founder of modern physiology, Sir Charles Bell :—

"The thalamus forms a nucleus round which the corpus striatum bends, and when their respective layers of striæ make their exit beyond these bodies to form the great fan, or solar-like expansion into the hemisphere of the cerebrum, their rays mingle together. A rude representation of these two parts of the cerebrum as we have traced them may be made with the hands. If I place my wrists together, parallel, and closing one hand embrace it with the other, I represent the two portions of one crus. The closed fist is the thalamus, and the other is the corpus striatum. If I then extend my fingers, interlacing their points, I represent the final distribution of the portions of the nervous matter which are dedicated to sensation and volition."—P. 244.

A second point, which has also been placed in some degree of doubt, is

the real formation of the corpus callosum. If we are to adopt the hypothesis of Foville, this vast organ, the largest of the encephalon, is nothing else than a connecting medium between the crura cerebri, instead of being, what is shown equally by careful dissection and comparative anatomy, the great commissure of the hemispheres of the brain. The author, herein concurring with the best English anatomists, contends for the truth of the latter account;—"the great *transverse commissure*, or corpus callosum, is a body consisting of fibres of medullary neurine, the extremities of which are everywhere in contact with the internal or central surface of the cineritious layer which forms the convolutions of the hemispheres,—the hemispherical ganglia. These fibres consequently establish a communication between the cineritious neurine of the whole convoluted surface of both sides of the cerebrum."—(*L. c.* p. 250.) This account is illustrated by an admirable and most correct figure of the fibrous expansion above described.

Condemning, as we totally do, the cumbrous descriptions still persisted in by so many writers and teachers; and confidently anticipating that, at no distant period, the whole of these will be replaced by an intelligible and simple anatomy of the brain, we have much pleasure in extracting the following judicious observations, with which the author concludes his philosophic account of that organ:—

"In conclusion, let me express the hope that these views or analyses, if I may be allowed so to call them, of the component parts of the encephalon will really simplify the whole of its anatomy, and materially assist the student in acquiring a knowledge of its true character. I wish that custom did not require the student to burthen his memory with fanciful and unmeaning names, and that, instead of learning a long catalogue of the contents of the lateral ventricles as they are erroneously designated, and puzzling himself with the absurd titles of hippocampus major and minor, pes hippocampi, tænia hippocampi, cornu Ammonis, &c., he should be required simply to observe how the spinal columns appear to terminate superiorly in two large tubercles, the *corpora striata* and *thalami*, from the sides and under parts of which the hemispheres spring out, being afterwards reflected so as completely to envelope this bulbous extremity of the spinal cord. In the same way the third ventricle should be described as a fissure separating the two halves of the brain, his particular attention being directed to the commissures which pass across it to connect the different cerebral ganglia with one another. The description of the relative position of these ganglia, the commissures connecting them, and their relation to the ganglia and columns of the spinal cord, comprehend all the information which is either interesting or useful to the student." *L. c.* p. 283.

We regret that our limits compel us to pass by the section treating on the central attachments of the so-called cerebral nerves, and especially that relating to the development of the nervous system.

In the tenth chapter the author gives a brief resumé of his physiological views, in many of which we fully coincide; but from others we must withhold our assent. The following are regarded, and justly, as "fundamental principles:"

"1. That vesicular neurine is the source of power.

"2. That medullary neurine is the conductor of it.

"3. That medullary neurine is also the conductor of those impressions which call forth the power of the vesicular neurine.

"4. That the vesicular neurine is collected in masses of variable form and size—the *ganglia*.

"5. That the medullary neurine is moulded into cords and bands—the *nerves* and *commissures*." P. 329.

The functions of the nerves are thus described:—

"The experiments of Sir C. Bell, Magendie, and Mayo, have proved that there are nerves subservient to sensation—sensiferous or sensory nerves, and nerves of voluntary motion. The physiological researches of Whytt, Prochaska, and, more perfectly, Marshall Hall, confirmed by the anatomical observations of Grainger, Carpenter and Newport, have established another system of nerves for the involuntary—the conservative movements of the body under the title of the excito-motory system of nerves. All sound research and careful experiment prove that a nerve in the whole extent of its course, whether that course is between the fibres of a muscle, in the canal of a bone, in the substance of the spinal cord, in the crura of the brain, or in the masses of the hemispheres, always performs one and the same office, conducting always in one and the same direction." P. 330.

In approaching the complex organs enclosed within the skull, Mr. Solly is impressed with the same difficulties that have obstructed all physiologists labouring in the same field: vivisections, comparative anatomy, and pathology, have thrown much light upon the mysteries of the mental phenomena, but still the curious inquirer is but too often doomed to disappointment. The author is inclined to adopt a theory, already advocated more or less by several eminent writers, among whom may be enumerated Drs. Todd and Carpenter and Mr. Bowman, to the effect that certain masses of grey matter—the auditory ganglia of the medulla oblongata, the tubercula quadrigemina, the optic thalami, and the corpora striata, are, independently of the cerebral hemispheres, so many separate "sensorial centres," that is, distinct organs of perception.

As there appears to us to be some obscurity, if not contradiction, in the views which Mr. Solly states he has adopted from Dr. Carpenter, we prefer presenting them to our readers in his own words:—

"The anterior and posterior cerebral ganglia are regarded by Dr. Carpenter as forming part of the series of sensorial centres, of which we have seen other members in the olfactory, optic, and auditory ganglia. That they are independent centres of action, not mere appendages to the hemispheric ganglia, appears from the large quantity of vesicular neurine which they contain; and that the corpora striata are so, further appears from the absence of any correspondence in size between them and the hemispheric ganglia. Thus in fishes, we find that the corpora striata make up the principal bulk of the second pair of masses; in reptiles, birds, and the lower Mammalia, they still form a very large portion of that which is commonly termed the cerebrum; and their subordinate aspect in man and the higher Mammalia is solely due to the large relative development of the hemispheric ganglia. On the other hand, there is scarcely any rudiment of the thalami optici to be discovered in fishes; their proportional size increases in reptiles, birds, and the lower Mammalia; but it is only in man that their dimensions approach those of the corpora striata. The peculiar connection of the thalami optici with the posterior columns of the spinal cord, and their great development in man, suggests the idea that they are the ganglia of *tactual* sensation; whilst the connection of the corpora striata with the anterior column indicates their relation with the motor function. The very close relation between the thalami optici and the corpora striata—corresponding, as Messrs. Todd and Bowman have suggested, with that which exists between the posterior and anterior peaks of grey matter in the spinal cord—harmonizes well with the fact that

the greater number of muscular movements are directed by common sensation; whilst the special connection established by the inter-cerebral commissure between the corpora striata and the optic ganglia (tubercula quadrigemina) explains the peculiar influence of the sense of light in directing certain classes of muscular actions. The communication which is formed by the medullary substance of the cerebrum between these ganglia and the hemispheric ganglia seems to be the medium by which *sensations* are transmitted to the latter, to become the stimulus of intellectual operations, and by which the influence of *volition* is transmitted downwards to excite muscular motions through the corpora striata." P. 334.

In addition to these functions, the "whole chain of sensory ganglia is regarded as the centre of those *automatic* muscular movements which differ from those of a simply reflex character in being dependent upon sensation;" such as the instinctive actions of the lower animals, and a variety of actions performed by the human being—the consensual movements of the eyes, the regulation of the laryngeal muscles in the production of vocal sounds, the convulsive movements in hydrophobia, brought on by the sight or sound of water, &c. As to the hemispheres, they are the exclusive organ of the mental functions—the instrument by which the sensorial impressions are not only perceived, but are converted into ideas.

As we have already stated (Med.-Chir. Rev. 1845, p. 490) some of the reasons which may be adduced in opposition to these doctrines, we would only observe further that, up to the present time, they have not been reconciled, so far as they relate to the seat of consciousness, with certain familiar and admitted facts in physiology and pathology. One or two of these only can be now quoted: if both hemispheres be removed in a dog, as in Hertwig's experiments, the animal does not hear the report of a pistol; if the hemispheres be excised in a pigeon, sight and hearing are abolished; if effusion of blood take place into the substance of one hemisphere *above* the corpora striata and optic thalami, or if from fracture of the skull a portion of bone be driven into the same part, inducing softening and suppuration of the white fibrous matter, there is paralysis more or less complete of the opposite side of the body. Now in all these and similar instances the assumed "sensorial organs," "the whole chain of sensory ganglia," are left intact; and yet the phenomena of consciousness, attributed by this theory to them—vision—audition—tactual sensation, and voluntary motion are destroyed. There is one fact connected with these cases, which is most significant; and which, whatever theory is adopted as to the organ of perception, must ever be regarded as the clue to the whole subject of paralysis and convulsion, as far as the brain is involved: we allude to the well-known circumstance that, in effusion and compression connected with one hemisphere, the paralysis is on the *opposite* side of the body. This is a clear demonstration that the decussating fibres already noticed, constitute, in reality, what physiologists contend they are, *isolated conductors;* for the mischief is traced precisely by following their remarkable and peculiar track. This fact, and the no less instructive cases of paralysis affecting special muscles of the orbit, further indicate that every affection of convulsion and palsy connected with the cranial nervous centres, however complex and apparently contradictory they may be, must be studied in strict reference to the course and destination of the primary fibrillæ. That the whole subject is still involved in

much difficulty we are most ready to admit; but at the same time we must reiterate our conviction, that this will never be removed till physiologists, by returning to the doctrine formerly received, namely, that the cerebral hemispheres are the exclusive seat of all consciousness, shall place their conclusions in harmony with the unquestionable results of pathology. In treating of convulsive affections, Mr. Solly quotes some extremely apposite remarks of Lallemande, to which, as bearing on the question just considered, we would call the attention of our readers (p. 554 *et seq.*)

Although we have ourselves no faith whatever in the truth of phrenology, it would be inconsistent with candour to withhold the author's arguments in favor of this popular doctrine.

"My reasons for believing that there must be a great deal of truth in phrenology are fourfold. First. I have received from practical phrenologists, and especially the late worthy Mr. Deville, such accurate characters of individuals known to me, but unknown to them, that I cannot believe the accounts I have received could be the result of accident and conjecture, which must have been the case if phrenology is untrue.

"Secondly. Phrenology alone—as it appears to me—can account for all the varieties of insanity, especially monomania.

"Thirdly. The facts which have been collected by the late Mr. Deville, showing that the brain will alter its form at any period of life.

"Fourthly. The existence of longitudinal commissures." *L. c.* p. 339.

We need scarcely remark that great importance is attached by phrenologists to the evidence derived from the investigation of insanity. Upon this most interesting subject Mr. Solly observes:—

"If phrenology is true, insanity on its first ingress is frequently not a disease of the whole brain, but of only a part of it. The first effect of inflammation is to excite to an unnatural degree the natural function of an organ. The function of the organ thus exalted obtains a mastery over the rest. For instance, a man, from defective education, combined with hereditary tendency, allows his love of approbation, his vanity, in other words, to grow with his growth, and strengthen with his strength, gradually becoming the sole ruling principle of life: at last it, instead of reason, so completely guides and regulates all his actions, that they are contrary to reason, and justly called the acts of a lunatic. Yet all this may go on with reasoning faculties so acute, that he conceals the dominant feeling of his breast, the mainspring of all his actions, and in a court of law defies any one to prove him insane.

"The great amelioration which has been effected in the condition of the lunatic has been founded on this principle, that none are so mad as to be incapable of appreciating kindness. Throughout all the admirable and interesting reports of Dr. Conolly, it will be seen that this has been the guiding principle of his boldly humane treatment. The first thing, says this admirable man, is to gain the *confidence* of your patient; and that once obtained, you may do *anything* with him.

"Now, if this is true, (and no one who has treated the insane on these principles doubts it,) so is it equally true that they may be awed by punishment and even acknowledge its justice. Only the last time I had the pleasure of visiting that noble asylum, Hanwell, I listened with much interest to a lunatic whom we met in the grounds. He began by requesting Dr. Conolly to procure his release from the Asylum, and then went on in a rambling manner, reasoning on things and circumstances which had no existence, showing his mental aberration; but he finished by saying, as an argument for his being allowed his liberty, *that he*

had always conducted himself with propriety while there, which was perfectly true. His sense of right and wrong was as perfect as ever, and this sense enabled him to conduct himself properly. But if he had supposed that the circumstance of his being lunatic gave him a license for any conduct, and freed him from all responsibility, would he have been so anxious to conduct himself properly? And if he were told that the law of the land would not take notice of an improper act, even if that act amounted to the murder of a fellow-creature, he would not feel the same reason for self-control."—P. 340.

The portion of Mr. Solly's work which treats of the diseases of the brain, is distinguished by a happy combination of practical observation with scientific research; and as, in addition, the views of the most eminent modern writers are given in their own words, a comprehensive and valuable resumé of cerebral pathology as it is cultivated in the present day, is presented to the reader. In a subject so comprehensive, we can only select one or two of the more general topics for consideration. The observations of the author on derangements of the circulation, especially on the various forms of anæmic affection, are very judicious, and well worthy the attention of the practitioner. Those only who have carefully investigated the complex texture of the brain and spinal cord with the aid of the microscope, are capable of forming a just conception of the predominating influence that must be exerted on the actions of these organs by disturbance of their capillary system. Impressed with this conviction Mr. Solly, in considering the cause of the symptoms characteristic of cerebral anæmia, dissents from the opinion of Dr. Burrowes, that this consists of diminished pressure; he thinks, and in this view we concur, that the mischief arises from the defective supply of blood in the capillary vessels.

"We know that the function of all other organs, uninfluenced by pressure, may be excited by a flow of blood into them, or their function may be arrested by any stoppage in their supplies. Take the salivary glands or the testicles, as an illustration: mental emotions will both excite and arrest their secretions; and I believe that the brain would be similarly affected, and to the same extent as now, even if that organ were not enclosed in a spherical box, and supported on all sides by the cerebro-spinal fluid."—*L. c.* p. 349.

In speaking of anæmic coma, the author enforces the necessity of well distinguishing between this affection and the coma arising from inflammation and effusion—a distinction of vast importance in practice, and for firmly establishing which, the profession is mainly indebted to that admirable observer, Dr. M. Hall. Although much attention has been paid to this class of affections, the following judicious observations of Dr. Gooch will not be here misplaced:—

"So inveterate is the disposition to attribute drowsiness in children to congestion of the brain, and to treat it so, that I have seen an infant, four months old, half dead from the diarrhœa produced by artificial food, and capable of being saved only by cordials, aromatics, and a breast of milk; but because it lay dozing on its nurse's lap, two leeches had been put on the temples, and this by a practitioner of more than average sense and knowledge. I took off the leeches, stopped the bleeding of the bites, and attempted nothing but to restrain the diarrhœa, and get in plenty of nature's nutriment, and as I succeeded in this the drowsiness went off, and the child

revived. If it could have reasoned and spoken, it would have told this practitioner how wrong he was; any one, who from long defect in the organs of nutrition is reduced so that he has neither flesh on his body nor blood in his veins, well knows what it is to lay down his head and doze away half the day without any congestion or inflammation of the brain. This error, although I have specified it only in a particular complaint of children, may be observed in our notions and treatment of other diseases, and at other periods of life. If a woman has a profuse hæmorrhage after delivery, she will probably have a distressing headache, with throbbing in the head, noises in the ears, a colourless complexion, and a quick, weak, often thrilling, pulse, all which symptoms are greatly increased by any exertion. I have seen this state treated in various ways, by small opiates, gentle aperients, and unstimulating nourishment, with no relief. I have seen blood taken away from the head, and it has afforded relief for a few hours, but then the headache, throbbing, and noises, have returned worse than ever; the truth is, that this is the acute state of what, in a minor degree and in a more chronic form, occurs in chlorosis, by which I mean pale-faced amenorrhœa, whether at puberty or in after-life. It may be called acute chlororis, and, like that disease, is best cured by steel, given at first in small doses, gradually increased, merely obviating constipation by aloetic aperients."—*L. c.* p. 367.

The necessity of employing the microscope in the investigations of morbid anatomy, is strongly advocated by the author; indeed, when it is recollected that the essential structure of every organ in the body—in fact *the* organ physiologically considered, is altogether hid from the naked eye; that the capillary vessels, constituting the active part of the vascular system—the cells of the various glands, forming the real apparatus of secretion—the grey corpuscles and primary tubules composing the instrument of innervation, are each and all of microscopic dimensions, what reasonable expectation can there be of detecting those molecular changes, which constitute the first and essential alterations in all organic disease, so long as the examination is made by the unaided senses and the very objects implicated are unseen? In some instances, indeed, the microscope will do little more than disclose the real seat of the disease; but in others it will also reveal important indications explanatory of the true character of the disease. One of these latter cases concerns that obscure affection, *ramollissement*, or softening of the brain, a subject on which the author has collected much valuable information. Our readers will recall the involved and contradictory opinions of writers as to the true nature of this affection; the long discussions whether it be, as Lallemande contends, inflammatory; or, as Rostan regards it, an affection entirely sui generis; or as Cruveilhier affirms it to be, capillary apoplexy, and so forth. In the midst of this confusion, Gluge had the great merit of discovering, with the aid of the microscope, a certain test of inflammatory action in many instances of cerebral softening, indicated by the presence in the affected part of exudation corpuscles, or, as they were termed by this writer, "compound inflammation globules." In some valuable papers published in the Edinburgh Medical and Surgical Journal, Dr. Hughes Bennett has shown, by a similar mode of investigation, that two kinds of ramollissement exist, an inflammatory and a non-inflammatory; that these are distinguishable microscopically by plainly recognisable characteristics; and that inflammation in the nervous centres has, by these means, been demonstrated in several instances after it had escaped the search of good

morbid anatomists, although indicated by the most unequivocal symptoms. Mr. Solly has added an interesting communication from Dr. Peacock, confirmatory of several of Dr. Bennett's researches, and from which we extract the following conclusions:—

"1st. That in all cases where characteristic symptoms of softening of the brain are present during life, evidences will be found, on microscopic examination, of the extravasation of lymph into the cerebral substance under one or other of the several forms of the so-called exudation granules, corpuscles, or masses:

"2ndly. That the appearance of portions of the brain softened after death, either artificially, by manipulation, or from *post-mortem* change, often, to the naked eye, so closely resembles the genuine results of disease as to render it extremely difficult, if not impossible, for practised morbid anatomists to decide between them: and

"3rdly. And consequently, that portions of brain, presenting every appearance of softening to the naked eye, but in which the microscope does not reveal the presence of some form of exudation, intermixed with the broken-up cerebral substance, cannot, in the present state of our knowledge, be regarded as having resulted from any diseased process during life." P. 388.

We are happy to find Mr. Solly advocating a principle for which we have always contended, namely, "that every decided deviation from the normal action of the brain is always found to correspond to some alteration of structure." The progress of minute observation, joined to a more just appreciation of the relations existing between the vital actions and the instruments by which they are produced, will doubtless explode the unphilosophical notion that such a thing as a pure functional disease can exist in any part of the body. Lest it should be supposed that this is a mere speculative question in which the practitioner has no concern, we would submit to our readers the following important observations, which coming from a scientific surgeon, who has enjoyed extensive opportunities at Hanwell and elsewhere, of inspecting the brains of lunatics, are deserving of the most serious attention.

"I have long felt convinced," remarks Mr. Solly, "that much of the obscurity which envelopes these diseases and those of other parts of the brain, might be removed by comparing them with diseases of the eye; viewing them through the light which the observation of this interesting class of affections affords us. I do not refer so much to acute disease as chronic, though both are useful as instructors. One great reason why these affections of the eye ought to guide us in our treatment and prognosis of inflammation, both chronic and acute in other organs, is the facility with which we can observe the action of remedies, medicines, topical applications, general stimulants, and diet, upon an organ so open to observation. I believe that every form of mental derangement is dependent on some change, though often very slight and temporary, of the vital condition of the hemispherical ganglion.

"I am convinced that the reason why physicians, to whom the treatment of the insane has been entrusted, believe in the existence of mental disease unattended with disease of the instrument which the mind employs in its communications with the world, is because the medicine, both constitutional and local, has so little control over these diseases, and the great good to be derived from moral treatment. A knowledge of the treatment of diseases of the eye would teach them a different lesson. Let any man ignorant of the treatment of ophthalmic diseases attempt the cure of a case of strumous opthalmia; he would, in all probability, seeing the red, inflamed conjunctiva, the pain suffered by the patient, and the distress occasioned by the

presence of light, employ all the most approved antiphlogistic measures. He would bleed from the arm, purge violently, and then possibly put his patient under the influence of mercury. What would be the consequence? Why, most assuredly the loss of the eye, total blindness. And the same sad results followed the treatment of insanity when it was considered to be an inflammation of the brain, except in very acute cases occurring in subjects with much constitutional power; injudicous treatment being attended in the one case with the loss of sight, in the other with the loss of intellect. But suppose a judicious surgeon, one bred in the school of Farre, Travers, Lawrence, and my late respected colleague, Frederic Tyrrell, called upon to treat this strumous inflammation of the eye. He would support his patient's general health with a tonic plan of treatment; he would improve the condition of the circulating fluid and the instruments which circulate it. He would endeavour to arrest local inflammation by small local blood-lettings, counter-irritants, and astringent lotions, by removing him from all those atmospheric influences and moral circumstances which would stimulate the organ. And thus he might *ultimately* succeed; but what care, what patience, and what confidence in the remedial agents employed, does it require on the part of the surgeon who treats these cases, to effect a cure!

"If, then, it is so difficult to subdue an inflammation in an organ, the actual condition of whose blood-vessels we can view, to which we can actually apply local remedies, and from which we can withdraw the injurious agents which have produced this inflammation, and exclude the natural stimulus of the organ, and see, in the whole course of our remedial measures, the progress or the failure of each particular plan of treatment, is it astonishing that men should have failed so much in the treatment of chronic and strumous inflammation of the arachnoid, pia mater, and hemispherical ganglion, when they have all these difficulties to contend with, and want many of the adjuncts?" P. 478.

We very much regret that our limits will not allow us to notice the valuable sections treating on apoplexy and convulsive affections; they are eminently of a practical character and merit the careful perusal of every surgeon and physician. For the same reason we can only briefly refer to the interesting discussion of the author on the nature and treatment of that most intractable affection, epilepsy. As to the seat of the disease, Mr. Solly dissents from the doctrine of Dr. M. Hall, that it is the true spinal cord; being convinced that "the brain and not the spinal cord, is primarily affected." With respect to the pathology of epilepsy, we have the following theory:—

"The first morbid action is a sudden determination of blood to the brain, which expends itself, in the secretion of that nervous power which, in a state of health, is employed by the brain to convey volition to the muscles, and which power is, I have no doubt, identical with electrictity. This excessive secretion is carried off by the motor nerves, like a discharge from an electric battery, and, from its quantity and excess, produces excessive action of the muscles. It is another illustration of a law that we had occasion to decide upon already, namely, that the first effect of arterial excitement in every secreting organ is to excite to an unnatural degree the natural function of the organ. We know that mental emotion will cause a sudden determination of blood to other organs, which according to the nature of the part, will be followed or not by secretion.

"Blushing and erection of the penis are instances of sudden determination of blood to a particular part. And the lachrymal glands, salivary glands, testicles, prostate gland, gastric glands, and even the kidneys, often pour forth their secretions so abundantly and so suddenly that the formative fluid, the blood, must have circulated through their capillaries in greater quantity and with greater

rapidity than when the glands were at rest, and their secretions suspended. I think that the periodic attacks of mania, with which many of the insane are afflicted, may be regarded in this light." P. 591.

In connexion with this point, the author thus explains his views as to the condition of the blood-vessels in congestion :—

"The expression 'determination of blood to the head' is often made use of, but without any explanation of the manner in which this takes place. I doubt whether the profession generally have any distinct idea as to the exact condition of the vascular system which produces it. I would venture to offer the following theory, the first idea of which I certainly derived many years ago from that most truly philosophical work, the Elements of Physics, of Dr. Arnott. It applies not merely to the head, but everywhere else. The middle or muscular coat of the arteries in a state of health contracts with each systole of the ventricles just sufficiently to give a solidity to the wall of the pipe, so that the force of the contraction is not lost on a yielding surface. A much greater force is required to drive water through a leather hose than through a leaden tube. The middle coat contracts just sufficient to assimilate the artery physically and temporarily to the *leaden* tube. Arteries with permanently rigid walls, like leaden tubes, would have interfered by their rigidity with the motions of the limbs; and hence this beautiful contrivance. When this middle coat does not contract, or only contracts imperfectly, then the force of the heart dilates the tubes, and produces congestion.

"I believe, then, that determination of blood to the head arises simply from deficient contraction of the muscular coat of the capillaries of the brain, preceded by excitement of the heart's action." P. 592.

The reader will understand from these extracts, that the author regards the essential cause of epilepsy to be a congestion of the arteries and capillaries of the brain, the venous turgescence, which has been noticed by so many writers, being merely the result of the previous arterial accumulation. (P. 595.) In another part of the work, the resemblance of that troublesome affection commonly called 'catchings of the limbs' to epilepsy is pointed out; the cause is thus stated :—

"Worry in business, mental anxiety, and vexation of spirit, will sometimes bring on spasmodic action of the muscles, and paralysis. In some cases the anxiety and mental irritation induces disease in the hemispherical ganglion, seriously affecting the temper, but not affecting the intellect. Such cases are familiar to all practical men, but it is very difficult to explain their pathology. I suppose that the disease or diseased action excites unnaturally the tubular neurine, which, commencing in this ganglion as the motor tract, conducts the will to the muscles; and the consequence of this excitement is an irregular supply of stimulus to the muscular system exhibited by the twitchings and spasms. This irregular action the mind can more or less control and arrest when awake; but as soon as sleep takes place, then the spasms commence. I suspect that epilepsy is a form of this irregular innervation, only that in epilepsy the nervous or electric fluid *accumulates* in undue quantity, and passes off in a large quantity at once, like the discharge of an electric battery. In many cases of epilepsy, the discharge takes place in small quantities both before and after the complete fit. I have two patients under my care now who suffer seriously in this way; one, a single man, has always warning of the advent of the fit by twitchings of the right leg as soon as he drops off to sleep; the other, a married man, has these twitchings so constantly in bed, that his wife is often kept awake during a whole night. In the non-epileptic cases, though electric fluid is secreted in undue quantities, still it does not accumulate, so as to produce a complete convulsive fit, but is constantly oozing out." P. 444.

The fact here noticed, that the twitchings usually come on as soon as the person falls asleep, is a strong corroboration of the theory, which attributes all convulsive actions to the true spinal system. We are acquainted with a family, several of the members of which are peculiarly subject to these "catchings," and in all they principally occur during sleep.

But to return from this digression. Mr. Solly subsequently adduces several arguments, to prove that the brain is in a state of congestion during the fit: thus he has always seen a flushing of the face *previous* to the convulsive paroxysm, "previous, as he believes, to the discharge of the electric fluid in those epileptics who were full-blooded and plethoric;" then the amazing benefit he has seen from the sedative influence of digitalis, which medicine is most serviceable when it keeps down the pulse below the standard of health, is another corroboration of the views stated above. Dr. Conolly has also observed that epileptic patients are occasionally warned of the approach of the paroxysm by mental excitement, owing, as Mr. Solly infers, to increased arterial action. Other patients have, as a warning, a singing in the ears, arising it is thought from the dilated carotid vibrating in its osseous canal, like an enlarged artery going to an inflamed part. The evidence of morbid anatomy confirms the idea of this congested state of the blood-vessels, for "in all cases of fatal epilepsy, where there has been an autopsy, the vessels of the brain and membranes have been found enormously distended, and in some there has been extravasation."

"The *Œnanthe crocata*, or hemlock water drop wort, when taken in any quantity, produces epileptic convulsions. I was present at the *post mortem* examination of four convicts, who died at Woolwich from eating it. The progressive amount of sanguineous effusion on the brain was in proportion to the length of time they survived. The seizure was most striking and instructive.

"In all there was great congestion and some sanguineous effusion on the surface of the brain; in those that lived the longest, the quantity was in proportion to the length of time they survived the seizure. The first man died in about an hour, and the last in about two hours." P. 600.

Among the remote causes of epilepsy it is well to recollect that syphilitic irritation of the dura mater, a fact first pointed out to the author by Mr. Copeland, is to be enumerated.

As regards the treatment of these obstinate affections, Mr. Solly has found the oxide of silver and the continued use of digitalis most efficient as medicines; the latter being adapted to young and excitable subjects, in whom it produces the best results, and the former to older patients, where the disease is more confirmed and the fits do not occur so frequently. In order to tranquillize the stomach when the digitalis is administered, creosote or hydrocyanic acid should be given; the digestive organs being generally in fault, will of course require attention. Notwithstanding the conviction the author entertains of the congested state of the cerebral vessels, he does not advocate blood-letting, either general or local.

"I never saw any good derived from blood-letting, and I have seen a *great deal of harm* from it. I bled freely in one or two cases some years ago, under the impression that the disease was inflammatory, when there was a decidedly plethoric state of the system and great congestion of the brain; but I am convinced it caused a repetition of the attacks. Even the application of leeches, either before the attack, at the time, or afterwards, only does harm." P. 615.

There are of course some exceptional cases where depletion may be beneficial; when, for example, there is hypertrophy of the left ventricle, a not unfrequent cause of epilepsy, a few leeches applied to the cardiac region will be found useful, especially if combined with hydrocyanic acid, which is of great value in subduing irritability of the heart. Foville also recommends the periodical application of leeches to the arms, in plethoric individuals, with large heads, habitually injected with blood.

We must here conclude our notice of this admirable treatise; and, in doing so, we earnestly advise all our professional brethren to enrich their libraries with a work which bears the stamp of extensive observation and careful research; and which has in addition this peculiar advantage, that it combines, in a moderate compass, a scientific and practical account of the anatomy, physiology, and pathology of the brain.

Ricerche ed Esperienze sul Sangue Umano. Del Dottore *Gio. Polli.* Serie I. II. III. ed. IV. (Annali Universali, Vol. 106, 109, 113, 121, ed. 122. 1843—47.)

Researches and Experiments upon the Human Blood. By Dr. *Polli.*

Dr. Polli having been much struck, during his studentship at Pavia several years since, with the perusal of the account of some of Hewson's Experiments upon the Blood, resolved entering himself upon a course of experimental inquiry respecting certain points needing illustration; and he has, from time to time, communicated to the Annali the valuable series of papers detailing their results and the therapeutical conclusions derivable from these, an analysis of which we now propose laying before our readers.

The *First Series* has for its title

Researches and Experiments upon the Formation of the Buffy Coat of the Blood, and its Symptomatic Value in Disease.

After adverting to the contradictory accounts which were formerly furnished of the appearance of the blood in disease, and the rarity and uncertainty with which practitioners employed these as indications of treatment, he observes that the chemical physicians of our own times have effectually revived attention to the subject, and led to the belief that at least as much information may be obtained from the inspection of the blood as from post-mortem examinations. He then continues:—

"The true pathological anatomy of the blood is its analysis, since we can only divide the fluid by means of re-agents. But every hand is not able to wield such a scalpel as the re-agent. This requires a longer and more severe course of study than is necessary for the acquisition of anatomical knowledge. But, without pretending that every practitioner should know as well how to analyse th-

blood as to examine a body, it is certainly to be desired that he should be enabled to read, in the physical appearance of the blood he removes from a patient, those constant characters, that more or less clearly reveal its intimate composition, and to which they have been shown to bear a necessary relation. The labours of Prout, Denis, Lecanu, Andral, and Gavarret have revealed to us so many facts that, although the pathological doctrine of the blood is not quite complete, the morbid anatomy of this fluid portion of the organism stands upon an equality with that of its solid tissues."—Vol. 106, p. 76.

1. *On the Influence exerted by the Coagulability of the Blood in the production of the Buffy Coat.*—Every one knows that blood drawn from the arm separates in very different periods of time into clot and serum; and the question here agitated is, what effect has its more speedy or tardy coagulation in the induction of the buffy coat. Some experiments of Hewson upon this subject, as we have already stated, attracted the author's attention at an early period; and these, with some observations he had the opportunity of making in the hospital, led him to the opinion that a slow coagulation of the blood was highly favourable to the production of the buffy coat. So many chemists of celebrity however support, and so many oppose this opinion, that he deemed it an excellent occasion for instituting a series of experiments, which, from their number and the care with which they were conducted, would be in a position to secure acquiescence in the results they indicated.

It is obvious, in a question of time of this sort, unless great care be taken in ascertaining the precise period of coagulation, great errors may easily occur. The following is Signor Polli's criterion:—

"The blood was caught in a conical glass vessel holding about an ounce, and which when nearly filled was left in a state of complete rest. Watch in hand, the instant of complete coagulation was carefully observed. The criterion of this was the witnessing *a few drops of serum transude at the surface of the consolidated coagulum, or between the edge of the crassamentum and the parietes of the recipient.* The point at which the blood ceases to be fluid, and solidifies, so that the recipient may be turned upside down without pouring it out, did not seem to me to be so true a point for determining the existence of complete coagulation, as that derived from the transudation of a few drops of the serum, which is to remain permanently fluid, through some portion of the consolidated mass. This fluid, if collected and placed in a watch-glass, remains constantly fluid, undergoing no manner of coagulation. As long as the serum continues imprisoned within the tissue of the clot, rendered more or less dense by its commencing coagulation, we cannot say that such coagulation has taken place; but when the serum is beginning to separate itself from that which was only maintained fluid by the vital influence, and which has now passed into the solid state, the fact is decided."—Vol. 106, p. 86.

The "fixed point" here indicated is of unquestionable importance, since without the aid of some such test of complete coagulation, different observers will infer this to have taken place at very different periods. Dr. Polli adds, that it very seldom happens that the serum does not then show itself; but still occasionally the coagulum contracts adhesion with the recipitent sufficiently firmly to prevent its passage. By gently moving the edge of the coagulum by means of a feather, or even inclining the vessel on one side, the serum is rendered apparent. Blood so observed has led the author to lay down the three following propositions:—

(A.) *That blood forming a buffy coat takes a much longer space of time to coagulate than does blood which forms no such coat.*—The blood examined in 405 cases of disease, was found to have required the mean period of 27 min. 30 sec., when it produced the buffy coat, and but 11 m. 50 s. when no buffy coat presented itself—that is to say, a period much less than a half in the one case than in the other. The time required for blood to coagulate, when this was buffy, was found to be nearly twice as long in the case of men (41 m.) as in that of women (23 m.); and that this last generally takes a notably longer time to coagulate than does that of children (18 m.) The cases were taken as they presented themselves without selection in the hospital.

(B.) *Blood which of itself would not produce the buffy coat, presents this whenever its coagulation can be sufficiently delayed.*—Alkaline solutions, employed not in sufficiently strong proportions to maintain the permanent fluidity of the blood, possess the power of retarding its coagulation; and in twenty experiments here referred to, portions of blood, which otherwise gave no buff, were in this way delayed in their coagulation for some hours with the effect of inducing a buffy coat. In like manner, normal blood may be made to produce this by delaying its coagulation through the prevention of the contact of air by a layer of oil or carbonic acid gas.

(C.) *Blood disposed to produce a buffy coat may be prevented doing so by sufficiently accelerating its coagulation.*—An experiment repeatedly performed by the author conclusively settles this point. Let blood from the arm of a patient suffering from inflammation be caught in two separate vessels, one of which is to be put aside at once, and the contents of the other previously stirred for a minute or two by the finger or a glass rod, or the vessel so shaken as to keep them in motion for that time. It is then to be set aside also. The first portion will complete its coagulation in 25, 30, or more minutes, giving a more or less thick buffy coat, but that which has been agitated will coagulate in ten minutes, presenting no buffiness whatever: so that persons who have been shown the two coagula could hardly credit they were produced from the same blood, being disposed to consider the one a specimen of morbid, the other of normal blood. Other means of accelerating coagulation, such as adding water to the blood as it flows from the vein, or maintaining it at the same temperature as it had held in the body, produce the same result. The same thing may indeed be observed oftentimes naturally as well as artificially. Thus, during a venesection, the first blood abstracted coagulates slowly and buffs, while the latter portion coagulates far more rapidly and without the buffy coat. So, too, if we abstract blood from both arms, one of which has been maintained turgid by the application of the fillet some time before, and which has only been applied to the other just before opening the vein; in the former case the blood coagulates rapidly without buff: in the latter, slowly and with the amount of buff usually observed in the particular disease.

2. *Of the Accidental Circumstances which are capable of modifying the Appearance of the Buffy Coat of the Blood.*—The examination of these may lead us to the explanation of the various apparent exceptions to the law laid down. The circumstances which may give rise to variations in the manifestation of the effects dependent upon the coagulation of the

blood may be divided into such as are *intrinsic* and such as are *extrinsic* to the blood. The first are the *density* and *temperature* of the blood when it quits the vein: and the second embrace the *contact of the atmospheric air*, the *nature of the recipient*, the *temperature of the air*, the *agitation the blood is submitted to*, and the *rapidity with which it flows*.

(1.) *Density of the Blood.*—For the estimation of the density of the blood Dr. Polli employs an areometer which is divided into ten degrees an inch apart, each degree being subdivided into ten parts. The density of the blood, in reference to its buffy or non-buffy state, was examined in 180 instances with the following general results:—

"That a *lesser density* accompanies *buffy blood*, as a greater density is associated with *unbuffed blood;* since, taking it generally, the density of the buffed is to that of the unbuffed blood as 5·716 to 6·911. Moreover, the density of the blood of women, taken in general, and without taking the buffy condition into account, is less than that of males as 6·142 to 6·575. The coagulation of the blood, also *considered without reference to the production of buffiness*, likewise takes place more readily in women than in men, requiring for the former a mean time 17 m. 34 s.: for the latter 22 m. 44 s."—Vol. 106. p. 116.

Comparative experiments showed that, while the mean density of buffy blood is more than a degree of the areometer less than that of the unbuffy blood, the density of the serum of the same bloods differed only $\frac{1}{11}$ of a degree; so that it may be affirmed that, "the influence of the density of the serum in the production of the buffy coat is very feeble, the principal modification in the characters of the blood depending upon its coagulation being chiefly due to the different density of the coagulable components of the blood, or to the quantity of fibrine held in the fluid state during the interval which precedes coagulation."

Some important practical results may also be deduced from these experiments upon the density of the blood. Thus: *successive abstraction of blood exert an extraordinary influence in diminishing its density.* From two tables which are given, we deduce that successive depletion will reduce the density of the blood more than one half; viz. from 7°·5 to 3°·5, while that of the serum is reduced only from 4° to 2°·5; whence the corollary, that "*bleeding may be considered as exerting a far more evacuant effect upon the coagulable mass held in a fluid state in blood just drawn*, i. e. *the fibrine, than upon the materials held dissolved in the serum even after the coagulation*, i. e. *the albumen.*" A phenomenon of familiar observation, viz. that the blood at the commencement of inflammatory diseases may present little or no buff, but that in future bleedings this may become highly developed—is explained by the *diminution of the density of the fluid by successive venesections.* The effect may not only be observed in successive bleedings, but *at different periods of the same bleeding*, by examining the respective density of portions of the fluid at the beginning and end of a bleeding. Taking the mean of 40 experiments, the first portion possessed a density of 6·127 and the last of 5·96.

Another series of experiments clearly proved that the blood is rendered more dense by inducing an artificial stagnation or engorgement by means of the fillet for some time previously to opening the vein. "*Venous blood then, when it is prevented by any obstacle from circulating freely in its*

veins, becomes more dense, or loses a certain portion of its watery principle. Does not this fact constitute a principal pivot of the doctrine which explains œdemas, cellular dropsies, and other infiltrations, by considering them as exudations induced by obstructed circulation ?"

The density of the blood varies as regards *sex* and *age*. It is greater in the blood of men than in that of women, a mean of 90 examinations of each giving 6·437 to 6·170. In adults the density is generally greater than in children. In *Summer*, also, it is more dense than in Spring and Winter, owing to the more abundant transpiration which then takes place. The author has also many times observed a sudden and unusual increase in the density of the blood of certain patients who have been subjected to profuse sweatings or abundant evacuations.

(2.) *Temperature.*—The coagulation of the blood being one of the first of the metamorphoses prior to decomposition which the blood undergoes, heat tends to accelerate and cold to postpone it.

" This fact, together with the no less important one of the lesser density of the blood during spring-time, explains to a certain extent the frequency and abundance with which blood becomes buffed at that season; for, without considering the severity of the visceral phlegmasiæ which prevail in the Spring as compared with the Summer, *the slow coagulation of the blood* favoured by the colder temperature and by its own lesser density, may, at the very least, *cause an exaggeration in the appearance of the buffiness*, which merits the attention of the physician, so that he may be enabled to assign their just value to the symptomatic phenomena furnished by the blood in disease."—Vol 106, p. 137.

(3.) *Contact of the Atmospheric Air.*—A number of ingenious experiments are related in proof, that the contact of the atmosphere favours the coagulation of the blood, just as whatever diminishes or prevents such contact retards this phenomenon. Dr. P. objects to the experiments and conclusions of Magendie, Magnus, and others, upon the influence of various gases in inducing coagulation, inasmuch as the mode of conducting the former do not allow of faith being attached to the latter. He describes at some length the precautions he himself took to secure more exact results, and concludes that *nitrogen* and *oxygen*, separately examined, do not seem to promote the coagulation of the blood more than does the air itself; indeed, if there is any slight difference, they seem to retard it; and that *carbonic acid gas* evidently retards the coagulation, so as to induce the formation of the buffy coat more easily than in the normal state. The oxygen, as well as the azote and atmospheric air, after having reacted on the blood which coagulated under their influence, always presented themselves loaded with carbonic acid gas, proceeding doubtless from the blood in which it had been held dissolved.

" *The blood, therefore, coagulates more readily when in contact with the air and its gases than when deprived of their influence, because it yields to them more readily its carbonic acid gas: and it coagulates more tardily in carbonic acid gas, because, besides not being able to emit that which it contains, it dissolves another portion of this gas, which it cannot be doubted has the effect of still longer maintaining it in the fluid state.*"

(4.) *Nature of the Recipient.*—Comparative experiments have shown that coagulation takes place more readily in glass vessels than in tin, and

much later in leaden ones. The size of the vessels, and the contact of other solid bodies, likewise exert great influence.

(5.) *Agitation.*—This, if carried to any extent, as by stirring the blood with a bundle of rods, will prevent its coagulation by depriving it of all its fibrine: but if the blood be stirred or shaken for a few minutes, and then left at rest, coagulation is only hastened. The different capacity of the vessel, or rather the different amount of the mass of the blood collected together, has, as well as the different mode it flows from the arm, great influence in determining the buffy appearance. A series of experiments showed that a small portion of blood was always longer in coagulating and more often buffy, while a large quantity coagulated quickly without buff; and this circumstance is explained by the amount of agitation kept up during the longer time the larger portion takes to flow.

"So that the mere circumstance of collecting much blood in one recipient, for the reason stated, is the cause of its coagulating more quickly, and hence hindering the appearance of buffiness which a small portion would have afforded. A quantity of blood drawn at a time may likewise mask in another manner the natural aspect of the blood. For the last portion drawn possessing a very different coagulability to the first, and coagulating generally much quicker, covers with a stratum possessed of little or no buffiness the lower strata, which of themselves would furnish much more buff, and this imparts to the blood a mendacious appearance. These facts putting us on our guard respecting the absence or slight quantity of buff in blood received during a copious venesection into only one vessel, show us the necessity *of always receiving a small portion separately when we are desirous of drawing any indications from its appearance with confidence.*"—Vol. 106, p. 160.

3. "*Of the Causes of the Coagulation of the Blood, and of its slow or rapid Coagulation in Diseases.*

"From the preceding observations we may infer that the appearance or non-appearance of buffiness of the blood is a phenomenon chiefly dependent upon and expressed by the time employed in its coagulation, notwithstanding that some other circumstances may secondarily induce certain modifications; and the object is now to inquire what is the cause of the ready or tardy coagulation of the blood when removed from the living economy, as this alone gives value to the presence or absence of the buff, when we wish to draw conclusions from the aspect of the blood in disease."

All circumstances which tend to increase physiologically or morbidly the activity of the animal functions lead to a slower coagulation of the fibrine of blood removed from the body; and all the circumstances producing a diminution of vital power lead to its more ready consolidation. This is seen in the more ready coagulation of the blood of women than that of men—the diminution of the power of the fibrine to hold itself in the fluid state in proportion to the amount of blood taken, and to the lateness of the period of the bleeding during which it is examined—the prompt coagulation which takes place on the induction of syncope and of general or partial obstruction of the circulation—and the greater readiness with which it may be induced in the blood of small and weak animals than in large and strong ones. Whence we may conclude—

"1. That the consolidation of blood drawn from a living animal is a phenome-

non which announces its want of vitality, and is the expression of its passage from the condition of a living to that of a dead body. 2. As the longer maintenance of the fluid state of the blood always coincides with the circumstances of the energetic vitality of the animal as represented by the masculine sex, the free action of the vessels, robust constitution, phlogistic excitement, &c.; and its ready coagulation with circumstances of weakness from sex, age, repeated depletion, exhaustion of the nervous power, &c.: so the *time during which the blood remains fluid may be taken as a measure of the remaining vital activity, and the celerity with which it coagulates as the expression of its feeble resistance to decomposition.*" Vol. 106, p. 261.

Signor Polli enters into some interesting speculations, into which we cannot follow him, upon the amount of vitality remaining in the drawn blood preventing its coagulation, and the nature of this last process, which he regards as resulting from a species of *spontaneous crystallization* of the fibrine held by the blood in solution. After examining into various opinions upon the *causes of the coagulation*, he thus states the result of his own investigations upon the effect of *carbonic acid* in inducing it:—

"1. That although the blood owes its primary tendency to coagulation to a cause as yet unknown, the variable quantity of carbonic acid gas existing in it seems the cause of its variable coagulability in different physiological or morbid circumstances, and therefore one of the principal agents in determining buffiness. 2. That in respect to the symptomatic value of the buff, it may be laid down that a light buffy coat, without frothiness, placed upon a coagulum of very deep colour, not only indicates a slow coagulation of the blood, but also the accumulation of a large quantity of carbonic acid gas in it; while when a rose or scarlet coloured layer, almost always frothy, presents itself upon a not very black coagulum, it announces that such blood is loaded with a large quantity of oxygen." P. 285.

4. The *Fourth Section* being occupied in a critical examination of the opinions of Andral, Gavarret, Giaccomini, and Mandl, upon the formation of the buffy coat, we pass it over, as the space we have at command barely suffices for the exposition of the views entertained by our author himself.

5. *On the Signification of the various Appearances of the Coagulum in relation to its Density, Coagulability, and the Proportion of its Component Parts.*

The examination of blood 12 or 24 hours after its extraction may furnish valuable indications to the physician who may not have had the opportunity of observing it at an earlier period.

"The blood is *plethoric*, or with a prevalence of red globules, when it presents a voluminous crassamentum of a rather bright red colour, soft, with little separation of serum, and which at most lets fall to the bottom of the vessel a certain quantity of separated globules, which form a stratum of rather deeper colour. The abundance of the globules is such, that the fibrine can not only not compress its lamellæ so as to acquire tenacity, but it is unable to comprise the whole of the globules, and lets a portion fall form the coagulum, which, by their weight, always gravitate to the bottom of the vessel. In this case the blood is never buffy, but is covered with a red foam, or by a border of a reddish colour. But the blood may be plethoric and also present the (so-called) buffy coat, which then has a whitish or greenish white colour, and a gelatinous consistency, the clot being of a deep red-colour, having a layer of coloured globules lying at the bottom of the vessel.

"The blood is *anæmic* and poor in globules when it presents a small clot of a light red colour, mostly hollowed above and swollen out below. It is submerged and sometimes floats in a large quantity of serum, which may be quite transparent, but which is oftener rendered somewhat turbid by a little of the suspended colouring matter. The clot is tenacious and resistent because the fibrine, existing in disproportionate quantities to the other principles, has not its interstices filled with globules, so that its fibres can better approach each other and display a greater power of aggregation. And it is frequently swimming in the serum, because not loaded with globules, it assumes the position which comports with the specific gravity of the fibrine. The serum is reddish from its water dissolving a portion of the colouring matter." P. 305.

Dr. Polli enters at considerable length into the description of the various kinds of fibrinous crusts which are formed upon coagulated blood, and the indications these furnish of slow or rapid coagulation, &c., but we have not space to follow him in this.

Series II.—On a New Criterion for the Regulation of Bloodletting.

This criterion is deduced from the author's former series of researches and confirmed by practical application in the Hospital. It is thus stated to be "*the period coagulation of the blood observed at different intervals of time between the abstractions, and in different portions of the mass taken during one blood-letting;*" and is amplified as follows:—

"1. Every time a large abstraction of blood is practised, so as to lead to lipothymia, *the last portion* of that removed always coagulates with *greatest promptitude*, whatever may have been the time occupied by the first portion in coagulating. 2. Whenever, on the contrary, upon a person suffering from sanguineous congestion of the nervous centres, asphyxia, apoplexy, &c., bleeding is practised, and, by its use, the vital functions are again set at liberty, the last portion of the blood so removed, coagulates *much more slowly* than that which was first emitted. 3. That it suffices to interrupt in some manner the course of the blood in the vein, or to diminish, by means of a ligature applied to an extremity, the irradiation of the nervous power, in order to secure the speedy coagulation of blood, which, a short time after, owing to the obstacles being removed, may re-acquire the power of remaining long without coagulation. 4. That in diseases decidedly of an inflammatory and grave character, during which, for the safety of the patient, repeated bloodletting is requisite, if, on the occasion of every venesection, the coagulation of the first and last portions drawn be examined: it will be found that at the beginning the coagulation of the latter portion takes place *subsequently* to the former, and continues to do so in an equal ratio to the development of the morbid process, until this reaches its height. From this point, however, as the disease commences declining, the coagulation of the blood of the latter portions *precedes* that of the former. 5. That in the cases in which abstraction of blood has been desisted from for some days, when the slow coagulation of *the last portion* taken announced a continuance of the phlogistic increment and the tolerance of bloodletting, it has become necessary to resort anew to this therapeutical agent, which can in no case be laid aside with the ready cure of the patient, unless the latter portion of the blood manifests an opposite disposition to that now pointed out. 6. That in opposite cases in which the abstraction of blood is persisted in, *notwithstanding its rapid coagulation* after all the venesections and during the two extreme periods of the same case, it has to

be speedily renounced in consequence of the symptoms of intolerance which manifest themselves; and in those few unfortunate cases in which bloodletting is obstinately persevered in under the guidance of fallacious symptoms, vital exhaustion cuts short the career of the patient much more rapidly than would have done the course of the disease.

"It results then from these observations that the maintenance of the fluid state of the blood, comparing one bleeding with another, or different periods of the same bleeding, is a measure of the vital energy proper to the individual, and of that brought into play by the morbid process; and that hence may be determined tolerance and indication of blood-letting; as on the other hand a prompt coagulation of the blood announces diminution of vital energy, or its exhaustion by the pathological action; and in every case that the power governing the phlogistic or morbid vital movements is lowered." Vol. 109, p. 65.

The criterion is of easy application, the first and last portions of blood drawn being separately collected in glass vessels, and placed at rest beyond the influence of disturbing causes before adverted to. As the difference of time employed by the blood in coagulating depends both upon the condition of the individual and the amount of blood drawn, the criterion in question may not only serve as a guide in judging of the propriety of bleeding in a certain contingency, but may determine the exact quantity to be drawn, and the period of its repetition.

"Let an individual be bled to faintness, and you will always have the last portion of the blood rapidly coagulated, and consequently deprived of buffiness. Receive the blood into six, eight, or ten small recipients, of a similar form and nature, and the coagulation in the first will be in exact relation with the disposition of the fibrine to maintain itself in the liquid form proportionately to the particular physiological or morbid state of the organism; while in the last, such disposition will become gradually paralysed and almost destroyed from the gradually increasing effect of the abstraction itself. By contemplating this phenomenon, which is always a result bearing proportion to the two influences above alluded to, we are enabled to lay down a rule for in some cases practising abundant bloodletting at one time, in others practising it at intervals, or in small quantities; or again simply interrupting its flow once or twice for some minutes during the abstraction, &c.—accordingly as we may be desirous of obtaining a sudden subdual of the morbid exuberance of the vascular activity, or of securing a copious depletion without too great exhaustion of the strength, or the functional disturbance ensuing upon lipothymia, which may injuriously affect the regular course of some affections.

"From the different coagulation of the various portions of blood we may, moreover, as we have said, measure the *intensity of the inflammation* and the *tolerance of the individual;* or, as others would express it, we may measure the morbid capacity and the amount of diathesis. There may, indeed, be a case in which the first portion of blood drawn indicates by its very slow coagulation a very high pathological condition, while the last portion announces in its rapid coagulation that the emission of blood has completely lowered the powers. This phenomenon may be dependent upon the existence of a very circumscribed, though a very intense affection, or upon exhaustion induced in an individual primarily possessed of very feeble powers of organic re-action; and in such a case bleeding must be most reservedly employed, and frequently entirely rejected, for the reason that it is a lesser danger to leave the disease to proceed unchecked, than to have recourse to means which remove it and the patient together. This difficult pathological circumstance, which a celebrated Italian physician justly compares to an *island of fire in a sea of ice*, is already known to practitioners as one which requires in the use of antiphlogistic measures great regard to be paid to

the failure of the general strength. But, unfortunately, it has not always been easy to establish its diagnosis in time, or before unadvisedly energetic therapeutical procedures have been put in force. But the criterion I propose informs us of these two opposite conditions co-existing in the same individuals, and measures their degree with a facility and security that no method of investigation hitherto recommended in these difficult cases can boast of." Vol. 109, p. 70.

To the objection that the criterion only comes into operation after the abstraction of blood, Dr. Polli observes that, in ordinary inflammatory diseases, the repetition of the bloodletting is the point to be enquired into; and that, even in those rarer cases in which the diagnosis is very obscure, and in which a first bleeding might prove the cause of safety or of death, no harm whatever, and much good, would result from a very small *exploratory* venesection, and made in the view of obtaining the desired information. Such, consisting of one or two ounces, received in two separate vessels, should be instituted in all obscure cases of this kind, before resorting to an ordinary venesection. "Perhaps even those small bleedings should be practised in all diseases indistinctly as a means of exploring the condition of the blood, for the same reason that, since auscultation has been employed upon all patients, it has not unfrequently revealed latent morbid conditions, to which the attention of the practitioner might otherwise not have been called until a more remote and a too late period." A small subtraction which can do no harm to the economy, will yet depict to us the true characters of the vital condition of the tissues, and of the amount of the exaltation of the vascular activity and nervous function. It often suffices for the discovery of those circumscribed and concealed phlegmasiæ, which, frequently not spreading to such organs as would furnish external symptoms of their existence, pursue their undermining course until they have reduced the viscera they affect to such a condition, that some acute contingency at last suddenly betrays their formidable character.

"Although the preservation of its fluidity by the blood, or the more or less time it requires for coagulation, constitutes for us the most certain measure of the activity of the phlogistic force, this is, however, only durable in its indications in proportion to the stability of the morbid process itself. The phlogosis may, during its course, spontaneously increase or diminish in intensity, accordingly as it extends to neighbouring tissues, or is confined to those first invaded. So that the different resistance of the blood to coagulation which in every case announces with a rare exactitude the present state, cannot be extended, except within certain limits, to the indication of that which is to follow; since this latter can only be the complex and simultaneous effect of the condition of development of the pathological lesion, and of the modification which the bloodletting itself may have induced. Our criterion, as expressing the present state of the organism, and the impression which the bleeding has developed, furnishes indications which are available for about twelve hours after, and may continue to be so for a much longer period, even to the supervention of complete health, providing new morbid causes and accidental inflammations do not supervene and complicate the course of the disease. And of this we may assure ourselves by the repeated observation of the coagulation of blood taken at brief intervals; since the times employed in the coagulation of the blood taken at the successive abstractions will generally glide, whether these are diminishing or increasing, gradually into each other, sudden variations not being observable save when exacerbations or irregular complications coincide." P. 73.

In corroboration of the above views tables are furnished of twenty cases of inflammatory disease observed in the hospital, for the relief of which were collectively performed 147 venesections. Notes to each case reported exhibit the author's views of the amount of corroboration derivable from it. Some of these are highly interesting, but we have only space to notice some of the practical conclusions he arrives at.

"The observations already made upon the indications the physician may draw from the observation of the coagulation of the blood, and the clinical cases adduced in confirmation and illustration of this criterion, clearly prove that its value rather lies in its enabling us to fix a limit to the abstraction than in encouraging its continuation. And, in fact, if we are not deceived, the comparison of the coagulation of the first and last portions may, independently of the presence of all other symptoms, distinctly indicate whether the evacuation will tend to normalize the vital powers of the functions of the organism, at one time liberating them from oppressive congestions, and at another from the obstacles presented by the excessive and unbalanced action of the nerves, or whether it attacks them with all its impoverishing effects, and directly exhausts the forces necessary to the carrying on of life. Of the two indications which this sign offers the last is not only the most important, since its neglect almost amounts to a fatal result in the disease, but it is also the most attainable, or at least the best supported by facts. The cases referred to show that, if the coagulation takes place with a certain celerity, and this manifests itself repeatedly, and goes on increasing with the bloodletting, we cannot persist in the measure without losing the patient; while the patient hardly ever dies when it is suspended prior to the coagulation having acquired great rapidity.

* * * * * * * * *

"*It is not necessary for the complete cure of an inflammation to continue the bleedings until the blood no longer gives any buffiness; while it is absolutely necessary to cease the emission when the blood coagulates more rapidly than in the normal state.* The production of buffiness of blood of equal coagulability, as shown in the former series, is always rendered more easy and in larger quantity after a certain number of bleedings than at first, in consequence of the diminished density which the blood acquires; which naturally always much diminishes the phlogistic expression and the consequent indication for bleeding drawn from the crust that covers the blood after a certain number of emissions. The crust or buffiness, in fact, not being *essentially* produced by an increase of fibrine, by a diminution of red globules, or by an attenuation of the serum, but arising from a certain slowness of the coagulation—(of that faculty by which, in certain morbid conditions of the organism, and especially under the influence of the phlogistic process, the fibrine has acquired the power of maintaining itself in a state of fluidity for a period always much longer than in the normal state), it may at once disappear by the operation of whatever modifies that slowness. When the phlegmasia is subdued, and the morbid reactions give way to healthy movements, the blood will cease to contain fibrine in a hypersthenic condition, and will then undergo coagulation in a period of time that does not permit the appearance of the buffiness. It happens not infrequently that if, for some reason independently of a reproduction of the phlegmasia, we draw blood during the advanced convalescence of a severe inflammation, in the treatment of which bleeding had been suspended, while the blood was yet covered with a firm phlogistic crust, it will now be found to present no trace whatever of this. A patient may have blood in circulation which if drawn would furnish a buffy crust, and who will yet be perfectly cured without bloodletting. This change in the blood within the vessels, without profuse crises inducing the belief that the morbid matters supposed to be indicated by the buffiness had been evacuated by other channels, frequently excited the surprise of the ancients; but, faithful to observation, they had

nevertheless laid down as a canon, *Ob solam crustam inflammatoriam venæsectio repetenda non est.*' (*Quarin Met. med. Inflam.* p. 70.)" Vol. 109, p. 104.

Series III.—On the Condition of the Fibrine of the Blood in Inflammation.

A question touched upon in the last extract quoted is treated more at length in the present series of observations:—

"The fibrine of the blood exists in a perfectly fluid state in the blood while within the vessels of a living animal, and for some time after being drawn from these. This fluidity, however, is not like an ordinary solution of a solid body in a liquid menstruum. It is true that the alkaline salts hold the fibrine a long time dissolved, out of the vessels, and will re-dissolve it if already coagulated; carbonic acid, too, retards the coagulation to such an extent that it also may be termed a solvent of the fibrine; but the fundamental reason why fibrine is in a fluid state within the vessels of a living animal, and why out of these it more or less readily consolidates, is entirely independent of the alcalescence of the serum and the impregnation with carbonic acid. It is intimately connected with the peculiar vital formation of the tissues, among which a few minutes before it had been circulating. Negative electricity seems also to retard coagulation, but it is not yet clearly made out whether the influence it exerts is purely dynamic, or whether this does not rather arise from the chemical changes induced in the solutions of the salts which are found in the serum in contact with the fibrine. The fluidity of the fibrine in the blood is a species of *vital* fluidity, and its *coagulation* is a cessation of this vital property. Although chemists may demonstrate fibrine and albumen to have the same atomic composition, out of the animal body, and from their isomeric characters, may assume each substance will be similarly affected even within the body by chemical influences—and thus simplify the chemical history of those two substances—they will not by that have removed the necessity of their physiological distinction. When we see them in the laboratory we may allow that they are the same organic material the fundamental composition of which is termed by the moderns *protein*, and it is a matter of indifference whether the same name be given to the two or not; but when we observe them in the living animal, or just extracted from its vessels and yet endued with vital characters, we then see the necessity of distinguishing them by distinctive appellations. One of these materials (*the albumen*) remains permanently fluid even when out of the vessels, after the coagulation and even the destruction of the clot; the other (*the fibrine*) more or less readily loses its fluidity and solidifies. Such a difference is amply sufficient to prove the necessity of not confounding together these two organic materials, whatever may be the identity of the remote principles which analysis discovers in them. The names *fibrine* and *albumen* then, should be preserved at least by physicians, as signs expressive of the two physiological characters peculiar to the mode of existence of this organic material during its functions in the vital circle; while when they are divested of the characters they owe to vitality, their radical composition will be most conveniently expressed by the chemists by the term *protein*." Vol. 113, p. 333.

Inflammation may modify the fibrine of the blood in three ways: by increasing its quantity; retarding its coagulation; and by rarefying it.

1. *Increase in the Quantity of Fibrine.*—The author's investigations by means of the areometer confirm the conclusions arrived at by Andral, Becquerel, and others, that in inflammation there is a marked increase of

fibrine; and he believes that from the researches of Becquerel, Rodier, and Mulder, it may be deduced, "*that this increase of fibrine is due to the oxidation of a proportionate quantity of the albumen of the serum of the blood.*"

2. *Delay in the Coagulation of the Fibrine* (*Bradifibrina.*)—Healthy fibrine ordinarily coagulates in a few minutes after the blood has been drawn. If the individual furnishing it is very enfeebled, aged, or very young, oppressed with plethora or overcome by faintness, the coagulation takes place so rapidly as even to occur around the aperture of the vein whence it flows. On the contrary, when the person is suffering from inflammation, it mostly coagulates slowly enough to allow of the formation of the buffy coat, and sometimes so great is its resistance to coagulation, that it may remain fluid for several days first.

An interesting example is cited, in which the blood first drawn from a patient suffering under severe pneumonia did not coagulate for fifteen days, when it formed a coriaceous crust. This man was bled ten times, and after each bleeding, just as the inflammation showed more disposition to yield, so did the blood coagulate more readily, and after the last venesections it did so in a few hours. Many of the cases of dissolved and incoagulable blood related by various authors have been mere examples of slow coagulation, in consequence of the persistence of the morbid process; and ignorance of this fact has led to the dangerous practice being so frequently followed of taking the indication for repetition of the bleeding from the ready coagulation of the blood, and the contra-indication from the persistence of its fluid condition!

"A question here at once arises. In what light are we to regard all the descriptions of non-coagulable blood consigned by authors in their histories of putrid and malignant diseases? Are we to reject them as false or admit them as characterizing diseases which are no longer met with? We incline to the belief that the excellent authors who have spoken of dissolved blood, and of blood which would not coagulate, have for the most part badly, or at least incompletely, observed, and have been induced by the condition of their own mind, or the influence of some dominant theory, too hastily to conjoin certain properties with certain physical appearances. And that such an error may have easily and frequently occurred, and will do so again, for a longer period than we could desire, the case above referred to suffices to prove. The blood at the first bleeding possessed all the characteristics by which the older physicians were accustomed to judge of it as dissolved and incoagulable, and indeed a practitioner of our own times, unless his attention had been called to the subject, would not have probably given a different opinion. What would be said of blood which had continued fluid for more than a week, and presented such an absence of the so-called plasticity as to allow of its being mixed together like so much serum, and which deposited at the bottom of the vessel a mass of deep coloured red globules, destitute of all cohesion? Would it not be said that this was the dissolved blood of an adynamia, a wanting in fibrinous principle, or tending to putrefactive dissolution? Well! This same blood, in process of time, presented the firmest crassamentum, and a most decided buffiness. So far, too, from furnishing signs of putrefactive decomposition, it did not exhibit these for a month, while an equal quantity of blood taken for an ordinary disease of the chest, and exposed side-by-side with it to the same air and temperature, putrefied in a fortnight. The individual, likewise, who furnished this blood, so far from suffering under adynamia or any cachexia,

was the prey of a violent inflammation, which the most active curative measures, consisting of twelve bleedings of 12 ounces each practised in eight days, &c. &c. hardly sufficed to subdue; and nevertheless the cure took place after a very short convalescence, so that 20 days after he had been received at the hospital he was enabled to leave it again in full health.

"For a long time past I have used every diligence in our large hospital (at Milan) to discover some examples of this dissolved, non-coagulable blood, as described by some pathologists; but I have not been able to find one solitary case, in which blood, *left to itself for a sufficient time*, and protected from external destructive influences, has not undergone a distinct coagulation, prior to putrefaction. Many of those I examined had remained for days in a fluid state, and many had all the appearances justifying completely the prediction of incoagulability, resolution, &c.: but with some care I was always enabled to demonstrate the coagulability. More than once also it happened to me to see blood coagulate which had been taken from the veins of a body 36 or 48 hours after death. And in respect to this, I may observe in passing, that it seems to me that the conditions of rigor or relaxation of the parts of a dead body are in every case dependent upon the state of coagulation of the fibrine in its capillaries, whether this is accomplished, retarded, or re-dissolved." Vol. 113, p. 348.

The author details at some length the particulars of five of the cases observed by him in the hospital, and a hasty examination of which would have led to their being set down as examples of the dissolved state of the blood. He concludes by repeating his opinion, that the *coagulation of the blood prior to its putrefaction is in all cases inevitable;* that the blood in scorbutus and typhus which authors have noted as exemplifying its dissolved condition, is blood more or less poor in fibrine, and more or less slowly coagulable; but that, according to the quantity and quality of the fibrine, it does coagulate sooner or later, and those who have not seen its coagulation and yet noted its putrefaction, have observed it too early or too late. For the expression of long-delayed coagulation he proposes the term *Bradifibrina* (*βραδυς, slow*), as merely indicating slowness, without implying any opinion as to the cause of this.

4. *Rarefaction of the Fibrine* (*Parafibrina*).—From various experiments which he undertook the author concludes "that the fibrine of the blood may, under morbid circumstances, assume a lesser density, or become rarefied; that fibrine so modified imparts its tenuity to the mass of the blood in which it exists; and that in such condition it at the least is of lesser density than is the albumen of the serum." This, however, does not hold good of all the fibrine of the blood contained in an organism suffering under inflammation, but probably can only be extended to all that of new formation, or which is directly produced by the inflammatory process itself. The blood in the normal state holds fibrine in solution, which by coagulating, renders the liquid residuum of a lesser density, as is the case with every solid body which had previously been dissolved in a liquid of lesser specific gravity. The fibrine rendered slow in coagulating by inflammation, *bradifibrina*, also renders the fluid more dense in which it is dissolved, and lessens its specific gravity upon separating from it. "But there is a third species of fibrine, which is the modified fibrine upon which we are now especially treating, and which has the peculiar property of rendering the liquid in which it is found specifically lighter. We propose

to term it *Parafibrina*, in imitation of the chemists who thus distinguish isomeric substances, or those which, preserving the same composition, are yet modified in their properties."

Parafibrine in general coagulates with exceeding slowness, so as even to surpass bradifibrine in that respect. Its coagulation, too, takes place in such slender filaments as to be hardly recognisable by the naked eye, and giving rise, together with the enclosed serum, to a mass possessing rather a gelatinous than a fibrous appearance. The delicate meshes thus formed may be to some extent compared to the cellular net-work which gives something like consistence to the albumen of the white of an egg, or to that which supports the vitreous humour of the eye; but, when separated by pressure from the serum it contains, it is found to be a fibrous tenacious body. The serum raised under the epidermis by blisters or slight scalds is especially rich in parafibrine, and its properties can be best studied in this fluid. When parafibrine coexists with ordinary fibrine or bradifibrine, it coagulates at a later period upon the surface of this as a gelatinous deposit of varying extent; and blood which retains its fluidity for a very long time always contains it. "There are cases in which the blood presents very little crust, although the crassamentum possesses great consistency, and is covered with a thin stratum of a tremulous gelatiniform buff. The production of parafibrine in such cases does not seem to be accompanied by a proportionate augmented quantity of ordinary fibrine, or with the production of bradifibrine. The three modifications of fibrine now noticed may exist independently of each other in a given specimen of blood, and in very different quantities; just as they may all three exist in the same blood, likewise in different proportions."

Signor Polli adverts to the fact of Hewson and Dr. John Davy having already expressed their opinions, that the production of the buffy coat is in some measure due to the greater tenuity of the fibrine; but infers, from a passage in Mr. Wharton Jones' Report in 1843, that these had received little or no sanction among their own countrymen.

Applications.—"The character we have recognised in Parafibrine, of possessing a greater tenuity than the serum in which it is found liquified, furnishes the explanation of a pathological phenomenon of great moment; I mean of the fibrinous or plastic transudation, and the sero-fibrinous transudation which constitute the termination of the greater part of the membranous inflammations. Great surprise has always been excited by the dense strata of fibrine which have been found on membranes which have undergone inflammation, and the large pseudo-membranes or flakes of coagulable lymph which are seen swimming in the serum contained in the cavities of inflamed membranes; and the explanation of these familiar facts has continued a desideratum.

"It certainly was far easier to imagine that extravasated serum became coagulated, than to believe that fibrine, which out of the blood-vessels is the body to which the blood owes its consistency, tenacity, and durability, could pass through the walls of the vessels. Nevertheless, it is demonstrated that not only are these morbid transudations of a truly fibrinous nature, but that they transude through the vessels, and coagulate sooner or later, just as does blood which has been drawn. If we examine these products in the dead body, we certainly only find them in their solid form; but the evidence of practitioners is not wanting to shew that, in the fluid evacuated from the chest during life for a pleurisy, the serum has been found in a short time the consistency of a jelly. So, again, pus evacuated in empyema or abscess has, after

a while, separated itself into clot and serum; serum evacuated for ascites supervening on peritonitis, or from a hydrocele consequent upon inflammation of the tunica vaginalis, has been converted by coagulation into a tremulous mass. That kind of gelatiniform crust which often covers the surface of blistered parts is but a fibrinous deposit, which, at the beginning, consisted of but a very fluid exudation." Vol. 113, p. 367.

The author adds, that this lightness and tenuity of the parafibrine furnishes also an explanation of a fact which may sometimes be observed in certain stages of inflammation, and which by some has been adduced as an argument in opposition to the law of the formation of the buff of the blood laid down in the foregoing pages—viz. the case in which a thick buffy coat is found on blood which has coagulated with a certain amount of promptitude. Suppose that in such blood there exists a sufficient amount of parafibrine to rarefy the fluid, and that it has been drawn after several prior emissions; being thus greatly diluted and quickly coagulable, the conditions are present which permit the red globules to rapidly descend, leaving on the surface a clear stratum of fibrine, which also, after a little time coagulating, may give a thick buff.

The following are some of Dr. Polli's observations upon the conclusions to be drawn from the different appearances of the fibrine :—

"These various modifications of the fibrine seem not only to characterise inflammation in the most distinct manner, but each seems to connect itself with a particular degree of intensity of the inflammatory process. Thus: the simple *increase of the quantity* of the fibrine indicates the development of the first stage of an inflammation the extension of which will be measured by the proportionate excess of this material. The *retardation of the coagulation* indicates an aggravated degree of the inflammatory process, inducing that modification in the fibrine we have termed *bradifibrine*. Lastly, the highest degree of a phlegmasia gives rise to that modification, by reason of which the blood presents the gelatiniform crust, dependent upon the production of *parafibrine*.

* * * * * * *

"If the coagulum forms with sufficient promptitude to prevent buffiness, and does not give it even when coagulating somewhat slowly from its great density, but is itself consistent and hard, it is an indication that in such blood there is *simple excess or abundance of fibrine*. The phlogosis is only in its first stage, and it will become more or less extended in proportion as the quantity of fibrine is increased. If there is a clot not only consistent and hard, but likewise furnished with a thick stratum of a white coriaceous crust, this having been formed by a proportionally slow coagulation, this is an indication not only that there is an inflammation of some extent, but that it has reached that higher degree of intensity, in which a *certain quantity* of *bradifibrine* is produced. If, on the contrary, the crassamentum is observed to possess but slight consistency, approaching in that respect to a normal condition, but that it possesses a little of the gelatiniform crust or buff, the inflammation which so modifies the blood has reached its highest degree of intensity, but is circumscribed; while we must regard it as both intense and extensive when this gelatiniform crust of *parafibrine* is abundant, and also accompanied by a greater or less share of bradifibrine. This highest degree of the inflammatory process may exist in some points of a tissue and manifest itself as just stated, while in other portions of the same tissue a milder phlogosis, competent only to lead to the two other modifications of the fibrine, may be pursuing its course. Thus we may have at the same period the tissues invaded by an inflammation which for a long period is confined to its first degree of intensity; other portions affected with greater, or, as we may call

it, a second degree of intensity; and still other points, which however are for the most part very limited, in which the process goes on with its highest intensity. In such a case the blood will furnish a very consistent coagulum, and one rich in fibrine, which will cover it with a thick coriaceous buff, and upon the surface of this gelatiniform deposits will be observed." P. 371.

The reasons which have induced the author to consider the *parafibrine or gelatiniform crust* to denote the highest intensity of the inflammatory process are the following. 1. It appears much more frequently in the earlier than in the later periods of the disease, when all the symptoms denote the activity of the process; and disappears before the *bradifibrine* (or *coriaceous crust*), and long before the simply increased fibrine (*firm coagulum*) as the disease advances towards its cure. 2. It appears as a consequence of intense inflammation of the skin excited by blisters, &c., but not when this exists in a milder degree. 3. It is found in the fluids produced in various intense inflammations, such as pleurisy, &c. In a note the author states, that Dr. Gamberini, one of the most careful observers in Italy, has informed him that he hardly recollects a case in which the appearance of gelatiniform flakes and lines upon the clot was not accompanied with great intensity of the disease.

Series IV.—On the Effects of the Abstraction of Blood upon the Human Organism.

"If the abstraction of blood were confined to the production of a greater or less diminution of this fluid, and to a proportionate decrease in the vital power and in the functions of the part which most nearly depend upon it, the question of bloodletting would be one of such simplicity that its mere proposition would suffice for its solution. But the effect of bleeding cannot be regarded as a direct diminution of vital power solely because it induces a direct diminution of the material for the exercise of life; nor as an enfeeblement of that power because it directly lessens a means necessary for its manifestation. It may and does produce these effects, not however in a direct way, but by a series of successive and reciprocal modifications, that furnish an explanation of all the various phenomena, to which the loss of blood, whether in the physiological or morbid condition, gives rise.

"*The first modification which the abstraction of blood induces in the organism is exerted upon the blood itself;* a modification both immediate and great, though hitherto not noticed, or only superficially so, by clinical observers. And it is from this modification that all the other multiplied phenomena originate. It is from having commenced the examination of the different effects of bloodletting in an inverse order—from having sought for the material and functional changes in the solids consequent upon it, before examining what changes the circulating residuum itself had undergone—that the study of this subject has proved arduous, vague, and, I may add, of little utility. I have no intention to attempt the examination of the intimate dynamic or molecular changes which the blood under this influence undergoes, because I do not feel certain of the utility of enquiries during which the senses do not continuously bear us company; nor have I the intention of submitting the blood to chemical decomposition and the forced isolation of its principles by means of re-agents and analyses, because the indications derivable from these do not seem to me all equally to be relied upon. But I believe that the mere accurate determination of the principal sensible phenomena which the blood under such circumstances presents, may be so conducted as to

produce an assemblage of precise expressions of its manner of modifying itself in strict relation with its material conditions, and the amount of vitality it indicates. And it is the result of the study of such modifications which I am about briefly to advert to, with above all the intention of showing the extent to which we may determine with exactitude the *influence of bloodletting on the cure of diseases*; and of what importance the determination of this is in clinical practice." Vol. 121, p. 7.

The mass of the blood may be modified after venesection as respects its *quantity*, its *quality*, and its *motion*.

1. *Effects of Bloodletting upon the Quantity of the Blood.*—It is an error to suppose that the mass of the blood becomes diminished in quantity in proportion to the amount abstracted. To prove this, it is only necessary to examine, by means of the areometer, portions taken at the commencement and at the conclusion of a venesection, when the latter will be almost invariably found to be of less density than the former. M. Polli has supplied several tables deduced from often-repeated investigations of the exact amount of this diminution of density, both during any one bleeding, or from one bleeding to another, when these have been repeated. The following are the conclusions at which he arrives:—

"1. The blood which at the end of the emission possesses a less density than that first drawn, and its serum which is often found to be of greater density than that which accompanies the first blood (though of lesser than the whole mass of the blood), prove that, during the very act of bleeding, there is absorbed into the circulating channels a greater or less quantity of serum of varying density. 2. The quantity of serum that enters the circulation during an ordinary bloodletting to induce in the totality of the remaining mass the mean dilution which the areometer exhibits, is approximatively equivalent to a thirtieth part by weight of the remaining blood. 3. The quantity of serum which is absorbed from one bleeding to another in an interval of twelve hours, is about double that amount. 4. The dilution of the blood which takes place during bleeding is principally produced by the mechanical forcing into the vessels, by means of atmospheric pressure, of the fluids which everywhere moisten the tissues; but the dilution which takes place from one bleeding to another, is the effect of an absorption governed by the vital laws." P. 27.

There are certain facts also, which are explained by the above circumstances, such as the thirst which patients so often experience after bleeding; the dryness of the mouth and fauces, which women suffering from great metrorrhagia complain of; the diminution or even complete absorption of some dropsies after free bleeding, &c.

"As regards the question before us, the human body may be regarded as a spongy tissue, entirely permeable by the different humours which fill its cells and freely traverse its vessels. The atmospheric pressure to which it is submitted not allowing that any cell or any vessel shall remain empty or only partially filled, is continually propelling the liquids to whence least resistance offers itself, co-operating with the vital action of the tissues and the fibres which constantly contract on the volume of the contained fluids. During the aspiration which is immediately produced by opening a vein, the liquids which proceed constantly to dilute the mass of the blood may be regarded then as only mechanically driven into the vessels, and they do not in fact exceed in quantity the amount of blood lost; but, in the intervals which occur between one bloodletting and another, the diluting liquids are physiologically introduced by means of the absorbents, when

too they are more elaborated, and often temporarily in greater quantity than suffices to replace the blood taken. And this explanation leads to the establishment of the difference which exists between a free evacuation of blood at one time, and a number of smaller abstractions made at different periods, although, taken altogether, not more blood is taken by the one plan than by the other." P. 23.

Practical Applications.—The fact of the rapid absorption of fluids which supervenes on venesection may be advantageously borne in mind when we have in view the augmentation of the operation of certain medicinal substances, the diminution of the density of the blood, and the removal of effusions from important organs; while, when we wish to delay absorption in the case of poisoning, purulent infection, &c., we must abstain from the abstraction of blood. Employed during the existence of an abscess, and especially if this is in communication with the air, it would become but the means of inducing a more general poisoning of the blood. So, just as the operation of purgatives and emetics long taken without any effect being produced is thus much expedited; and a prudent emission of blood will favour the specific action of mercury; so in recent poisoning, and in those somewhat similar cases of gastric-bilious saburræ of an acrid and putrefactive character, venesection but hastens the fatal result. "To the same cause, also, we may attribute the promptly fatal termination of certain chronic affections of spoiled organs, which nature had nevertheless endeavoured to maintain in an isolated condition, and which uncompromised by bleeding might not so easily have terminated life."

2. *Effects of Bloodletting upon the Qualities of the Blood.*—The blood may be *qualitatively* modified in respect to its *density*, its *coagulability*, and its *temperature*.

(A). *Modifications of the Density of the Blood.*—These have been already briefly alluded to in the foregoing section: but the author here enters into the subject in great detail, exhibiting the tabular results of observations he has made on patients, and of numerous experiments he has performed on the horse. The questions he announces for solution are—1. What are the laws which govern the diminution of the density of the blood in general, during the same bleeding, and in the intervals of different bleedings? 2. What are the laws which govern the recovery of its density by the blood, after the cessation of such evacuations? 3. What is the relation which these modifications of the entire blood bear to those of its serum; and to what extent do they also take place in respect to its other component parts? To follow him through the detailed replies he furnishes, would far exceed our limits; but we may state, as the general result, that the density of the blood was found to diminish in proportion to the amount abstracted; and to be recovered after the abstraction had ceased, in proportion as the individual had become otherwise restored to his normal condition. Comparing the condition of the mass of the blood with that of its serum, "the blood loses nearly $2\frac{1}{2}$ of its cruor to one of the solid materials of the serum, as the effect of several bleedings; while, during the same bloodletting, the loss seems exclusively to affect

the cruor, the serum almost always increasing in density." The relative condition of the various component parts of the blood is thus stated:—

"The speedy dilution of this fluid is far from merely consisting in a simple watery addition to its mass; and the term *Hydræmia*, proposed to distinguish this particular condition, would only express one of the modifications it undergoes, and that perhaps the least important. Recapitulating the variations which the blood presents in its principles, with a diminution of its density, and availing ourselves not only of our own areometrical data, but also of the analytical results declared by hæmatologists in recent times, we may conclude

"1. That, as a consequence of a *series of moderate bloodlettings* practised at short intervals in the same individual, the *fibrine* is stationary; the *red globules* and *albumen* diminished; the *saline*, *fatty*, and *extractive* matters are rather stationary than in excess; and the *water* is increased. 2. That, at the conclusion of a single *very free bloodletting*, the *fibrine* is increased; the *red globules* are diminished; the *albumen* is stationary or increased; the *saline*, *fatty*, and *extractive matters*, and the *water* are increased. 3. That, as a more remote consequence of one *very abundant bloodletting*, the *fibrine* diminishes; the *red globules* continue to diminish; the albumen is diminished; the *saline matters*, *extractive*, *fatty matters* and *water increase*.

"The materials most easily reproduced in the circulatory mass after abstraction are then the water, the saline and extractive matters, and the fibrine; the albumen is reproduced with more difficulty, but the red globules with much more so. One of the effects of bloodletting the least easy to remove is the *defect of red globules*; and as these are the most vital portion of the fluid, constituting almost organized beings functioning of themselves, we may say that bloodletting tends to deprive the blood of its most characteristic element: and this condition of the fluid will consequently be more faithfully expressed by the term *despoliation* (*spogliazione*), employed a century ago by Quesnay, than by the terms *hydræmia* or *anæmia*, used by the moderns.

"By bleeding, then, the blood is despoiled of its most noble constituents, becomes watery, and is mixed with ill-elaborated materials, having little affinity with it. The vital excitement of the tissues and nerves is thus intercepted and depressed, just as it would be by a direct injection of foreign matters into the blood; so that, in this point of view, bloodletting is a species of poisoning of the blood itself, greater or less according to the quantity taken and the mode of its abstraction. And it is of this poisoning of the blood, as well as of the deprivation of its more perfect constituents, that medicine avails itself, when abstracting blood, to overcome or moderate processes of irritation or inflammation; it presents directly to the patient thus suffering, as succedaneous and adjuvatory to bloodletting, small doses of poison, which absorbed into the blood, reproduce in part the good effects directly derivable from bloodletting." Vol. 121, p. 79.

An increased *quantity of fibrine* after repeated bloodletting is not only found in man suffering under inflammation, and kept on low diet; but also in healthy horses kept also on short commons, and bled day by day. The evacuation of blood alone, then, leads to the production of fibrine, as noticed already by Pessina and others.

"In our experiments upon the horse we have stated that the thickness of the crust was proportioned to the frequency of the abstraction; so that when, after the first bleeding, it formed but a third or a fourth of the coagulum, after the last it constituted four-fifths. The principal cause of this increasing thickness of the crust is the diminished density of the blood and the paucity of red corpuscles, by reason of which these are enabled speedily to descend to the bottom

of the recipient, where they occupy a small space only, leaving a proportionally larger stratum to figure as a crust. This crust may be called the *false buff* or *buffiness from bloodletting*, to distinguish it from the *phlogistic buffiness*, which occurs in blood far denser and more rich in red globules, and whose principal cause is slowness of coagulation." P. 72.

(B). *Modifications of the Coagulability of the Blood.*—This point has been too minutely considered in our abstract of the First Series of the author's Researches to require further notice here. We may merely observe that his experiments upon horses confirmed the conclusions he arrived at at the bed-side, and which he sums up in the following proposition: "*In proportion to the quantity of blood taken at one bloodletting, the shortness of the interval which transpires between different bleedings, and to the number of these abstractions practised on the same individual, so much the more readily will the blood be found to coagulate, or so much the less resistance will it present to the coagulation of its fibrine.*"

(C). *Modification of the Temperature of the Blood.*—The investigations of Nasse, Marshall Hall, and Popp, are adverted to in this section; and from these, together with a numerous series of original observations, Dr. Polli arrives at the following conclusion:—

"That the *immediate effect* of bleeding is to *diminish the temperature* of the remaining blood, and of the subject to which it belongs; but that, *some time after the bleeding*, and upon the *repetition of the bleeding* in the same individual, an opposite effect, or an *elevation of the temperature*, is produced.

"The causes, by reason of which among the effects consequent on bloodletting there is an elevation of temperature, are complex, and it must suffice here to indicate them. 1. The acceleration of the circulation, which renders the respiration also more frequent, and the pulmonary hæmatosis more active. 2. The introduction of combustible substances, especially fatty matters, by means of the more active absorption induced by the bloodletting, the which, when commingled with the inspired oxygen, must necessarily furnish new elements of calorification." Vol. 121, p. 267.

(D). *Practical Applications.*—(1) In the inflammatory diseases of the pulmonary organs, the blood can be held in an attenuated and diluted state under the influence of bloodletting to a far greater extent than by the imbibition of aqueous fluids; and in this way is expectoration rendered easier than it otherwise would be.

(2) Profuse sweating, abundant alvine excretions, prolonged abstinence from drink, and whatever increases pulmonary exhalation (as exercise, a warm, dry, or rarefied air), render the blood more dense; and hence some of the benefit which persons of lymphatic constitutions derive from a residence in mountainous regions, and the free use of purgatives. Becquerel has pointed out the fact of the frequent use of *purgatives* rendering the urine scarce and sedimentiferous; and, as a strict relation prevails between the density of the urine and that of the blood, care should be taken, in the case of *pure plethora*, not to aggravate, by the use of these substances, a condition of the blood which is only relievable by bloodletting.

(3) It is not a matter of indifference whether a *given amount of blood be abstracted at one bleeding or at several.* By the first plan a much more complete and sudden modification of its mass is produced, and an effect

to the same amount would require three times as much blood to be taken by small bleedings for its production; giving rise to a greater loss of red globules, a slower reparation, and a more tedious convalescence. The smaller bleedings allow of an imperfect reparation in the intervals, but the greater tolerance thus induced is a deceptive phenomenon when full bleeding is indicated, although, under other circumstances, it may be made usefully available.

"Free abstraction is indicated when we have to do with a commencing inflammation of a severe character in a robust individual; while the smaller loss is indicated in feeble subjects, liable to disturbances of the circulation, or endowed with great nervous susceptibility. In case of collections of corrupt matters (as abscess, open cancer, gangrenous foci, &c.), a large abstraction is never advisable; while in *marked plethora* (dense blood too rich in globules) it is indicated. Free abstraction, developing in great part also a mechanical effect, is required in those hydraulic derangements of the circulation arising from remora, or partial fulness of vessels; and in cases in which it is important to excite, at whatever cost, a rapid absorption of effused fluids. Small, interrupted emissions, will, on the other hand, be always preferable, when, in a diseased state of the circulatory organs, rapid changes of the fluid in them, or a difficult assimilation of fluids absorbed without much election, is to be feared." Vol. 121, p. 281.

(4) The phenomena observed in the excessive loss of blood in *hæmorrhages* teach us two things. *First.* That a profuse loss of blood is best borne when taken so as to imitate in some degree the stillicido of hæmorrhage than if abstracted rapidly by a free bleeding. Quesnay showed that, if a large opening be made in a vein, we can regulate the flow by means of a bandage just as we please; and, if it be so managed that about six ounces only are lost in the hour, the patient will be found able to tolerate such an evacuation for a very long period; so that at the end of the 24 hours, perhaps, 12 lbs. (of 12 ozs.) may have been taken.

"In suffocative angina, in grave pulmonary congestions threatening asphyxia, in certain very rapid cerebral phlegmasiæ, and, above all, in cases in which energetic and prompt treatment is demanded, while the tendency of the patient to syncope impedes its adoption, or gives rise to functional disturbances it is desired to avoid, this means of bloodletting becomes of great use."

Secondly. When we are desirous of *arresting a hæmorrhage*, we may often accomplish this by drawing a small quantity of blood in a large stream; so that, with no great loss and endured for a very short period, coagulation at the mouths of the affected vessels may be promoted. This prevents that despoiling of the blood and exhaustion of the strength which supervene on a constant drain of blood, surprisingly as the system may seem to tolerate this.

"And it is not unreasonable to suppose that many cases of metrorrhagia, hæmoptysis, epistaxis, and even hæmorrhage from mechanical lesions of internal vessels, to combat which a number of interrupted bleedings have been in vain employed, besides a variety of cold and hæmostatic applications, would have been easily checked, had the patient been boldly bled, *ad deliquium*, at the commencement."

(5). It is a well-established fact, that excessive bloodletting, and even moderate in subjects having diseased circulating organs, give rise to *dropsical effusions*. This does not seem to be dependent upon a disturbed state of the circulation, or on any venous stasis, but may be, in our author's

view, referred to three causes. (*a*). In such persons there is induced in the remaining mass of the blood the same serious modifications observable in Bright's disease—watery dilution, diminution of globules, and great impoverishment of albumen. (*b*). After repeated bleeding, the secretory organs become inert and indolent. (*c*). The last cause, probably the foundation of the other two, is found in the diminished density of the blood, allowing it to yield more readily to the capillary attraction of the membranes, and thus give rise to an exosmosis or hydropic effusion which would not occur in denser blood.

This leads us to an appreciation of the *advantageous employment* of *blisters* for the cure of dropsies dependent npon a modification of the crasis of the blood. By free vesication we may take from a patient from 8 to 16 ozs. of a fluid which is a species of diluted blood—" thus evacuating all the materials of the blood except the red globules, practising, in fact, a *white bloodletting*, acting upon the remaining mass in a completely opposite manner to its spoliation by bleeding. This is probably the chief reason for the great assistance all practitioners have derived from the use of blisters at the close of the treatment of inflammation, when much blood has been abstracted, and the removal of a commencing effusion is desired. How can we in such cases better remedy both the effects of the disease and the treatment than by the use of blisters?" Large flying blisters are best adapted to this end.

(6). The attenuation of the blood by repeated bleeding, its deprivation of red particles, and the excess of fibrine, induce an amount of crust or buffiness which would not result from the mere slow coagulation of phlogosis. A *false buff or a buffiness from bleeding*, may thus become very marked, and a practitioner ignorant of its mode of generation may draw very unsafe indications from its inspection.

(7). The question whether bleeding, used immoderately, or in cases in which it is not strictly necessary, may not *give rise to a necessity for its more frequent repetition*, a habit, is answered affirmatively for the following reasons:—

" (*a*). That it accustoms the organism to tolerate a change in the composition of the mass of the blood which renders the effects of subsequent abstractions less sensible. (*b*). It habituates it to the renunciation of natural modes of restoration, or such as, by means of a little expectation, might easily be made to appear: so that after one, two, or three morbid emergencies, cured in this oblique manner, on a fourth occurring, bleeding becomes inevitable. (*c*). A reparation of blood greater than the loss is sometimes provoked, giving rise to a species of *false plethora*."

(8). Not only may excessive bleeding render convalescence tedious, and give rise cachexiæ and vices of innervation difficult of removal, but it may lead to a fatal termination of the malady, which would not otherwise have taken place. The effects of repeated bleeding are accumulative; and the apparent toleration exhibited by the individual in so few signs being offered of disturbed functions is a far less faithful guide than is the examination of the changes operated in the blood. These are progressive, as the fluid cannot be re-constituted when the abstractions succeed each other at short intervals. The effect does not terminate with the bleeding; for the absorption of new materials, and the influence of these upon the blood, continue long after.

To the remaining section on the "*Effects of Bloodletting upon the Motion of the Blood*," we have space only to allude. The experiments of De Heide, Haller, and Spallanzani upon cold-blooded animals, together with his own on horses, and his observation of man suffering under disease, lead the author to the general conclusions that the *immediate* effect of bloodletting both, on man and animals is the production of a *greater quickness* of the pulse: but, in both, when carried to *syncopy*, it *diminishes* the number of beats. *Repeated abstraction* not only tends to *accelerate the circulation*, but also, in a still greater degree, the *respiration*; and this increased frequency of pulse and respiration are among the effects of bloodletting latest in disappearing.

This brings Dr. Polli's valuable labours to a conclusion, as terminating the investigation of the *immediate modifications* which the blood undergoes. To complete the subject, the effects which the fluid so modified produces on the various organs has yet to be examined; and we hope this able enquirer will not recoil before a task he declares too difficult and laborious for him to encounter. Certain are we that a more careful and talented investigator will not be found; and in no country is such an enquiry more urgently called for than in Italy, where it seems to us that this therapeutical agent is employed with a most unjustifiable freedom and rashness.

The Physiological Anatomy and Physiology of Man. Part the Third. By *Robert Bentley Todd*, M.D., F.R.S., Professor of Physiology, and *William Bowman*, F.R.S., Demonstrator of Anatomy in King's College, London. John W. Parker, 1847.

We are happy to announce the appearance of the first portion of the second and concluding volume of this important work; and trust that the remaining part will, as the advertisement states, be speedily published. The part before us contains a very interesting account of the organs of smell, vision and hearing, including their structural anatomy and physiology; the anatomy and functions of the several encephalic nerves of the great sympathetic, are next given; and lastly, the organs of digestion are partly investigated. We propose to lay before our readers a concise account of the many new facts connected with the Organs of Sense, with which the authors have enriched their pages; but to accomplish this object, it will be necessary to refer to the latter part of the first volume, containing the Organs of Touch and Taste, published about two years since, and which we have not hitherto had an oportunity of noticing. We must, however, in the first place congratulate Dr. Todd and Mr. Bowman, and especially the latter gentleman, that they have been able, by their admirable and skilful observations, to throw so much new light on the delicate organization of many important parts; a merit this of no ordinary kind, when it is recollected that the intimate texture of the organs to which we refer, has been made the subject of laborious and repeated vestigation by several of the most distinguished microscopists of Europe.

Without entering into any metaphysical speculations, we may briefly consider, as constituting the most fitting introduction to the subject matter of the work before us, the nature of sensation in general, and especially the relations existing between the mind and the external objects of sense; and we are the more strongly prompted to this, by the consideration that some of the errors of former times still linger among us; and especially because, although the mechanism of perception, as we may for the moment be permitted to term it, is tolerably well understood by physiologists, it is but very imperfectly apprehended by persons in general, even in many instances by those who have paid some attention to the subject. The authors preface their account of the special senses by the following definition:—

"Sensation is an affection of the mind occasioned by an impression made on certain parts of the nervous system, hence called sensitive. A state of the sensitive organs, and a corresponding perception by the mind, must concur to produce sensation: either condition may exist alone, but then the phenomenon is not a true sensation, in the acceptation here given to the word. Thus, light falling on the eye in sleep excites the whole visual sensitive apparatus, while the organ of perception is inactive; on the other hand, in dreams, vivid pictures of objects float before the mind, and are referred by it to the external organ, which may be all the while entirely quiescent." Vol. I., p. 402.

Sensation is thus properly regarded as a phenomenon of *consciousness*, an acceptation of the term which definitively excludes what has been called, after Bichat, organic sensibility. But, although the authors are correct as far as they have gone, they have not explained what it is which, in the act of perception, is presented to the mind; an explanation which is the more demanded, since, in a passage immediately following, it is noticed as a remarkable circumstance that "the organ of the mind itself does not appear capable of undergoing sensory excitement;" or, in other words, of receiving directly impressions from the external objects of sense. The fact is, that in every kind and form of sensation, the mind does not take cognizance of the properties of external objects, but of the altered state of the nerve of sense induced by the impression made upon it, as by light, sound, &c. The truth, which is essential to a just appreciation of the whole matter, is thus clearly stated by Professor Müller in his Physiology: "sensation consists in the sensorium receiving, through the medium of the nerves and as the result of the action of an external cause, a knowledge of certain qualities or conditions, not of external bodies, *but of the nerves of sense themselves.*" In order then to excite the act of perception, the impression must in every instance be made on a nerve of sense; and this being so, the wonder ceases as to the cerebral hemispheres, the seat of all perception, being themselves incapable of perceiving impressions made directly upon them by external bodies. But we learn, moreover, from this, that although many persons conceive that what are called "subjective" and "objective" sensations are essentially distinct modes of sensation, in one case the mind taking cognizance of a condition of the nerve, and in the other of the quality of the external object, they in reality only differ from each other as to the manner in which the nerve is excited; in subjective sensation, a stimulus of an unusual kind is applied, as when the retina is pressed upon by the finger applied to the globe of the eye, or when the auditory nerve is excited by some deranged state of the circula-

tion inducing tinnitus aurium; whereas, in objective sensation, what is considered the natural stimulus is applied.

In describing the organ of taste, the authors have given a most interesting account of the various forms of papillæ placed on the tongue. The most remarkable fact is, that the conical or filiform papillæ, distinguished on the dorsum by their whitish colour, are not only provided with an abundant epithelium frequently composing two thirds of their length, but actually present, at least a few of them, minute hairs, the largest of which are $\frac{1}{10}$ of an inch long, and from $\frac{1}{2000}$ to $\frac{1}{3000}$ of an inch thick. There can be little doubt that these papillæ are subservient rather to touch than to taste: and they may, by their structure and partial mobility, fulfil the office assigned to them by the authors, that, namely, of directing the muscular actions of the tongue during mastication, and thus promoting the almost manual dexterity of the organ in dealing with minute particles of food.

The chapter treating on the Minute Anatomy of the Eye, contains several new facts, which must prove most interesting as regards both the functions and diseases of the various component parts of this most complex organ. In the first place, as respects the cornea, it may not be superfluous to state, although the greater part of this has been for some time known, that it is composed of five coats or layers, clearly distinguishable from each other; these are the conjunctival layer of epithelium, the anterior elastic lamina, the cornea proper, the posterior elastic lamina, and the epithelium of the aqueous humour, or posterior epithelium. The conjunctival epithelium may always be obtained from a fresh eye by gently scraping the surface of the cornea; it consists of three or four layers of superposed particles, inclining to the columnar form where they rest on the anterior elastic lamina. "It is in this epithelium that particles driven with force against the eye generally lodge, and it is easily detached by the instrument used to extract them. Vessels shooting into the cornea in disease* lie under it, and small ulcers are formed by its destruction." Vol. II., p. 20.

The cornea proper, or lamellated cornea, consists of a peculiar modification of the white fibrous tissue, which is continuous with that of the sclerotic. The lamellæ, sixty and upwards in number, formed of these fibres, are intimately united to one another by processes of a similar texture, which produce, however, a very delicate, tubular structure, now for the first time thus described:—

"The resulting areolæ, which in the sclerotic are irregular, and on all sides open, are converted in the cornea into tubular spaces, which have a very singular arrangement, hitherto undescribed. They lie in superposed planes, the contiguous ones of the same plane being for the most part parallel, but crossing those of the neighbouring planes at an angle, and seldom communicating with them. The arrangement and size of these tubes can be shown by driving mercury, or coloured size, or air, into a small puncture made in the cornea. They may also be shown under a high power by moistening a thin section of a dried cornea, and opening it out by needles. The tissue forming the parietes of these tubes is membranous rather than fibrous, though with the best glasses a fibrous striation may be frequently seen, both in the laminæ separating the different series of tubes, and in that dividing those of the same layer from each other. By acetic

* In health it is now known that the cornea contains no blood-vessels.

acid, also, the structure swells, and displays corpuscles resembling those apparent in the white fibrous tissue. Such is rhe lamellar structure of the cornea, which makes it so much easier to thrust an instrument horizontally than vertically into its substance. The tubes or elongated spaces of which we have spoken, are not distended with any fluid, but are merely moistened in the same way as the areolæ of ordinary areolar tissue. A perfectly fresh and transparent cornea is rendered opaque by pressure, but it regains its brilliance on the removal of the compressing force. Some have supposed this to result from the expulsion of fluid from between its laminæ; but that the opacity is owing simply to a derangement of the elementary parts of its structure is plain from the fact, that the same phenomena are exhibited by a section however thin, immersed in water, and deranged by stretching." P. 20.

The authors regard the iris as a process of the choroid, with which it is continuous, although the structure is of course much modified. The connexion of this imperfect diaphragm with the cornea is known to be very firm, but the minute texture uniting them together has not heretofore been investigated. The anterior surface of the iris becomes continuous in the following manner with the posterior elastic lamina of the cornea:—

"This lamina near its border begins to send off from its anterior surface, or that towards the laminated cornea, a network of elastic fibres, which stretch towards the border, becoming thicker as they advance, until at length the entire thickness of the lamina is expended by being converted into them. These fibres then bend backwards from the whole circumference of the cornea, to the circumference of the front of the iris, and are there implanted, passing in this course across the rim of the anterior chamber, and through the aqueous humour. They are seen more easily in some animals than in others, forming a regular series of pillars around the anterior chamber. Behind these there is a more diffused union of the tissue of the iris with the sclerotic, by means of the ciliary ligament." P. 25.

As regards that most interesting point—the muscularity of the iris, it is found, when microscopically examined, that its principal tissue so nearly resembles in its anatomical characters the unstriped muscular fibre, that it may be considered as a variety of that tissue.

"The principal direction taken by the fibres is towards the pupil, although they are more or less meandering and interlacing in this course. Arrived near the pupil, they appear to join, and form indistinct arches. In many instances it is easy to detect a set of circular fibres, either gathered into a principal bundle near the pupil or more diffused, but always lying in front of the others. These seem to answer to the circular fibres of the bird's iris, which are of the striped variety, and occupy the front of the membrane. There may also be usually distinguished in the very thin margin of the pupil an arrangement of fibres more circular than radiating." P. 26.

Placed behind the ciliary ligament and covering the outside of the ciliary body, is a very interesting structure, of a greyish and transparent appearance, which was described by many of the older anatomists, and especially by that admirable writer Porterfield, as muscular, a view adopted by Wagner, and confirmed in the present work by direct microscopic examination.

"It belongs to the unstriped variety of muscle, and its fibres appear to radiate backwards from the junction of the sclerotic and cornea, and to lose themselves on the outer surface of the ciliary body. The more superficial fibres are in contact with, but scarcely adhere to the sclerotic, and are inserted into the posterior part of

the ciliary body; while the deeper ones seem to dip behind the iris to the more prominent parts of the ciliary processes which approach the lens. The ciliary muscle must have the effect of advancing the ciliary processes, and with them the lens, towards the cornea." P. 27.

The authors justly observe that the muscular nature of this *ciliary muscle*, as it is called, is confirmed by its anatomy in birds, where it is largely developed, as noticed by Sir P. Crampton. In this class its fibres, we are informed, are, like those of the iris in the same animals, of the striped variety, and are supplied by ciliary nerves traversing the muscle in a circular direction.

A very beautiful representation is given of the complex organization of the retina, to which we would call the particular notice of our readers. This delicate nervous expansion has of late years been very minutely described, and various layers of grey and fibrous matter have been discovered. According to the authority before us, the following parts are to be detected proceeding *from within, outwards:* 1. A layer of transparent cells, connecting the hyaloid tunic and retina; 2. A grey nervous layer, composed of (*a*), a fibrous lamina, and (*b*), a vesicular lamina; 3. A *granular layer*, divisable by great care into three laminæ; 4. The membrana Jacobi. The great importance of this, the essential part of the eye, induces us to extract the following description of the constituent parts just enumerated:

"The first part of the retina to be described is the *fibrous grey layer*, which forms the immediate continuation of the optic nerve, and which is seated on its inner surface. This is a layer of fibrous character, radiating from the end of the optic nerve, and apparently consisting of the tubular fibres of that nerve deprived of their white substance; that is, being no longer tubular and white, but solid and grey, and united together more or less into a membrane. This at least seems to us to be certain, that the white substance of Schwann does not exist in the nervous substance of the retina, but ceases as the nerve perforates the sclerotic. It has been particularly described as existing in the retina of the rabbit; but the fact seems to be, that in this animal the nerve does not end in the retina till some way within the globe, for after bifurcating and spreading out as a white streak within the choroid, the bundles of nerve-tubes suddenly lose their white lustre, and assume the appearance of the grey fibres of the layer now under consideration. These bundles, both in animals and man, may be seen to anastomose in a close plexiform manner, especially near the optic nerve, and finally constitute a thin sheet, which becomes thinner and less fibrous as we trace it forwards, until at length it can be no longer discerned. This fibrous grey layer of the retina is united to the hyaloid membrane, containing the vitreous humour, by a layer of nucleated cells almost perfectly transparent, and sometimes very difficult of discovery on that account. It is to be remarked, that the fibrous grey layer is the only nervous element of the retina existing over the extremity of the optic nerve where it enters the globe—a spot incapable of vision. Immediately around this spot, the other layers commence which have now to be described, and the first of these is the *vesicular grey layer*. This layer is on the outer surface of the fibrous layer, and so intimately blended with it, that it might almost seem as if the fibres successively terminated in it. The vesicular layer is thicker behind, and gradually thinner forwards. It very accurately corresponds with the vesicular matter of the convolutions of the cerebrum, consisting of a finely granular matrix with interspersed very delicate vesicles, furnished with pellucid globular nuclei of characteristic appearance.

"The blood-vessels of the retina, which are thickly distributed, belong solely to the fibrous and vesicular layers now mentioned. The central artery of the

retina, after entering the globe in the axis of the optic nerve, sends four or five radiating branches, which almost immediately perforate the fibrous layer and spread out in a beautifully arborescent manner, as a capillary network in the substance of the vesicular stratum. After slight maceration, it is easy to wash the nervous material out of the meshes of the vessels; and they then form a vascular layer, but which it is hardly correct to describe as a distinct lamina of the retina. They are merely the nutrient vessels of the part, and are the representative of the close network of the grey substance of the cerebral convolutions. Their wall is a diaphanous membrane with nuclei projecting at intervals, and the meshes average $\frac{1}{400}$ of an inch diameter.

"Behind the vesicular grey layer is the *granular layer*, a term we shall apply to it, because it seems to consist of a close aggregation of small granules, which refract the light more powerfully than the neighbouring parts, and have scarcely any appearance of intervening matrix; they might be regarded perhaps as analogous to the nuclei of cells, and much resemble a layer of granules in the substance of some of the cerebral convolutions, and of the laminæ of the cerebellum. They are made more evident by acetic acid. This layer is divided into two, of which the inner is much the narrower, by a *pale stratum*, which can only be seen by very careful manipulation." P. 30.

Another subject of interest relates to the real nature of the yellow spot or central foramen of Sœmmerring. It only exists in man and the monkey among mammalia, though an analogous part has been found by Dr. Knox in reptiles. It is thus described: this spot consists of a slight projection of the retina, having a minute aperture in the summit, through which the interior of the globe can be seen, when the sclerotic and choroid have been cautiously removed; as to structure, it appears that the fibrous lamina of the optic nerve cannot be traced quite up to the spot, whilst the nucleated cells of the fibrous plexus already described, closely set together, cover its whole surface. This absence of the fibrous element, in a part so exquisitely sensitive as the foramen of Sœmmerring, seems to indicate that, in the operation of the senses, the first and essential impression is made on the grey element of the nervous system; or, in other words, that the peripheral as well as central part of the system is provided with vesicular matter, the nerve-fibres being merely interposed to transmit the impression.

In the chapter relating to the Organ of Hearing, an elaborate account is given of the minute structure of the cochlea, which, to be clearly understood, requires the assistance of the admirable wood-cuts by which the text is illustrated. After describing a peculiar arrangement connected with the osseous portion of the lamina spiralis, and which it is proposed to call the *denticulate lamina*, the authors proceed to notice a semi-transparent structure attached to the membranous zone of the cochlear septum, and which, presenting a muscular texture, is termed the *cochlearis muscle*.

"At its outer or convex margin, the membranous zone is connected to the outer wall by a semi-transparent structure. This gelatinous-looking tissue was observed by Breschet, and is indeed very obvious on opening the cochlea; but we are not aware of any one having hinted at what we regard to be its real nature. The outer wall of the cochlear canal presents a groove, ascending the entire coil, opposite the osseous zone of the lamina spiralis, and formed principally by a rim of bone, which, in section, looks like a spur, projecting from the tympanic margin of the groove, the

opposite margin being very slightly or not at all marked. This groove diminishes in size towards the apex of the cochlea. It gives attachment to the structure in question, by means of a firm dense film of tissue, having a fibrous character, and the fibres of which run lengthwise in the groove, and are intimately united to it, especially along the projecting rim. From this *cochlear ligament*, the cochlearis muscle passes to the margin of the membranous zone, filling the groove, and projecting into the canal, so as to assist in dividing the tympanic and vestibular scalæ from one another, and thus forming in fact the most external, or the *muscular zone* of the spiral lamina. Thus the cochlear muscle is broad at its origin from the groove of bone, and slopes above and below to the thin margin in which it terminates, so that its section is triangular, and it presents three surfaces, one towards the groove of bone, and one to each of the scalæ. The surface towards the vestibular scala is much wider than that towards the tympanic scala, and presents, in a band running parallel to and at a short distance from the margin of the membranous zone, a series of arched vertical pillars, with intervening recesses, much resembling the arrangement of the musculi pectinati of the heart. These lead to and terminate in the outer clear belt of the membranous zone, which forms a kind of tendon to the muscle. This entire arrangement is almost sufficient of itself to determine the muscular nature of the structure. If its fibres were of the striped variety no doubt would remain; but its mass, evidently fibrous, is loaded with nuclei, and filled with capillaries, following the direction of the fibres, and in almost all respects it has the closest similarity to the ciliary muscle of the eye." P. 80.

A structure thus remarkable must have, it is evident, some important office in the mechanism of audition. It is thought by Dr. Todd and Mr. Bowman that the action of the muscle must be that of making tense the membranous portion of the lamina spiralis, and so perhaps of adjusting it to the modifications of sound.

"As the ciliary muscle, though of the unstriped variety, adjusts the transparent media of the eye to distinct vision at different distances under the guidance of the will, so it is not impossible that the cochlear muscle may have a voluntary adjusting power, though its precise mode of action as a part of an acoustic apparatus may still remain obscure. On the whole, however, we are more disposed to regard this very interesting structure as having a preservative office, as being placed there to defend the cochlear nerves from undue vibrations of sound, in a way analogous to that in which the iris protects the retina from excessive light. These nerves are acted on principally by vibrations brought through the osseous part of the cochlea, and it is probable that the arrangement of the scalæ is one designed to allow of protective movements of the lamina spiralis by muscular action, under a stimulus reflected from impressions on the auditory nerve." P. 80.

As we have occupied so much space in noticing the various novel and important anatomical facts contained in these Parts, we are compelled, though with reluctance, to pass by the physiological portion of the work. In the last chapter, the authors enter upon the important function of Digestion; but, as the subject is not concluded in the Part now published, it will be preferable to defer any observations we may have to offer in reference to it, till the entire work is completed. In the interim we must, however, urgently recommend those among our readers who are not already in possession of this "Physiological Anatomy," no longer to delay adding it to their library; a recommendation we willingly risk on the preceding extracts, which, limited as they necessarily have been, are still sufficient to indicate that this valuable treatise is quite indispensable to all who aspire to keep up their knowledge with the high standard of the present day.

I. The American Journal of Insanity. April, 1847.

II. Fourth Annual Report of the Managers of the State Lunatic Asylum, New York, 1847.

III. Remarques sur quelques Etablissemens d'Alienes de la Belgique, de la Hollande, et de L'Angleterre. Par M. *Brierre de Boismont.* [Annales d'Hygiène Publique, T. 37 & 38. 1847.]

Remarks upon some of the Establishments for the Insane in Belgium, Holland, and England. By M. *Brierre de Boismont.*]

Having so recently dwelt upon the various topics connected with the Insane, we should not have again adverted to the subject at present, but that the above-named publications, which are not likely to fall into the hands of many of our readers, contain some well-written papers, of which an account can scarcely fail to prove interesting. In the Journal of Insanity, Dr. Ray, Superintendent of the Butler Hospital for the Insane, whose Essay upon the British Lunatic Asylums we recently noticed, has an excellent critical dissertation upon—

Shakespeare's Delineations of Insanity.

After observing that none but a master-mind can, from a few casual opportunities, present a portrait of the pathological conditions of the human mind with sufficient faithfulness to satisfy the professional observer, he goes on to say:—

"Such a mind was Shakespeare's; and it is because he clearly perceived at a glance those numberless shades of distinction that entirely escape the notice of ordinary observers; that his characters, whether sane or insane, are neither personified abstractions of specific qualities, marked by a name and assigned a part in the play, nor servile copies from life that have lost their interest under the process of transference, but real, mortal men, who live and act before us, and lose their senses it may be, and whose names live after them in the memory of men. His success in this difficult line is to be attributed to that distinguishing faculty of his mind of deducing with wonderful correctness general principles of character from the narrowest possible range of observation. And yet he had peculiar difficulties to overcome. He had not only to divest himself of the popular misconceptions of insanity which regard it as a jumble of intellectual manifestations, acknowledging no principle of cohesion or concatenation, but his opportunities for observing the insane were scanty and imperfect. No friendly asylum furnished subjects for study, whose mental endowments were worthy of his study, and such as he occasionally met at the roadside, or beheld through the bars of their prison-house, were for the most part, it is probable, too far degraded by neglect and unkindness, to be conducive to any poetical purpose. It is not to be supposed, however, that he was guided solely by intuition. He unquestionably did observe the insane, but he observed them as the great comparative anatomist of our age observed the remains of extinct species of animals—from one of the smallest bones, reconstructing the whole skeleton of the creature, re-investing it with flesh and blood, and deducing its manners and habits. By a similar kind of sagacity, Shakespeare, from a single trait of

mental disease that he did observe, was enabled to infer the existence of many others that he did not observe, and from this profound insight into the law of psychological relations, he derived the light that observation had failed to supply. Thus, in spite of all the obstacles in his way, he succeeded to a degree that has seldom been equalled, in representing insanity, both in the form of maniacal wildness and disorder, and that of melancholy dejection and gloom. Its progress through various stages, from the first scarcely perceptible deviation from the soundness of health to its termination in recovery or death, is traced with that thorough fidelity to nature so characteristic of all his conceptions." P. 291.

And thus it is: the more diligently that Shakespeare is studied by the aid of the lights which modern investigations and improvements have shed upon the normal and morbid conditions of the human mind, the more must we arise from such study impressed with admiration at the surprising insight into its innermost workings which his genius supplied him with. Much trash has indeed been set forth under the guise of commentary upon his immortal dramas; and passages in his writings have been twisted and turned to indicate the anticipation of numerous discoveries appertaining solely to a later epoch; but the more they are examined by the aid of reason, knowledge, and experience, rather than of the imagination, the more impressed shall we become with the persuasion that an intuitive knowledge of the laws of thought, and the springs of action of the human mind, was his portion in a degree possessed by no man prior or subsequent to his epoch. Perhaps in no case could this be better exemplified than in his mode of delineating insanity—the mere vulgar idea of which is so untrue and so easily realized, while the nicer touches escape all but the master's eye and hand.

Dr. Ray commences his Essay with an account of the madness of Lear, the picture of which he regards as a master-piece. Lear is eminently predisposed to insanity, being a man of "hot and hasty temper, though endowed with strong and generous passions, of a credulous and confiding disposition, governed by impulses rather than deliberate judgment, rendered impatient of restraint or contradiction by the habit of command, with a nervous temperament, strongly susceptible of the vexations of life, and moreover, with all those moral infirmities aggravated by old age." The early stage or incubation of the insanity is manifested not so much by marked delusions, as by an exaggeration of these peculiarities of character: and we are quite prepared to expect that the mind, which was so ill counterpoised as to banish his favourite child and abdicate his throne from mere unreflecting impulse, will sink under the infliction of the ingratitude of his other daughters, upon whom he had conferred so much; but the effects of this, as displayed in the generous confiding old man's credulity at first, and then his intolerable anguish of mind, mingled with the conviction that his senses are forsaking him, form a series of pictures, which for beauty and truth to nature are unrivalled.

"Unable as the insane are to perceive their own insanity, yet this apprehension of its approach, so frequently repeated by Lear, usually occurs during its incubation. While still able to control his mental manifestations, the patient is tortured with anticipations of insanity; but when he actually becomes so insane, that the most careless observer perceives the fact, then he entertains the most complacent opinions of his intellectual vigour and soundness. And yet this is

one of the nicer traits of insanity which the ordinary observer would hardly be supposed to notice." P. 297.

Dr. Ray observes that Shakespeare has exhibited additional proofs of his observant powers in his delineation of the raving stage of Lear's insanity. This in the ordinary acceptation indicates but a mere arbitrary unconnected jumble of words: but, in point of fact, the ideas which these represent are connected by certain laws of association, although the discovery of them may be no easy matter. During the cerebral excitement, impressions formerly made may be vividly recalled, so as to resemble present realities, and as such are thought and discoursed about by the patient. Such images, too, rapidly succeed each other, and give rise to corresponding ever-changing expression of the thoughts in mania. The condition is very analogous indeed to that of ordinary dreaming, and "mania may with some propriety be designated as dreaming with all the senses open, the morbid excitement rendering the images unusually vivid." Again, the maniac, unrestrained by any sense of propriety or fitness, gives expression to any thought that may present itself; while, in a person of sane mind, a large portion of the thoughts remain unuttered on account of their want of relevancy to the subject in hand. "Every one must be aware how often, in the course of ordinary conversation, thoughts start up having the remotest possible connexion with anything already said—so remote indeed as to defy any one but himself to discover it. Any person who should utter every thought that arose in his mind in the freest possible conversation, would most certainly be taken for a fool or a maniac." Bearing these facts in mind—

"We readily see how there should always be some method in madness, however wild or furious it may be; some traces of that delicate thread, which, though broken in numerous points, still forms the connecting link between many groups and patches of thought. It is in consequence of Shakespeare's knowledge of this psycological law, that in all his representations of madness, even though characterized by wildness and irregularity, we are never at a loss to perceive that the disease is real, not assumed. Not so, however, with most writers, even of distinguished name, who have undertaken to represent the workings of a raving mind. Unaware of the law in question, and governed by the popular notions upon the subject—they seem only to have aimed at unlimited extravagance and incoherence." P. 302.

Lear's recovery from his insanity is depicted with no less consummate skill. He awakes recovered from a refreshing sleep, which the drugs administered to him have procured, but it is at first but dimly and only gradually he comprehends his real position.

"A more faithful picture of the mind when it is emerging from the darkness of disease into the clear atmosphere of health restored, was never executed than this of Lear's recovery. Generally, recovery from acute mania is gradual, one delusion after another giving way, until after a series of struggles which may occupy weeks or months, between the convictions of reason and the suggestions of disease, the patient comes out a sound, rational man. In a small proportion of cases, however, this change takes place very rapidly; within the space of a few hours or a day, he recognizes his true condition, abandons his delusions, and contemplates all his relations in an entirely different light." P. 304.

As Edgar, in the same play, simulates insanity, it furnishes the poet

with an opportunity which he does not neglect to avail himself of, of exhibiting the exaggeration which would be likely to take with the multitude, but not to deceive the experienced observer. "Had it been Shakespeare's design to exhibit a case of real demonomania, or of chronic mania, we should unquestionably have had something very different from the part of Edgar. If the former, we should not have found the patient talking so clearly about his own case, while indulging in unlimited incoherence and rambling about everything else; and if the latter, we should not have seen a strain of acute morality succeeded, more than once, by a trait of mental imbecility."

Dr. Ray devotes a large space to the consideration of that noblest of all plays, HAMLET, and brings forward cogent reasons for gainsaying the gratuitous conclusions of those commentators who maintain the prince's insanity was feigned. Into his analysis of these opinions, founded as they are for the most part on an imperfect apprehension of the nature of the disease, we have not space to enter: but certainly the difficulties which are supposed to attach to the interpretation of some portions of the drama seem to us most easily soluble upon the hypothesis of the reality of the insanity. The true character of Hamlet's madness is thus set forth:

"In Lear we are presented with the origin, progress, and termination of a case of acute mania—that form of mental disorder in which the mind becomes, at last, completely unsettled, and all its operations pervaded by discord and confusion. Hamlet's insanity differs from Lear's in not having the successive steps of its progress so well-marked and regular; in presenting less incoherence of thought, and less nervous excitement. In his case, acute general mania like Lear's would have been incompatible with that degree of forecast and self-control which the character required; and simple monomania, where the sphere of the aberration is a very limited one, the individual, for the most part, observing the ordinary proprieties and courtesies of life, would have been equally out of the question, because it would not have exerted the requisite influence over the action of the play. With great skill, therefore—a skill founded on what would seem to be a professional knowledge of the subject—Shakespeare has selected for his purpose that form of the disease in which the patient is mad enough to satisfy the most superficial observer, while he still retains sufficient power of reflection and self-control to form and pursue, if not to execute, a well-defined, well-settled purpose of revenge. In order the better to understand the conduct of Hamlet, we should bear in mind that he was a man of warm affections, refined tastes, and a quick sense of honour, and possessed of a high order of intellectual endowments. With these simple elements of character the manifestations of disease are made to harmonize and blend so intimately together, that it is not always easy to distinguish between them." P. 313.

Rendered sad and gloomy by the death of his father, and the marriage of his mother, the subject of suicide had already anxiously occupied his thoughts, when the bloody secret revealed to him by his father's ghost induces a temporary flightiness and levity of manner too plainly indicative of its disordering effect upon his faculties. His subsequent interviews with Ophelia and Polonius are markedly indicative of insanity; while before his friends Rosencrantz and Guildenstein, who he suspected were bent upon watching him, he makes no attempt to prove the existence or reality of his madness—favourable as the opportunity was for simulation. He calmly describes to them the perverted condition of his feelings which had of late distressed him.

"'I have of late (but wherefore I know not) lost all my mirth, foregone all custom of exercises, and, indeed, it goes so heavily with my disposition, that this goodly frame, the earth, seems to me a sterile promontory; this most excellent canopy, the air, look you, this brave o'er-hanging firmament, this majestical roof fretted with golden fire, why, it appears no other thing to me than a foul and pestilent congregation of vapours.' A most faithful and vivid picture is this of a mental condition that is the precursor of decided insanity—the deepening shadow of that steadily advancing eclipse by which the understanding is to be darkened. In Hamlet the disease has not yet proceeded so far as to prevent him, in his calmer moments, from recognizing and deploring its existence, though he mistakes its character. Like every other person in his condition, he is very far from considering himself insane, and indeed there is no reason why he should. He entertains no delusions; persons and things appear to him in their customary relations; and, for the most part, he well sustains his character as a man and a prince. His unwonted excitability of temper, his occasional disregard of some minor propriety of life, the cloud which envelopes all outward things, depriving them of their worth and beauty—in the eyes of the world these do not constitute insanity, and are not incompatible with the most perfect integrity of intellect. Why, then, should he suppose himself insane, or beginning to be so? Such a mistake is very natural to the patient, but when made by others, who vaunt their knowledge of mental pathology, it proceeds from a less excusable kind of ignorance." P. 318.

The test of his sanity which Hamlet proffers in answer to the charge of madness—

> "It is not madness
> That I have uttered; bring me to the test,
> And I the matter will re-word which madness
> Would gambol from,"

is undoubtedly satisfactory enough in many cases, and was employed and commented upon with ability by Sir Henry Halford; but as Dr. Ray truly observes, that although "in most cases of *acute mania*, attended with much excitement, as well as in that form of mental impairment called *dementia*, the patient would be unable, no doubt, to repeat what he had just before deliberately uttered, but in such cases as Hamlet's, where some of the mental operations are perfectly well conducted, the power of repeating correctly one's own statements is not necessarily lost, and consequently is no proof of sanity in doubtful cases."

In allusion to that beautiful creation in the same play, the insanity of OPHELIA, in consequence of the utter wreck of her hopes and affections, Dr. Ray observes:—

"In this play, for the first and only time, Shakespeare has ventured on representing the two principal characters as insane. His wonderful success in managing such intractable materials the world has long acknowledged and admired. They are never in the way, and their insanity is never brought forward in order to enliven the interest by a display of that kind of energy and extravagance that flows from morbid mental excitement. On the contrary, it assists in the development of events, and bears its part in the great movement in which the actors are hurried along as if by an inevitable decree of fate. Herein lies the distinguishing character of Shakespeare's delineations of insanity. While other poets have made use of it chiefly to diversify the action of the play, and to excite vulgar curiosity by its strange and striking phenomena, he has made it the occasion of unfolding many a deep truth in

mental science, of displaying those motley combinations of thought that are the offspring of disease, and of tracing those mysterious associations by which the ideas of the insane mind are connected. Few men, I apprehend, are so familiar with those diversities of mental character, that are in any degree the result of disease, as not to find the sphere of their ideas on this subject somewhat enlarged by the careful study of Shakespeare." P. 324.

We have not space to accompany our author in his examination of the hallucinations of MACBETH and the shattered mental constitution of his ambitious wife. After alluding again to the inferiority and inartistic characters of the delineations of insanity by other dramatists, in which all individuality of character is merged in the mere vulgar and conventional idea of insanity, with all its exaggerations and errors, he thus concludes his interesting Essay:—

"In this review of Shakespeare's Delineations of Insanity, I trust I have made it appear in some measure how their wonderful fidelity to nature renders them not only valuable as pathological illustrations, but wonderfully effective in producing a dramatic impression. Great as he is in every other attribute of the poetical character, yet in this department of the art, he seems to be without a rival. No other writer, unless we except Sir W. Scott, has made the slightest approach to his success. In several of this writer's works, the workings of a disordered mind are displayed with the hand of a master, and that too with a degree of pathological accuracy which ordinary men would hardly acquire by years of observation within the precincts of a hospital. But the moralist possesses an advantage over the poet in the broader limits within which he may exercise his art, untrammelled by the restrictions enforced upon the other by severer rules of composition and the comparative brevity of his efforts.

"I have already intimated that, in his knowledge of insanity, Shakespeare was greatly in advance of his own and succeeding generations; and that this was owing not to any superior advantages he possessed for the study of the disease, but to an extraordinary power of observation which, more than any other mental attribute, perhaps, deserves to be considered as the true inspiration of genius. It needs but a glance at the common views of insanity that prevailed in his own, and even later times, not merely among the rude and uneducated, but among men of distinguished names, to show how little they evince of his profound science of mind. By a profession which has always numbered in its ranks a large proportion of the luminaries of the age, the insane generally, with the exception of such as were actually raving, or reduced to a state of idiotcy, were regarded as having reason enough to conduct themselves with tolerable propriety, and made responsible for their actions to a degree that would startle the criminalists of our own time, ready as most of them are, to look upon the plea of insanity as the last resort of ingenious counsel. Sir M. Hale declared, many years after Lear was written, that insanity affects only the strength and capacity of the mind, and upon this idea he has founded a test of responsibility. 'Such person as labouring under melancholy distempers, hath yet ordinarily as great understanding as ordinarily a child of fourteen years, is such a person as may be guilty of felony or treason.' These views, it is true, belong to a province of insanity somewhat remote from that which engaged Shakespeare's attention, but there can be no difficulty in inferring from his delineations of the disease, in what light he would have regarded them. Can we suppose, for instance, that if the question of the responsibility of Hamlet for killing Polonius had been referred to him, he would have pronounced him guilty of murder in the highest degree, because he possessed more understanding than a child fourteen years old? Had the jurist, in forming his opinions on this subject, meditated upon the pictures of Shakespeare as well as the principles of Coke and Lyttleton, it would have been better for his own reputation, and better—ah! how much

better—for the cause of humanity. Would that we were able to say that the Courts of our times have entirely avoided this error, and studied the influence of insanity upon human conduct more by the light of Shakespeare and of Nature, than of metaphysical dogmas and legal maxims." P. 332.

Dr. Brigham on the Medical Treatment of Insanity at the New York State Lunatic Asylum.

Dr. Brigham having been frequently questioned upon this subject supplies a short paper to the present volume of the "Journal." He observes that, as no specific treatment exists, each case is submitted to remedies appropriate to its stage, *recent cases* usually requiring a mild antiphlogistic treatment. Much depends upon the cause. Thus, if the disease arises from any direct physical injury to the head or violent mental commotion, free bleeding and purging are indicated. Such cases, however, are seldom seen in lunatic asylums; and only four of the 622 patients were bled during the last year, in one of whom only did benefit seem to accrue. Cerebral excitement is occasionally treated by local depletion; but generally by placing the feet in warm water, applying cold to the head and the administration of purgatives. One of the most certain means of subduing maniacal excitement is the pouring cold water from a height of four or five feet directly over the head; but it must be gently executed, only for a short period, under the immediate superintendence of the physician, and never when the bowels are confined or the stomach full. The warm bath, continued for half an hour or longer, cold being simultaneously very gently applied to the head, exerts also a very calming effect in many cases. In most cases of insanity warm bathing is beneficial; and cold bathing and the shower-bath might be resorted to more frequently, but that the patients are apt to regard them as intended for punishment.

Emetics and *cathartics* having proved of little service are now seldom prescribed, guarding against constipation, however, by diet and laxatives. In some recent cases Croton oil has proved very efficacious, apparently effecting a cure. It is the easiest of all medicines in its administration, and has led to no unpleasant results.

Blisters, *issues*, and especially *setons* in the neck, have been frequently tried, but rarely effected anything but incidental good by diverting the attention of the patient from imaginary sufferings and delusions.

Great success has frequently attended the use of *opium*. In some cases it appears useless, and in a few injurious, especially when the skin is hot and dry, and the pulse full and hard. Although not perhaps frequently curative it is a valuable adjuvant, securing an advantageous calm not otherwise procurable. "In some cases, however, it seems of itself to effect a cure; of this we can have no doubt, after having seen many patients apparently recover while taking it freely, and immediately relapse on its being withheld, and again recover under its use, and finally, after continuing it for a considerable time and gradually diminishing the dose, recover and remain well for years without it." Dr. Pritchard, who formerly spoke very disparagingly of this remedy, in a late edition highly commends it. The following formula is employed in many nervous, sleepless, and

hysterical cases. ℞. *Trœ. Lupuli.; Trœ. Hyoscy.* āā ʒ iv.; *Camphoræ* ʒ j.; *Ol. Valerian.* ♏ xxxij. Dose 1 to 2 drachms. The following is also useful in some cases of violent mania, especially, as is often the case, when the secretion of urine is defective. ℞. *Trœ. Digital.; Trœ. Scillæ,* āā ʒ ss.; *Vin. Ant. Tart., Spt. Nitræ Dulc.* āā ʒ j. M. Dose 30 to 60 drops.

Many cases, particularly if they have been of long duration, require *invigorating diet and tonics;* and many patients are brought to the institution seriously injured by the depletion which their maniacal excitement and great muscular efforts have unfortunately given rise to. The usual tonics are prescribed; but from none is a greater advantage derived than the following combination. *Extract. Conii* ʒ vj.; *Ferri Carb. Præcip.* ʒ xij.; *Molasses, Wine, Warm Water,* āā *two quarts; Ol. Sassafras* ʒ ij. *dissolved in Alcohol* ʒ viij. M. The usual dose is ½ oz. ad 1 oz. adding a little Tr. Aloes and Myrrh to each dose when a laxative is required. The following combination also has been found "quite serviceable" in many cases of debility and loss of appetite. ℞ *Trœ. Cinchon. Co.* ʒ j.; *Trœ. Gentian.* ʒ iij.; *Trœ. Capsici* ʒ ij.; *Sulph. Quin.* ʒ ss.; *Acid. Sulph.* ♏ xv.—M. A drachm to be given in water or ginger tea.

Insanity is often complicated with other affections. *Nocturnal emissions* are not infrequent and are very injurious, for which large doses of the *Trœ. Ferri Muriatis* seems the best remedy. So, too, *Passive Menorrhagia,* which is often obstinate and seemingly sometimes a cause of the insanity, may be treated by the same, or better still by *Tr. Cinnamom.* and *Tr. Aloes,* of each from 20 to 30 drops.

"It should ever be borne in mind that disease in the insane is very apt to be masked,—that serious disease of the lungs, or of some of the abdominal viscera may exist, but without being manifested by the usual symptoms, and may therefore be overlooked without careful examination. In other respects not particularized in these remarks, we are not aware that the diseases of the insane require different treatment from those of the sane."

Dr. Brigham's Report of the New York State Lunatic Asylum contains some interesting remarks on some of the physical characteristics of the insane.

Frequency of Pulse of the Insane.—Dr. Brigham has recorded the frequency of pulse in 1234 cases, giving the following results. From 40 to 50 in 8; 50 to 60 in 22; 60 to 70 in 183; 70 to 80 in 233; 80 to 90 in 466; 90 to 100 in 144; 100 to 110 in 124, and 110 to 120 in 54. Examining the pulse in 76 sane persons he found it from 60 to 70 in 6; from 70 to 80 in 47; and from 80 to 90 in 23. Both the sane and insane individuals were in a calm state and sitting posture. "Age seems not to have much influence upon the rapidity of the pulse, as a few of the most aged were found to have a rapid pulse. Those who have recently become insane, most generally have a frequent pulse, above 80 in a minute, though there are exceptions to this, as in some such cases the pulse is remarkably slow."

Size and Shape of the Head.—Dr. Brigham instituted careful admeasurements of the heads of 1163 insane persons (604 men and 559 women) and of 82 sane persons (45 men and 37 women). They were measured

around in three directions: 1, around the head, its greatest circumference; 2, from the opening of one ear, over the head, to the other; 3, from the root of the nose or lower part of the forehead, to the nape of the neck or occipital protuberance. These two last were found to be nearly alike in the same individuals; and, with few exceptions, the heads which were largest in one direction were so likewise in the other. In eight only of the 604 insane men did the largest circumference extend to 24 inches, and the other directions to from 14½ to 15½. In 314 the largest circumference was more than 22 inches, and in 282 from 21 to 22 inches. In 5 of the 45 sane men only did this extend to 23½ inches, and in 18 of the number it was less than 22—so that no material difference was observed in the size of the sane and the insane head. In both insane and sane women the highest figure reached by the largest circumference was 22½ inches. In 350 of the former, and 19 of the latter, it was less than 22 inches.

Weight of the Insane.—Dr. Brigham likewise presents us with tables of the weights of 1007 insane persons, and believes this point has been too much overlooked. We do not consider any importance is to be attached to any such account of the absolute weight of the insane—every possible latitude of this being perfectly consistent with complete health of body and mind; but we quite concur with him in the following observations upon the indications to be drawn from variations in the weight subsequent to admission.

"We have practised weighing each patient upon admission and occasionally afterwards, and I think we have derived considerable advantage from this custom. It is sometimes a valuable guide in prognosis, and often affords amusement and encouragement to the insane themselves when they find, contrary to their strong convictions, that instead of losing, they are increasing in flesh.

"A majority of the insane, when committed to our care, are less fleshy than natural. They have become more or less emaciated by disease, or by their imaginary troubles. Usually they regain flesh when they begin to recover, and frequently weigh more after complete restoration than at any other period of their lives. Some in the course of a few months have gained from 30 to 40 pounds. In recent cases of insanity we usually predict recovery when patients begin to increase in flesh, especially if at the same time there is some improvement of the mind. On the contrary, where the digestion and sleep and appetite are natural, and the patient increases in flesh, without any diminution of insanity, there is little hope of recovery; also, if the appetite continues good and emaciation increases, there is reason to fear an unfavourable result."

We have often thought that practitioners are too neglectful of the information which statistical data would furnish to them. Every one must have at times felt the great desirableness of being assured whether the patient is gaining or losing flesh; while the indefinite and frequently inaccurate statements furnished upon this head (especially by women who are continually confounding mere distension with increase of substance) may often mislead. In all chronic cases, a correct register of the patients' weight, as ascertained at periodical intervals, should be kept, the timid or wavering not being necessarily made acquainted with the results.

Hereditary Predisposition.—This, Dr. Brigham believes, exerts more influence in the production of the disease than all other causes combined.

"It does not of itself excite the disease, but when it strongly exists, a trivial cause will develop it. Thus, most of the supposed exciting causes in the foregoing table would, of themselves, be inoperative, if there was not an inherited constitutional tendency to insanity." The children may escape, and the grand-children suffer. Careful enquiry has shown that insanity is a little more likely to be transmitted by the mother than the father, and that the mothers are considerably more likely to transmit it to daughters than to sons; while the fathers most frequently transmit it to sons. Thus of 79 men, 42 had insane fathers and 35 insane mothers, and in two both parents were deranged; and of 96 women, 37 had insane fathers and 56 insane mothers, three inheriting the predisposition from both parents. "When children resemble in personal appearance the insane parent, and manifest the same peculiarities of feeling and temper, there is reason to apprehend they will be more or less disposed to the disorder of the parent they resemble." Of 1181 patients (594 m., 587 w.) 315 were known to have insane relatives, and in 175 cases the parents were the relatives. Many others would probably evidence the same predisposition were their history more accurately known. Dr. Brigham has, however, contrary to the opinion of most, found the inherited form of insanity as curable as any other, but very liable to relapse from slight and various causes. Individuals so predisposed may have repeated attacks, each one from a different exciting cause. The education of individuals having this predisposition is of vast importance.

"The early education of all such requires much attention. Great pains should be taken to form a character not so subject to strong emotions, to passion and caprice. Among the most frequent causes of insanity in those not predisposed to it, is the over-indulgence of the appetites and passions in early life; and to those who inherit a tendency to this disease such a course is highly pernicious. The utmost attention should be given to the securing a good bodily constitution. Such children should be confined but little at school: they should be encouraged to run about the fields and take much exercise in the open air, and thus ensure the equal and proper development of all the organs of the body. They should not have the intellect unduly tasked. Very early cultivation of the mind, and the excitement of the feelings by the strife for the praise and honour awarded to great efforts of mind and memory, are injurious to all children, and to those who inherit a tendency to nervous diseases or insanity, most pernicious."

Suicidal Form of Insanity.—Of 1181 patients admitted into the New York Asylum, 156 (63 m. and 93 w.) were disposed to suicide. "It is however a consoling fact, that this alarming variety of insanity is quite often a curable one. Among the most complete and permanent recoveries we have ever known, are a considerable number who, for several months, were very strongly inclined to self-destruction."

Homicidal Insanity.

The Report contains many interesting observations upon this all-important subject. Dr. Brigham believes the homicidal insane may be arranged into six classes, differing in their mental condition, as indicated by the motives which actuate them, or the circumstances which accompany the deed. So unsettled are the views of the public and of the legal and

medical professions upon this subject, that we are glad of the opportunity of recording any remarks of so acute and experienced an observer; and certainly the first step towards a more accurate comprehension of these cases would seem to be a more distinct apprehension of their distinguishing traits. The classes indicated by Dr. B. are as follows: 1. *Those who take life in a paroxysm of insane passion or fury.* Insanity so frequently induces excessive irritability of temper, that every asylum contains patients who are disposed to assault or kill. Most of the cases related in the newspapers of insane persons at large killing their relatives or friends during a gust of passion belong to this class. In general, other acts have previously indicated insanity on the part of the perpetrator; although occasionally cases occur in which it is difficult to decide whether the act results from insanity or depravity and violent passion.

"Usually, however, in these somewhat doubtful cases, it will be found that those who ought not to be considered responsible for their actions, by reason of insanity, have suffered from some severe illness, or from some great mental disturbance, since which time their temper and disposition have undergone a marked change. When no such facts are found to exist, nor any other evidence of mental disorder, we think the act itself committed during violent passion, is not proof of insanity, though it may have been committed under such aggravated provocation as to render it, in public opinion, justifiable homicide."

In the *second* class are placed those *who commit a homicide from delusion*, who are deceived or misled by their hallucinations, illusions, or disordered imaginations; and this is the form of homicidal insanity which is more prevalent than any other; the actuating motive being often benevolence towards the victim sacrificed to the delusion.

The *third* class is made up of "those who kill indiscriminately and apparently from a love of taking life; from a diseased propensity and conscious desire to destroy others, against which act neither reason nor conscience remonstrates." Some of those persons seem almost constantly desirous of injuring or killing others: they are conscious of this; but are actuated by no motive, malice, or passion.

In the *fourth* class we have persons "who kill without any apparent motive, from a sudden impulse, but of which they are not conscious, and who retain no recollection of anything that prompted them to the act. Those belonging to the *third* class, have an intense desire to kill, of which they are conscious, and are usually evidently insane in other respects; but those belonging to the *fourth* class kill from a sudden impulse, without any desire or conscious feeling relating to the act, and not unfrequently *the act itself is the first noticeable evidence of their mental derangement.*"

The *fifth* class is composed of persons who commit the crime without motive, from an irresistible impulse, of which they are conscious, and against which reason often remonstrates. Gall, in his work upon the brain, instanced several cases in which the insane implored that they might be protected against the effects of their propensities, and similar ones abound in works upon insanity.

To the *sixth class* belong such as kill from imitation, or an insane love of notoriety.

After this analysis of the cases that have come under his notice, Dr. Brigham adverts to the difficult question of legal responsibility, concern-

ing which, opinions seem to be as unfixed and practice as uncertain in the United States as in our own country. These cases, in fact, often form the most embarrassing ones which can come before a jury, even now that the public and the legal profession have acknowledged, in many instances, the existence of moral insanity; for, while humanity forbids the condemnation of an unfortunate individual whose only crime is disease, the protection of the public from the consequences of a too easy reception of proofs of the existence of this, is a duty no less urgent, and humanity no less genuine. The present strong feeling prevailing in this country against the validity of the punishment of death even in cases of murder, contributes another element of difficulty—juries finding insanity in many cases in which they otherwise would not, were they not fearful of entailing death as a consequence. So much is public opinion divided upon these questions, that it is seldom that the decisions of our tribunals give that general satisfaction, which is so commonly the case in relation to other subjects.

"The great difficulties and mistakes that have occurred on this subject," Dr. Brigham observes, "have arisen, we believe, from courts attempting to define, what cannot be correctly defined, viz. insanity; and to establish what does not exist, a certain test of insanity by which the existence of this disease can be recognized. This statement, we think, is fully confirmed by a brief history of the attempts of courts to define insanity, and to furnish juries a rule to guide them in determining the question of responsibility."

The old doctrine, as laid down by Coke and Hale, was, that for exemption on the ground of insanity, a *total deprivation of memory and understanding* must exist. Erskine admitted this on the trial of Hatfield in 1800; but his client was not in this predicament, and was acquitted on the ground of the existence of *delusion*, this being therefore established as the true character of insanity. However, it did not save Bellingham ten years later, who, had he been tried in our own times, would have certainly been acquitted as insane. The test laid down on that occasion by Gibbs, and admitted by Mansfield, was the ability of the accused to distinguish right from wrong, even though he might be incapable of conducting his own affairs.

"Since then the law, that is the decisions of the higher courts of England, have been loose and fluctuating. Generally the ability to distinguish right from wrong has been insisted upon more or less strongly. Not unfrequently, however, it has been partially or entirely abandoned, as it must have been on the trial of M'Naughten for killing Mr. Drummond in 1843. In this case it was found that the prisoner had good memory and understanding, and was capable of transacting business correctly, and there was no evidence that he did not understand the distinction between right and wrong at the time he committed the act for which he was tried and justly acquitted.

"Within a few years the English House of Lords have endeavoured to obtain from the Judges of the Law Courts their views of the law relating to insanity, or rather a legal definition of insanity, which should henceforth furnish a rule for the guidance of courts and juries whenever in criminal cases the plea of insanity might be set up. The answer of the Judges to the questions propounded to them must be regarded as a signal failure in this respect. Their answer was in fact but the repetition of the dictum we have mentioned, *that a man is responsible if he is capable of knowing right from wrong*. As was predicted at the time,

it was disregarded immediately after, as it had been in M'Naughten's case just before. It was not at the time satisfactory to some of the most enlightened members of the House of Lords. Lord Brougham complained that the test was vague and unsatisfactory, and doubted whether the juries before whom the question is tried really comprehended what is meant by it. It has been disregarded in England in numerous instances since."

Certainly the solemn assemblage of the judges which took place on the occasion in question was one of the happiest examples of the Mountain in Labour which modern times have afforded. That conclusions in which nothing was concluded should follow the deliberations of a body of men so little accustomed to an enlarged contemplation of human nature, and so religiously respectful of precedents and technicalities, is far less surprising than would have been the laying down any intelligible and comprehensive principle.

The decisions in the U. S. Courts alluded to by Dr. Brigham, seem to be nearly as uncertain and unstable, as guides for the future, as our own. In reference to the test already alluded to, he adds :—

"Although judges appear to think that it is easy for juries to decide whether the prisoner was able to distinguish right from wrong at the time, we are of opinion that there are few more difficult. Separate from the fact that no being short of Omnipotence always knows right from wrong, and that what is right in one age or nation is considered wrong in another, *how* are the jury to decide that, at the precise moment of committing a homicide, a man can or cannot distinguish right from wrong? What kind of evidence establishes this? The legal presumption is, that a man does know; but in case he does not, how is this to be proved! We certainly do not know, and believe, with Lord Brougham, that 'juries do not really comprehend what is meant by the question.' * * * *

"We have endeavoured at various times to ascertain from the insane themselves their ability to distinguish right from wrong, and for this purpose have questioned on the subject in various ways several hundreds, and we cannot better exhibit its inapplicability as a test of their responsibility, than to say what the truth enables us to say, that a large proportion of the insane now in asylum appear to understand its distinctions as well as persons in the community at large. We also believe this to be true of most of the insane we have seen elsewhere; and we confidently assert that this will be found to be the opinion of every person who has had the charge of an institution for the insane, and seen many deranged persons. But the cases we have already mentioned show the utter worthlessness of this as a test of the responsibility of the insane. One of the women killed her own child for the purpose of procuring her own death by execution. She certainly knew she was doing a wrong act, and one for which she expected to be punished. Others commit homicide from a sudden impulse, who have not previously exhibited any wrong conduct or intellectual aberration. A mother kills her only child, or a man his wife and family, to whom he is known to be ardently attached; or a child a parent, as in the instances we have mentioned. To such cases none of these tests of insanity apply, and yet they are among the most palpable and unquestionable cases of that kind of mental disease which should be an excuse for criminal acts.

"Notwithstanding the great improvement within the last fifty years, that has been made in the treatment of the insane, owing to an increased knowledge relating to insanity, it is still a fact that some of the most deplorable forms of this disease are not even now allowed to annul responsibility. Although it is the law of this and every civilized country, that it is the reason of man which makes him accountable for his actions; and that the deprivation of reason acquits him of crime; yet it is a lamentable fact, that at the present time, courts of justice do not acknowledge the existence

of some of the worst forms of mental alienation, and which, in the opinion of those acquainted with the insane, are as well established as any other. In the words of an enlightened Judge of this State, 'the law upon the subject of Insanity in its slow and cautious progress still lags far behind the advance of true knowledge.' * * * * * * Far from us is any desire to screen the guilty by the plea of insanity, and no one can regret more than us to see this plea improperly set up, as this must tend to jeopardize it when justly made. But we cannot resist the conviction that, by the different constructions given by courts to the law of insanity, that the rights both of the insane and of the community may be in danger. That in one case, when popular feeling is much excited against the accused, the strictest rules of law and the severest tests of insanity known to the common law of England are enforced, and notwithstanding strong proof of insanity conviction follows; while in another case, with little proof of insanity, but with popular sentiment in favour of the prisoner, other constructions are given to the law, and he is acquitted."

We have quoted much more largely from this Report than is customary with us in respect to similar documents: but the contents, as our readers may now judge, are of no ordinary value.

M. Brierre de Boismont on Establishments for the Insane in Belgium, Holland and England.

The opinions of foreigners upon our institutions, when matured by sufficient information and delivered with candour, are always valuable and acceptable, confirmatory as they frequently are of advantages already enjoyed or suggestive of desirable improvements. M. Brierre, already well known to our readers as an able practitioner in this department of medicine, has recently paid us a flying visit, and has published in the "Annales d'Hygiène," the impressions which his inspection of the metropolitan asylums produced. Before noticing these, however, we may advert to the account he furnishes of the singular Belgian establishment at *Gheel*, which he visited during the same tour. He reports that the provisions for the insane in Belgium and Holland are lamentably defective and far behind those of the rest of Europe in all the appurtenances for comfort and treatment.

The Insane Colony at Gheel.—Gheel, although still termed a village, is well entitled to the appellation of a town, inasmuch as it numbers from 7 to 8000 inhabitants, and is now a flourishing place, forming a marked contrast with its wretched condition as described by Esquirol a quarter of a century since. The entire number of lunatics dispersed over the village and adjoining hamlets amounts to about 800; the sum paid for them varying from 624 to as low as 24 francs per annum. Taking the mean of the entire number, the daily cost per head is only half a franc. The sum fixed by authority is from 170 to 200 francs per annum. A very limited number who live with the better classes of citizens, pay from 15 to 1800. On examining the abodes of the peasants in which the lunatics resided, M. Brierre found them clean and comfortable, one patient only for the most part being consigned to each

house. As the peasants are very anxious to retain these guests, and consider their removal, at the instance of the Inspector, casts a stigma upon them, it becomes their interest to treat and feed them well. There is, however, no special place for the treatment of the patients, and no medical treatment, save for incidental maladies, is put into force, while coercion is employed in certain cases to an extent utterly discountenanced in the rest of Europe. Thus, although the mass of the patients have liberty to go out and in when they choose, and to walk as freely about as the sane, M. Brierre observed some of these had heavy rings on their legs or hands just like galley-slaves, such having heretofore made attempts at escape: so too in the houses he observed large iron rings for the attachment of restive patients. At the time he visited Gheel there were not more than twenty under any restraint, and this was above the usual number. He heard no cries of suffering, even during the silence of a very hot night. Several of the patients pursued occupations, the majority, however, being unable or unwilling to work. The sojourn of these patients does not seem to have operated injuriously upon the inhabitants of the place, who become very rarely insane, and that usually from the ordinary causes. The promiscuous intercourse of the insane of opposite sexes also has not given rise to the production of illegitimate births—a circumstance which the author explains by the active surveillance the peasants maintain, and the moral and phlegmatic character of the Flemings. It is evident that, although the lunatics are well and kindly treated, just as if they were members of the respective families they reside in, there their advantages end. No attempt at physical or moral treatment is made, and no appliances for these exist; and the colony can be looked upon as little else than a comfortable abode for incurable patients.

"However we may reply to the questions of what are the advantages or inconveniences of this establishment; whatever benefits accrue from it, and whatever may be its future destination, we at least cannot but remark that, while the insane in France and England and many other countries, were imprisoned, chained and buried in cages and huts just like wild beasts, there had existed for ages in a lonely and unknown part of Belgium an establishment where hundreds of the insane were free and abandoned to themselves. Simple good sense had revealed to these poor peasants that mildness and kind treatment should form the basis of their relations with this strange description of guests, and that force should not be resorted to save in case of danger."

The English Asylums.—Dr. Brierre's attention was confined to Bethlem, St. Luke's, Hanwell, and one or two private asylums. His general opinion of *Bethlem* is highly favourable, save in respect to its situation within the metropolis. Among the few observations he makes of a qualificatory character are the following:—

"There is nothing particular in the medical treatment adopted. In the galleries there is a small apartment with the warm, cold, and surprise baths; but its narrow dimensions would only admit of one or two being bathed at a time; and we have the full conviction that this means, although employed, is not so frequently applied as in France. Medicinal substances seem to consitute a large portion of the treatment, and I do not wish to deny their importance; but attendance upon great numbers of the insane leaves no doubt in my mind of the greater importance of

baths in this affection; and I have elsewhere* shown that, by their aid, we can abridge the duration of certain forms of insanity, but for this end many baths and appliances are requisite.

* * * * * *

"At the present day, when the rules which should govern this description of establishments are agreed upon, there is no physician who would not admit that, ere long, this of Bethlem must be removed away from the metropolis to some agreeable locality. Another important amelioration would be the *residence of the physicians in the establishment itself*; but for this purpose a liberal salary should be accorded to them. Another measure, no less desirable, would be the appointment of certain special pupils, who should pass several years within its walls, and for whom it would be but just to secure appointments to provincial asylums upon their leaving this one. The conduct of France, in this particular, seems to me deserving of imitation; for almost all the asylums founded by the law of 1838, have been entrusted to physicians, whom M. Ferrus, Inspector-General of Asylums, has selected from those who have exclusively devoted themselves to the study of mental disease.

"The number of patients to be consigned to the care of the physician is not a matter of indifference; and if we consulted our own experience we would limit this to 100, 70 chronic and 30 acute cases."

Dr. Brierre gives a somewhat detailed account of the Criminal Wing at Bethlem, as being likely to prove most interesting to his readers in France, where it seems no such provision for the separation of this description of lunatics prevails. At the period of his visit there were 97 patients (77 m. and 20 w.,) the cause of whose detention are thus distributed. Crimes against the State, 2. Crimes against the person, 63. Crimes against property, 32. "I may be mistaken, but I have the conviction that the prospect of a sojourn in such a place would be more likely to deter a true criminal than the perspective of the scaffold."

Quite agreeing with the author, that the Criminal Lunatics should be separated from those of other descriptions, we would carry the principle farther still, and have a separate establishment under the control of Government, erected for them in a part of the country remote from the metropolis. Their detention at Bethlem is mischievous to the other patients, by reason of their more noisy dispositions, the prison-like aspect they give the place, and the limitation of accommodation they entail. It is no less injurious to the public interests. Located in an establishment under the governance of the city authorities, who have free ingress to it, they become a sort of raree show to all the fashionable idlers and visiters of the metropolis, for whose morbid appetites our worthy citizens have ever shown themselves most desirous of catering, whether by the exhibition of the poor wretches alluded to, or by the revolting proceedings at the condemned sermon or condemned cell in Newgate. It is true public opinion has of late sternly condemned these procedures, but we have no security of their non-revival but the removal of such high trusts from such incompetent hands. The mischief done to the moral sense of the community, and the stimulus given to the morbid desire of imitation so rife in the

---

* For an account of M. Brierre's opinions of the curative agency of prolonged warm baths in acute insanity, see Rev. Medicale, Nov. 1847, and Med.-Chir. Rev. N. S., Vol. V., p. 282.

insane and in those verging on insanity, by the recital in the newspapers of the various circumstances alluded to, should long since have secured the interference of the State.

While praising the great cleanliness (indeed he passes great encomia on this important item of management, as manifested by the utter absence of bad smells, in all our establishments) and the general good management at *St. Luke's*, M. Brierre properly maintains that, in so limited a space and so confined a situation, it is impossible for the patients to profit by the progress of improvements in this branch of medical science to the extent they otherwise might do. He also much objects to patients being allowed to sleep in separate rooms, being persuaded that efficiency of supervision and inspection is best secured by dormitories containing from eleven to twelve beds.

After describing Dr. Philp's private asylum at Kensington, he observes—

"I have examined Dr. Philp's house with much attention, and in regard to arrangement, cleanliness and comfort, it calls for, in my opinion, nothing but praise; but I must avow that my sympathies are enlisted with that system in which we live continuously with our patients. It is my conviction that the physician and his family should reside in the midst of the insane, so as to have them incessantly under the eye, and receive into his intimate society the convalescent, the monomaniacal, and especially the melancholic. This is my system, in which I am so ably seconded by the devotion of my wife, who keeps those poor suffering souls in her apartments for ten hours together, and it appears to me the means which, united with reading, music, diversions, exercise, and employment, furnishes the most incontestable results."

Hanwell.—This establishment M. Brierre visited with much curiosity and interest, as the great scene of the development of the non-restraint system. To this he has always shown himself an enlightened opponent; believing, in common with many other observers in this and other countries, that however infrequently it may be required, there are cases in which its mild application becomes the greater kindness to the unfortunate lunatic. There can be no doubt that the experience of Hanwell has been attempted to be prematurely generalized in respect to establishments receiving a different description of patients.

"The aspect of the inmates of Hanwell, the information I have obtained in England and that which was furnished me in the establishment itself, have left me in no kind of doubt as to the description of patients which this asylum is filled with. It is not to be dissimulated that Hanwell is a true hospital of incurables. Almost all the patients brought to it have already been treated elsewhere, and are consequently in a very favourable disposition for the non-restraint system. To convince ourselves of this, it suffices to observe what takes place in our own establishmeuts. At what epoch do paroxysms of fury, cries, agitation, suicidal ideas, the desire to injure others, refusal to eat, or attempts at escape, take place? At the commencement of the disease. It is then only that we are obliged to place the insane in the baths by force, to apply the strait-waistcoat, to fix them in their chairs, or to imprison them in their cells. But this condition lasts a very short time: and never does the excitement of acute mania resist seven or eight prolonged baths. Soon all this effervescence and fury subside; and patients, who seem determined to brave all measures, become tranquil, and follow the customs of the establishment. When new paroxysms occur, they

are, most commonly, but transitory: a bath, or a seclusion for a few hours, or the privation of some favourite dish, sufficing to restore order.

"The conditions here are quite different to those observed in insanity at its commencement. Moreover, we should take into consideration the characters of the English people. In England people are brought up with a respect to the laws, a deference for the upper classes, and a submission to custom. There they are slaves to the letter, and individuality, controlled by religion and patriotism, is not incessantly opposing itself to the slightest obstacle. Do not these national differences, joined to the chronic or incurable condition of the patient, suffice to a certain extent to explain the success of the non-restraint system at Hanwell?

* * * * * *

"It has been said, and not without foundation, that solitary confinement in a padded cell, and the subjection of a patient by a greater or less number of attendants, are truly coercive measures, the name of which has alone been changed. But even such means are not always applicable. 'For those who know the insane,' says M. Falret, 'non-restraint is a fiction, a simple substitution of one means for another;' and I may add my profound conviction, that solitary confinement is a mode of repression a thousand times more painful and more restrictive of liberty than the strait-waistcoat; and that it is contrary to the first principles which should prevail in the treatment of the violent insane, which consist in placing them in conditions the most favourable for taking that exercise in the open air, which Nature so imperiously demands for them."

M. Brierre de Boismont thus concludes his papers:—

"In summing up my impressions it will be seen—1. That the English establishments I have visited are remarkable for their size, cleanliness, and the care bestowed upon the patients; but that they differ from those of France in their construction, classification, and mode of treatment. In our comparative examination we should not lose sight of the serious, methodical, puritanical, matter of fact, but comfort-loving character of the English nation. 2. Some of these are defective in space, and consequently in the means for those agricultural labours, which form an indispensable portion of all effectual treatment. Their position in the midst of towns is also an obstacle to their amelioration. 3. The separation of the Criminal Insane is useful, and I sincerely desire to see it adopted in France with certain modifications. 4. The crowding together of patients, so as to assemble 1000 in one establishment, as at Hanwell, is contrary to the objects of Asylums. 5. The increase of incurable patients which threatens the invasion of the best institutions, should induce measures for the separation of such from those who have yet a chance of cure. 6. The non-restraint system has rendered good service by forcibly directing attention to barbarous systems of treatment in complete disaccordance with the manners of our times, but it is not always applicable."

I. On Pulmonary Consumption, and on Bronchial and Laryngeal Disease, with Remarks on Places of Residence chiefly resorted to by the Consumptive Invalid. By Sir *Charles Scudamore*, M.D., F.R.S., &c. &c. 8vo, pp. 259. London, Churchill, 1847.

II. Cold and Consumption; or, Consumption, its Prevention and Cure by Cold as a Constitutional, and Inhalation as a Local, Agent, &c. &c. By *Henry C. Deshon*, Member of the Royal Colleges of Physicians and Surgeons of London, &c. 8vo, pp. 153. London, Renshaw, 1847.

It is now ten or twelve years—it may be more—since Sir Charles Scudamore published a work on the treatment of Phthisis by the inhalation of Iodine. It was not well received by the profession at the time, in consequence of an air of mystery that was cast around the use of the remedy, in a manner which was rightly deemed not altogether consistent with the usage of an honourable profession. Sir Charles thereupon promptly made public his mode of practice; but we have never understood that it has been used with any satisfactory results by other medical men to warrant its general adoption, and it may therefore be fairly presumed that it, like too many vaunted remedies for pulmonary consumption, has been found inefficacious, if not positively hurtful. We have seen it tried under Sir Charles' own superintendence; but the results of the trial most assuredly did not encourage us to repeat it in our own practice.

It may be as well here to specify Sir Charles' formula, and the directions he gives for using it.

℞ Iodinii puri,
Potasii iodid. āā gr. vj.
Aquæ destillat. ℥ v. ℥ vj.
Alcoholis ℥ ij. M. fiat mistura, in inhalationem adhibenda.

"But invariably I direct the addition of a *saturated* tincture of the dried leaves of conium, which in the most favourable manner softens the action of the iodine solution, and tends to soothe the bronchial mucous membrane. Of the iodine solution, I commence with the dose of 30 minims, and increase it by 5 or 10 at a time, in a gradual manner, according to its effects and the nature of the case, till I may perhaps carry it to 240 minims; but, in the majority of instances, I confine my range to 180 minims. Whatever may be the quantity I use for each inhalation, I constantly direct that two-thirds of it be put at first, the other third when half of the time for inhaling has expired; otherwise it would be too strong at first and too weak at last; 30 minims of the tincture is the ordinary dose which I prescribe; and this need not be divided, nor does it in general require increase, as it is so much less volatile than the iodine, and enough of strength remains; but, if much of its soothing influence be wanted, either to allay irritable cough, or to act as a [soporific, a drachm, or even a drachm and a half, may be employed; but in such case, it is better to use it in divided portions. The water, to which these preparations are to be added, should be of a temperature of from 115° to 125° Farenheit; as a medium, 120°;

a little more or less is not material. The whole should be well blended by shaking the inhaler. This should be constructed of glass, for a metallic one instantly decomposes the iodine. Its tubes should be capacious, and the inhaler should never be quite half filled; for if these two last circumstances are not carefully attended to, much inconvenience would arise, the inhaling would be rendered difficult, and which by proper attention is so perfectly easy a process, that an invalid with the weakest respiratory powers does not experience any difficulty.

"The last part of these preparatory steps, for the purpose of keeping up the proper temperature of the contents of the inhaler, is to place it in an open vessel, large enough to allow of the inhaler being a little removed from its sides. Water, of a temperature from 120° to 130°, is to be put into it, enough to rise to about two-thirds of the inhaler; and, to prevent any inconvenience from the vapour which issues, the vessel should be covered over with a piece of thick paste-board, neatly fitted.

"Now desire the patient to inhale by making a rather deep inspiration, then to relax, or take off, the lips from the mouth-piece; and to inhale again immediately, carrying on the process effectively, so that the medicated vapour shall pass into the deep air-passages, but not in a quick and fatiguing manner. At the first time of using it, five or six minutes will be sufficient; but, in the progress of the treatment, it may be extended to twenty, twenty-five, or thirty; but I seldom, in my direction, exceed twenty. The frequency of repetition is from twice to thrice in the day; commonly thrice, for the first four or six weeks." P. 110.

Such is the remedy, from the use of which Sir Charles assures us that he has often derived the most surprising and truly gratifying results in the treatment of even aggravated cases of tubercular Phthisis. He does not indeed trust to it alone; but it is abundantly obvious that the other remedies employed are regarded by him but as adjuvants, and that it is to this one in particular that the credit of his success is believed to belong. As might be expected, he gives minute details of some of the wonderful cures that he has effected. The question is, can we place sufficient reliance upon Sir Charles' diagnostic skill to warrant us in recognising the curative efficacy of his practice? It is most unfortunate that he has not adduced any cases from the experience of other medical men, or, at all events, where their concurrent testimony might have been had, in confirmation of his own very favourable statements. In Case VIII., indeed, we learn that another medical gentleman was in attendance; but, alas! his testimony is one of the most damaging things in the world to the credit of our author's sagacity; for the ignorance displayed by this witness of the subject on which he gives his opinion is so transparent, that one might almost suspect that he is not an educated member of the profession. What will our readers say of the information contained in the following passage?

"The left lung gave strong puerperal resonance; the right lung was pectoriloquous, from the root to the mamma; there was much gurgling, and percussion was dull over the greater part of the right side; it was also much smaller than the left, and hollow at the clavicle. The body was much reduced; he had profuse hectic sweats, and the expectoration was copious, puriform, and very offensive; the pulse rapid. His debility was so great, that, to use his own words, he felt to be dying from day to day. The night perspirations were most profuse, and he was often sleepless. On the first inhalation, he expressed himself very sensibly relieved; afterwards, his breathing was never oppressed in going up stairs, as it was before using the inhalation; and, with a little more interval, he was able to walk two miles without fatigue." P. 140.

The report of the case, to which these remarks apply, may be fairly taken as a specimen of the evidence which Sir Charles adduces in favour of *his* method of treatment. It is headed—"*Tubercular Phisis; the existence of a larye cavity unquestionable. Symptoms of the case highly alarming; recovery, which lasted for a long period.*" The patient was believed to be in a most confirmed consumption; the right lung being supposed to contain a large tuberculous excavation, and the left one to be infiltrated with tubercles in a crude state. Great emaciation, afternoon hectic, and night sweats, had existed for a considerable time. Now for the treatment that was adopted, and its declared results :—

"The patient inhaled the mixture of iodine and conium regularly three times a day, at first for ten minutes, afterwards gradually increased to twenty; small blisters were applied to the chest from time to time; the lotion of tannin infusion, with acetic acid and eau de Cologne, was applied night and morning to the skin, followed by the use of the flesh-brush. Internally, pills composed of pilula. hydrarg. camphor. and c. colocynth. extract, were given at night, occasionally followed by a morning aperient draught; a strong infusion of the cortical part of sarsaparilla, with alkali and gentian, was used twice in the day; and, to procure comfortable sleep at night, he took a soothing morphine syrup, acidulated with diluted sulphuric acid. The plan of diet was changed to one highly nutritious; and such were the languor and debility, that wine, the best port and sherry, was allowed with unusual freedom. He usually took three or four glasses in the course of the day, in addition to a pint of sound draught porter, not only without disagreement, but with every sense of benefit. He had sometimes alarming attacks of exhaustion, at the commencement of my attendance. His diet had been too much restricted, and he had indeed said that he was 'dying from starvation.' After a few weeks, iron and quinine were administered in conjunction, instead of the other medicines." P. 140.

Within three weeks, "the specific symptoms were most materially relieved," and in three weeks more he was on the high road to recovery. Sir Charles saw his patient two or three months afterwards, and then he found "a remarkable diminution in the extent of the pectoriloquism, with an evident amelioration in the condition of each lung. The râles had ceased; and by auscultation there was satisfactory evidence of a very improved respiration. The expectoration continued, but was much lessened in quantity, and almost free from its former offensive odour. It appeared to me that the tuberculous cavity was in a favourable progress of healing; and certainly the whole aspect of the patient was promising a fair recovery; for to regain perfect health could not be expected, when so much disorganization of lungs had been produced, existing in conjunction with an unhealthy liver." It would appear that this patient recovered his health and strength so much that he was able to return to the West Indies, where he remained for nearly a twelvemonth, that he then returned to this country, "passed the winter in a cold wet part of the North of Scotland, and there took cold very seriously, and had an attack of acute symptoms which terminated fatally."

All that we can say respecting this case is, that we feel it our duty to caution our less experienced brethren against expecting anything at all like the success which Sir Charles considers to have been the result of his method of treatment; else they will assuredly be most grievously disappointed.

But it is not only in aggravated cases of tuberculous disease of the lungs that Sir Charles has succeeded. Even when there has been Empyema existing at the same time, he has met with the most gratifying results. Case XX. is one in point, provided indeed the reader is satisfied that there was any purulent effusion into the chest at all; for certainly the most characteristic symptoms of this very serious complication were not present, if we may judge from the report. The reader may determine for himself from the following narrative of the symptoms: there was an open abscess, we should remark, between the 5th and 6th ribs on the left side.

"The fifth, sixth and seventh ribs were elevated, giving to the side a very swollen appearance. The orifice of the abscess had its edges quite inverted. There was a slight purulent discharge; and it was remarkable that this alternated with the expectoration: when the one was free, the other was very slight. Of this fact I was several times an eye-witness. By auscultation and percussion, the following evidence was afforded: considerable resonance in the upper part of the right lung, and still more remarkable in the right axilla. Imperfect respiration in the upper part of the left lung; and below also it was imperfect and more indistinct.

"Sound duller than natural on the right side; dull at the inferior part of the left, especially when the patient was in the erect position; and becoming clearer when he lay on the opposite side. I drew the inference that there were tubercles in the upper part of each lung, but particularly the right; that there was effusion into the cavity of the left pleura; that nature had performed the operation for empyema in producing the external abscess; and that, internally, a communication had been formed, by ulceration, between the bronchi and pleura." P. 169.

In this case, too, the inhalation of *iodine* and *conium*, in conjunction with a generous diet and the use of sarsaparilla and other alteratives, effected the most satisfactory amendment: "indeed, it much exceeded all expectation." This patient too, like the preceding one, after having made such wonderful progress under Sir Charles' treatment, very imprudently exposed himself to wet and cold, which brought on a recurrence of all the bad symptoms and proved fatal. Unfortunately, there seems never to have been any examination after death of the state of the thoracic viscera; so that we are left in doubt as to the correctness of the diagnosis that had been formed.

What tends very greatly to diminish our confidence in the full accuracy of our author's statements in reference to the surprising cures which he has effected, is the circumstance of his, every now and then, seeking to disarm incredulity by protestations of his candour and love of truth. For example, in one passage he says,—"I can conscientiously declare that I have not been guilty of the least exaggeration in any part of my narrative of cases." Such language sounds to us very much like that of one who deprecates the charge of being a story-teller. Why should any man, assured of the perfect truthfulness of his statements, exhibit such a timorous cringing spirit? Truth is ever bold, nor seeks to win favour by assuming an air of obsequious humility. Besides, we like not such remarks as these:—"If I had made my own credit and reputation a selfish consideration in the class of cases to be treated by iodine inhalation, I should have rejected all such as were evidently in their nature so confirmed and desperate as to preclude all chance of recovery; but as inhalation has the property of mitigating symptoms, where it cannot accomplish more, I have always felt

it my duty to employ it in phthisis, if requested by the patient, even under most unfavourable circumstances of the disease ;" and again—" If I had been governed by a rigid solicitude for the credit of inhalation, I might have declined the application of the treatment to such a case as this, at the first view so evidently hopeless." Who ever suspects an honest physician of any other motive save the simple one of seeking to relieve the distress and sufferings of his patient ? Away then with all such silly, and worse than silly, talk about his credit and fame. Neither do we understand what our author means when he tells us that "no one is more fully aware than himself of the doubtful balance in which a physician places his feelings and his reputation, who engages in the treatment of consumption." The balance need never be doubtful either one way or the other, provided, all the while, he knows that naught save the dictates of holy truth and humanity has been the actuating principle of his conduct.

In another respect, the language employed by Sir Charles is not very likely to conciliate the approbation of the profession. The following passages will best serve to illustrate what we allude to.

" I adopt the opinion that tubercular phthisis is a specific blood disease, that there exists a tuberculous condition of the blood, and which, in the strongly marked examples of hereditary phthisis, is born with the individual as a germ." P. 55.

" The tubercular virus may exist in different degrees of intensity in different persons ; in one individual, leading to slight indications of consumption, what is called a tendency to it ; in a second subject, to the disease well-marked, in the chronic form ; in a third, the acute disease, running a most rapid course." P. 57.

" In conclusion, although the method of practice which I have advocated in these pages is the most useful and the most likely to succeed in tubercular phthisis of any with which I am acquainted, it falls short of that which, for the good of humanity, I desire. The *desideratum*, as I conceive, is a medicine which shall be found to exert a successful *specific agency* against the tuberculous poison ; to neutralise it, and effect a radical cure !" P. 216.

One can scarcely believe that any person, who talks of a *specific* for the tuberculous poison, can have very accurate notions of pulmonary consumption. It may be as well to mention here that Sir Charles seems to attribute the therapeutic agency of his remedy rather to its alterative effects upon the system in general, than upon its local operation upon the diseased tissue to which it is applied. The only evidence, however, adduced in favour of this idea is contained in the following paragraph :—

" That the introduction of iodine into the system through the medium of the lungs is effected, I have had the proof occasionally by witnessing some of those inconvenient effects on the constitution which its use as an internal medicine, or as rubbed in the form of ointment on the thyroid gland, now and then produce. But I am happy to add, that such instances are so very rare, as not to form any objection to its employment. I am convinced that it does not happen so much as once in fifty times. The disagreement in question is a peculiar nervousness, a tremour, and a timidity ; but no disorder of the stomach or bowels ; as is liable to happen from the ordinary taking of iodine." P. 211.

Before taking leave of Sir Charles, we wish to call the attention of the reader for a few minutes to some observations he has made, in the early part of his volume, on the question of the great increase of the animal heat in cases of consumption.

"It is a curious pathological fact, as I have found in a very extensive examination to be verified without a single exception, that in every case of tubercular phthisis the animal heat is more or less raised beyond the healthy standard. This may be stated as a mean at 96·5. It is always found highest in the morning at the time of rising from bed, with all persons. In the course of the day it is influenced by certain circumstances, and is raised by exercise; particularly in the fresh air of the country. In phthisis, I have found it range from 99° to 105°. I consider that the examination of the animal heat assists our diagnosis as to the existence of tubercles. In a doubtful case, I am pleased to find the animal heat not higher than 98°.

"It would be foreign to my present purpose to enter into the interesting, beautiful, yet difficult subject of animal heat; or attempt the consideration how far this function is to be referred to chemical action taking place in the lungs, how far to vital influence, and how far to the nervous system; but I believe it is on all hands agreed that the most immediate and influential cause of the production of animal heat is the combustion of carbon, brought in the venous blood to the ramifying capillaries, in order to receive aeration and the all-important influence of oxygen introduced by the air-passages.

"In this chemical view of the subject, and to which I now confine myself, it appears, I think, surprising, when we consider how much of tuberculated lungs is excluded from the aerating process, either from the compression of the air-cells by the tubercles, or their actual occupation by these foreign bodies, that the animal heat, instead of being lower, as we might imagine, as a consequence of the surface for the aeration of the blood being greatly lessened, is actually found to be higher than in the natural healthy state of the lungs." P. 30.

After relating some experiments which go to show that the quantity of carbonic acid evolved during respiration is greater in a phthisical patient than in a healthy person, our author remarks:—

"It appears to me that, in the relation of these experiments, I have offered strong evidence that in tubercular phthisis, notwithstanding the organic limitation of the function of the lungs, from the obstruction of the air-cells by tubercles, the important process of the decarbonisation of the blood, the combustion of carbon, and increased animal heat beyond the healthy standard, go on with more rapidity than in sound lungs. An increase of the animal heat does not ensue from merely quickened respiration, as is shown in the examples pf running up to the top of a lofty building; nar even more than a degree in the paroxysm of spasmodic asthma. I consider, indeed, that in some cases of this kind, where almost asphyxia takes place, the reverse would happen, and the animal heat be found below the natural standard.

"There is in tuberculated lungs an increased activity of function; and hence the hectic fever, so urgent in acute phthisis, and the hectic irritation, although often hardly amounting to evident fever, in the chronic form of the disease. The actions of the animal economy, when unnaturally hurried, are not so healthily performed. We may suppose that the oxidation of the blood being effected in so rapid a manner, cannot be so favourably accomplished; and also a morbid excitement prevails in the whole system. The nervous system is morbidly sensitive. Although the appetite may be good, and abundance of food be taken, yet nutrition is imperfect, and the body wastes—a consequence much to be referred to the imperfect and unhealthy performance of assimilation and sanguification; at the same time that the absorbents generally are probably thrown into a state of morbid activity." P. 33.

The general result of Sir Charles' observations and experiments is, that, "when the lungs, or mucous membrane of the air-passages, are in a state of irritation from disease, the animal heat is always more or less raised

beyond the natural standard." There can be no doubt but that a persistent increase of the animal heat, and (as always happens at the same time) of the pulse, is an almost infallible indication of an inward irritation: and that inward irritation is, in the majority of instances, dependent upon some pulmonic lesion.

The work of Dr. Deshon—who, we observe, lives at Budleigh Salterton in Devonshire—is altogether a most discreditable production, and one utterly unworthy of any educated and honourable member of our profession. We strongly suspected its character from its very title, and the suspicion was confirmed by the circumstance of our learning that no copy had been sent to the medical journals for review. But this attempt at latency will not preserve the author from exposure.

The first two chapters profess to give an anatomical and physiological description of the thorax and its viscera. We must find room for one or two *morceaux* of the silly stuff of which they consist. For example, we are told that—

"The lungs and heart, conjointly, may not inaptly be regarded as a distillatory apparatus; whereof the lungs resemble the furnace, to which the blood, exposed in a convenient retort, the branches of the pulmonary artery, and purified of its grosser parts by due combustion, is returned by the pulmonary veins or worm, to the heart or receiving vessel, to be from thence conveyed by the principal artery of the body, or main pipe, and by its innumerable branches for universal distribution." P. 5.

"As a general rule, it may be said that in aquatic animals, the lungs are prolonged from within outwardly to meet the air with which the water is saturated; but in terrestrial beings, on the contrary, the lungs are contained in a cavity into which the air enters to meet the blood. The utility of this arrangement is obvious, namely, to present to the gill of the former, by constant change and motion, the largest possible amount of air; which, on the other hand, being exhibited to the latter in a more pure and undiluted state, every end is accomplished by a smaller quantity entering the pulmonic sac or receiver. Again, it is absolutely necessary to respiration that it should be performed by means of a watery vehicle; the fish when brought on land becomes asphyxiated, not for want of air, but for want of water. Since the air is presented to it in a state, under which it cannot elaborate, and the evaporative effects of which it is unable to withstand. Its gill therefore quickly desiccates, and the animal perishes. The gill being in fact an unprotected lung, but the lung a protected gill; protected against, because exposed to evaporation by a contrivance, which at the same time, by its mucous surfaces and secreting glands, ministers to its aqueous condition, for the terrestrial, like the aquatic, animal must be a sphyxiated under a condition which deprives it of these means of extracting moisture.

"But how avails this digression, which I can fancy some pronouncing most irrelevant to the purpose. Man, say they, is not a fish, and what can the physiology of its respiration have to do with the subject under consideration? *Perhaps more than at first imagined, if, either for prevention or cure, the phthisical must advance a step towards the aqueous condition under which the fish respires.* For hereby is understood a fact which has been brought to light by the experience of later years, that moisture is not prejudicial to the consumptive, as was at first supposed. Without going to the extent of saying, with some physicians of the present day, that ague, the abundant offspring of moisture, is antagonistic to phthisis, it may readily be conceived that a damp atmosphere, by presenting aspirable air to the lungs under a form more analogous to the fluid about to receive it, facilitates the changes which occur during respiration, changes—upon the due execution of which—depend the health of the individual.

"There is a very intimate connection between the vital energy of the creature and the activity of this functiom. Birds, some of which are destined for rapid and extraordinary exertions, possess, in comparison with other animals, large vesicular lungs to which the bones, being hollow, and containing air, are likewise auxiliary. How bold and energetic the movements of the hawk! how slow and indolent those of the turtle! On a smaller scale is witnessed in man a remarkable concord between the vigorous performance of this function and the robust health of the individual." P. 18.

Equally rational is the description given of the digestive function. Will our readers believe that any man, who has honestly received a professional diploma, could pen such nonsense as this?

"If indeed digestion, more correctly termed assimilation, be allied to fermentation, which some believe to be the case, the extrication of carbonic acid and other gases is an accompaniment of that process, which would then seem to be absorbed by the walls of that viscus. Carbon is necessary, but in different quantities, for the elaboration of the tissues of both plants and animals." P. 22.

The "viscus" here alluded to we suppose to be the stomach.

With such specimens of our author's anatomical and physiological knowledge, no one can be surprised at the stupid blunders which he perpetrates, when he proceeds to treat of matters more immediately appertaining to practice. When describing the signs afforded by auscultation, we are told that Avenbrugger pointed out the applicability of percussion to the elucidation of chest diseases, towards the latter part of the *seventeenth* century! But this is a very minor blemish. In the very next page, we learn from the author himself the true why and wherefore of his appearing in print:—

"It is not the object of this brief treatise to enter at any length into an exposition of the signs afforded by percussion and auscultation, because such details are only befitting a work intended solely for the professional eye, and also because the self-application to the detection of disease is impossible; and even were it possible, would be absurd and dangerous. Be it my endeavour to attempt a reasonable explanation of their phenomena, and so to recommend their general adoption, as a great step towards improvement in the treatment of this disease." P. 49.

Yet this is a work dedicated by permission to George J. Guthrie, Esq., F.R.S., F.L.S., late President of the Royal College of Surgeons of England, &c. &c. Doubtless Mr. Guthrie was utterly unaware of the character of the publication to which he has lent the sanction of his respectable name; and our only reason for alludiug to this subject is to show the necessity of men of repute in our profession not accepting every trumpery dedication that is offered to, or thrust upon, them. If sufficient evidence has not already been adduced to show the nature of Dr. Deshon's production, what thinks the reader of the following?

"Need I tell you what happens every day? Hundreds of young females evidencing all the several symptoms of incipient phthisis, but which may merely denote mal-nutrition, and a proclivity to it, (for I defy any man to decide between them without the assistance of auscultation and percussion) are actually hurried into its confirmed phases by injudicious treatment—by purging, sweating, and cooling medicines; with cupping, leeches, and blistering; 'for look!' says the symptomatic physician, 'there is cough, difficult breathing, local pain, and bloody

expectoration; actually hemorrhage! We must subdue the inflammation.'" P. 59.

One of these "young females" is thus most interestingly described:—

"But the state of the chrysalis being at an end, a new field is open for Mademoiselle to range in. She makes her début in society; she may be wondrously delicate and beautiful, and like Sylvia, all the world may admire her; she may be

'Like all that poets fancy, like all that lovers dream,'

but still possessed of an acquired or hereditary susceptibility of constitution, which soon becomes augmented by various descriptions of excitement. The ball, the theatre, late dinners, midnight suppers, form the category of her sphere of action. Her exhausted frame, after exposure to the chilling damp of a nocturnal air, seeks on a soft, luxurious bed, repose; but a feverish sleep, amidst the freshest hours of morning, is her fate." P. 75.

Among other remedies, Dr. Deshon mentions inhalation as a valuable agent in the treatment of phthisis. "Among its advocates," says he, "in more modern times, appear the names of Laennec, Drs. Copland, Elliotson, Corrigan, Wilson, *Ramadge*, Hastings, and *Maddock*." The name of Scudamore is not so much as hinted at! By-the-bye, we doubt much whether our friend Dr. Hastings ever occupied so strange a position as that here accorded to him, between two such honourable worthies. Dr. Ramadge, we may mention, is repeatedly quoted by our author as a most esteemed authority; and, on one occasion, he appeals to the testimony of another notorious writer, who has declared that "with quinine and other *chronothermal* remedies, he has cured or arrested at least 500 cases of consumption, many of them, too, apparently in very advanced stages." The *modus operandi* of inhalation—nothing but the vapour of tepid water, to which a piece of camphor may be added, is used—is thus described:—

"The benefits resulting from the inhalation of watery vapour, at the highest temperature that can comfortably be borne, which would range from 160° to 180° Fahrenheit, is two-fold. By entering and expanding the circumjacent pulmonary vesicles, a most salutary pressure is exercised upon the walls of the excavation, and the more or less partial or complete approximation of its sides is effected; while, at the same time, the faculty of aspiration and expiration of the sound air-cells being augmented, a state of the lungs is induced unfavourable to the further development of the disease.

"But this is not all, for the direct application of heat and moisture to the ulcerated cavity has a most soothing and beneficial influence upon the nerves and vessels of the part; alleviating, in a remarkable manner, morbid irritation, and increasing the circulation to the highest pitch consonant with health, favouring to the utmost, reparation of the pulmonary lesion." P. 140.

As a matter of course, Dr. Deshon can quote cases in illustration of the success of his practice. Sir Charles Scudamore will perceive from the following narrative, that tubercular excavations can be cured without his favourite Iodine and Conium drops:—

"Within the last twelve months a person sought my advice upon whom the good effects of inhalation, in healing a cavern, were so evident as to justify my introducing

the case here. This patient manifested great debility and emaciation, and had night perspirations and profuse expectoration: he also suffered much from pains in the chest and larynx, which I expected to be ulcerated, to which nothing was so soothing as inhalation at a high temperature. Auscultation elicited pectoriloquy, and at once determined the nature of the case, which, indeed, was so marked, that a friend, who visited the patient for me, thought every hope of recovery forlorn, and advised a removal to his relatives and native air; yet this individual recovered, and now follows his usual avocations. It is not to be denied that inhalation, aided by appropriate medicines, saved this man's life, and that his continued health devolves upon his living judiciously, and using cold affusion and friction, to dispel occasional congestive pains which threaten, and would otherwise distress him." P. 141.

A most satisfactory and convincing case indeed! And now we have done with our painful and humiliating task. Would that it might with fairness have been avoided! But such cannot be, if the dignity and honour of the medical profession are to be upheld; for, assuredly, in no way is its character more signally tarnished than when any of its members are allowed to escape public censure and rebuke, when they bring discredit on its rightful claims. And here we are constrained to confess that there has been much in the medical literature of recent years, that is utterly inconsistent with the requirements not only of good taste and honourable feeling, but even of common truth and honesty. Scarcely a day passes over our heads but we meet with some flagrant example of the ill-directed love of public notoriety over-mastering the plain dictates of these too-much neglected guides. Numerous are the instances that might be quoted in proof of this charge. When we find a Fellow of the Royal Society lending himself to this sorry game, and condescending, for example, to borrow from the pages of the *Court Journal*, a recommendatory notice of his work on a subject connected with the toilette graces of the person, what may we not expect from others who are more in the shade?

A System of Surgery. By *J. M. Chelius.* Translated from the German, with additional Notes and Observations. By *John F. South.* Two vols., 8vo., pp. 1823. Analyt. Index, CLXX. Renshaw, 1847.

Although we have noticed this work more than once during its progress of publication in parts (which have been issued with exemplary regularity), we do not deem it right to withhold our expression of congratulation from the translator upon the satisfactory conclusion of his really laborious task. It will supply a want hitherto felt of a due exposition of the present condition of German Surgery in this country; for, as Mr. South observes, no standard work relating to this has appeared in English since the publication of Heister's book a century since. In his preface, and incidentally during the work, he speaks somewhat too disparagingly of French Surgery, "with its showy, but somewhat too hazardous operations;" but we are of opinion that not only are English students and practitioners more familiar with this last than with German surgery in consequence of the greater

diffusion of a knowledge of the French language, and the greater intercourse between Paris and London, but also because of its intrinsic superiority to that of any continental nation whatever. And in proof of this, we need not go farther than the book now before us; for while a translation of a work from the pen of a French surgeon and teacher occupying a position similar to that of Chelius might easily and advantageously be presented to the British public in *puris naturalibus*, we do not hesitate to say in regard to the present one, that had it not been for the valuable and numberless additions of the translator, in the shape of amplifications, illustrations, protests, corrections, and emendations, it would have infallibly fallen still-born from the press. Shorn of these, it is incomparably inferior, and in some respects half-a-century behind, our own manuals. Mr. South has, however, so edged in his author's text by notes upon notes derived from his own experience or reading, that, however uncomplimentary such a procedure may be to his author, and however undesirable such a precedent is for general imitation, he has succeeded in converting the original meagre "Handbuch der Chirurgie" into a tolerably complete System of Surgery, which both students and practitioners will find very useful as a work of reference, and the employment of which is facilitated by the addition of an admirable index, of dimensions seldom seen now-a-days, amply compensating for the defective arrangement of the matter of the work.

We may now present our readers with a few specimens of the contents.

Of the Elementary Proceedings of Surgical Operations—Considering the economy of space aimed at in the "Handbuch," (a principle utterly repudiated by the translator however,) one cannot help being somewhat amused at some of the unnecessary and Germanic details entered into, concisely enough it is true, in this section.

"There is scarcely a surgical operation which can be fully perfected on a diseased body by one single, simple act. All rather consist of several manœuvres following, according to determined rules, and distinguished by the name of *Steps* of the *Operation*. One of these is the special object of the operation, and the others must necessarily precede or follow, to effect this object, and bring about the restoration of the patient. The object of the operation is always the same, but the manner and way of attaining it may be very different, and this difference may consist either in the difference of the several steps of the operation, or of the entire way by which the attainment of the object of the proposed operation may be effected. Hence arises the distinction between *Operative Proceedings* and *Methods of Operating*.

"The *Method of Operating* is the compass of the regulated modes of proceeding, by which the object of an operation in any peculiar way is attained. In the various methods of operating, therefore, not merely are different parts cut through and in very different directions, but the practice of the methods of operating is so peculiar, that the one method does not exclude the other. Upon the choice of the method of operating depends, for the most part, the successful or unsuccessful result of the operation, just as upon the choice of the operative proceedings, rests the facility of its execution. The choice of the mode of proceeding is therefore of little consequence, and depends commonly on the operator himself. Hence, also, the variety of opinions as to the mode of proceeding in general, is greater than upon that of the method.

"In deciding upon the preference of the various methods of operation, the

following circumstances must be attended to. *First.* The least important organs must be injured, consequently the loss or destruction of organic parts caused by the operation, the pain and the traumatic reaction depending thereon is least. *Second.* The better method must always be most fitting for the greater number of cases. *Third.* This must consist in the manœuvres, which do not make the operator dependent upon accidental circumstances, but which rests completely on the will of the operator. Hereon and upon the nature of the parts to be wounded, are founded safety and facility in the execution of the method of operating. *Fourth.* The quickest cure which can be effected by the operation." Vol. II, p. 852.

The following circumstances require attention for the securing a probability of a successful result. 1. The disease to be removed should not be so connected with any general ailment, that this may act on it as a cause to keep it up continually. This would only be to remove the product of general disease, leaving the producing cause. The best results however follow when the general disease terminates in a local disease capable of removal. 2. The patient's weakness or sensibility should not be so excessive as to endanger life by the operation. 3. The local disease must not, by its long-continuance or other relations to the constitution, have acquired the rank of a secreting organ, or have removed any previously existing disease, or have checked it in its earlier development. Cases may occur, however, where, in spite of a cure not being anticipated from an operation, this may be employed as a means of mitigating suffering or prolonging existence.

In respect to the patient's condition, in those who can bear pain quietly and patiently, are operations least dangerous. Those, too, who have been long accustomed to, and enfeebled by painful disease, through the moral influence of the desire for the operation, and the less amount of traumatic reaction, bear them best. Persons of plethoric temperament, full of apparent health, or such as are very stout, of tall and strong make, bear them ill. Nervous persons may be placed in two categories—those "who are very sensitive and excitable, and those who, on the slightest cause, drop into moral despondency and nervous stupidity"—the former, although much affected by the pain, soon rally under the influence of hope and encouragement, which is not the case with the latter. The young bear operations better than the old, although in advanced age operations are not contra-indicated, and even often succeed better, in consequence of the less amount of traumatic reaction. In gouty subjects they are dangerous, and in the scrofulous the removal of a diseased part may be followed by the active development of scrofula in some internal organ. The nature of the preparation the patient is submitted to will depend upon the prevalence of any of the above circumstances—according to which we may endeavour to counteract general disease, raise his powers, depress augmented sensibility, &c. Robust and full-blooded persons should be kept some time on spare diet, and bleeding resorted to if this seems indicated. Great sensibility may be somewhat suppressed by opiates given before, and soon after, the operation.

As a general rule, the translator is, however, and we think properly, opposed to the use of *opium* or other sedative, owing to the difficulty of distinguishing between the effect of the medicine and the symptoms arising from constitutional excitement; and he believes that, even in the case of

persons habituated to the use of opium subjected to operations, the drug may be diminished or withheld sooner than is usually believed. The mere absence of the torturing pain they have been so long accustomed to seek its aid against sufficing to allow of sleep. If, however, the patient do not sleep, as the obtaining this is paramount, he does not withhold an opiate, whether the individual has been in the habit of taking it or not—while free livers may even require its repetition two or three times a day during the whole course of their case, keeping the bowels open at the same time, with an occasional dose of three or four grains of calomel, which acts upon, without irritating the canal. Mr. South adds :—

"Another very important point in the treatment of operations is the use of *porter*, *wine*, or spirituous *liquors*. Even where the patient has been prudent and temperate, it is occasionally necessary that one or other of these should be given soon after an operation. But for persons who have been accustomed to take large quantities of porter or spirits, or both, and who, in consequence of severe accidents, are subjected to the amputation of a limb, or who have severe lacerations, which, however, do not require operation, it is absolutely necessary for their safety that the stimulant should not only be not entirely withdrawn, but even somewhat very near the quantity they have been accustomed to, must be allowed, or they either sink at once, are attacked with erysipelas, or are violently affected with *delirium tremens*, in which condition they speedily die. The quantity taken may often seem enormous under the circumstances; three or four glasses of gin or brandy, and as much or more wine, and sometimes porter besides, in the course of the 24 hours, is by no means an unfrequent allowance; and I have just the recollection of one of the younger Cline's patients, a porter in the Royal Exchange, who required a pint of brandy daily after having suffered amputation of his leg after an accident. This man was saved by this treatment, and lived many years after, doubtless following the same free course of living which had required treatment, at that period thought exceedingly bold and almost marvellous in its result, although at present every day's practice, and no wonder at all." Vol. II., p. 855.

The restriction of operations to particular times of the year, Chelius observes, is no longer considered necessary, although very sultry weather is unfavourable to their issue. Where they can be deferred, a bright light is necessary for their execution, or the patient is liable to rheumatic and gouty affections, they should be delayed until steady weather may be calculated upon.

The elementary acts of every operation consist in Division, Apposition, or Dilatation. Of these, *Division* is the most general and important, and may be effected, 1, by a cut or incision; 2, by a stab or penetration; 3, by tearing asunder; and 4, by ligature. All instruments employed for *division of soft parts* by *cutting*, must be placed in two classes; those having but a single cutting edge, knives, bistouries, and scalpels, and those "consisting of two cutting edges, connected crosswise in their middle, and terminating in handles," the same forming our old familiar friend a pair of scissors. Then the different kind of knives (and scissors) are described according to the mode in which they are set in the handle or the form of the blade; a mere waste of words which a moment's glance at an instrument-maker's case would render needless. The *straight-eyed bistoury* is stated as the only one with which a regular cut can be made, and this is defined as follows :—A regular cut must have the same depth from

its beginning to its end: it must not have any bridges, the angles must not be cut more shallow than the middle, and the edges must not be jagged."

"The mode of holding the knife has an important influence on its use. In this respect, four postures or positions may be distinguished. 1. It is held like a pen, the handle being taken hold of with the thumb and middle-finger near the blade, and the fore-finger laid on its back. Herewith the knife can be used with ease, and employed in every direction. It is especially suitable where small cuts are to be made with great care. 2. The knife is held with the thumb on one, and the middle and ring-finger on the other side of its handle, and the fore-finger laid on the back of its blade, as in holding a violin-bow. 3. The handle is placed on the inside of the ball of the thumb, with the thumb on one side, and the middle, ring, and little finger on the other, whilst the fore-finger is extended upon the back of the blade. 4. The knife is grasped with the whole hand, the thumb on one, and the fingers on the other side of the handle. This is only applicable to large or amputating knives.

"Scissors effect the division of parts, like the bistoury, by drawing and pressure; but the pressure is greater, and therefore the scissor edge is generally not so fine as that of the knife; neither are the edges set directly opposite, but lie beside each other, so that ordinarily a cut with scissors is not so clean as that with a bistoury; the part too must also be pressed and squeezed before they are divided. On this account, the use of scissors is by many entirely rejected. The objections, however, to their use may be done away with by the proper fineness of their edge, and the great power with which they can be employed. It has been hitherto supposed that the due degree of fineness, like that of a bistoury, cannot be given to scissors without impairing their strength. I, however, possess scissors, made by our clever instrument maker Görck, which have the perfect edge of a bistoury, and with proper strength." P. 859.

Mr. South has most injudiciously committed himself in a postscript to an unfavourable and somewhat sneering opinion upon the *employment of Ether as a means of preventing pain during operations*. Writing in Feb. 1847, he was not called upon in a systematic work to even notice a remedy only introduced to the profession a few weeks; but, having determined to do so, it is matter of congratulation that his opinion has not been fortified by subsequent experience, and that it will only disfigure his book as a specimen of over-hasty conclusion. Speaking of the propriety of preliminary trials, he says—"With this opinion I fully concur, and I should certainly adopt it, if I made up my mind to try inhalation at all; but upon that point I am not decided, for I have considerable doubt of the propriety of putting a patient into so unnatural a condition as results from inhaling ether, which seems scarcely different from severe intoxication (!)—a state in which no surgeon would be desirous of having a patient who was about to be submitted to a serious operation." Of one thing we are certain, that if some surgeons have not made up their minds upon this matter, most patients have, and that the services of those who, upon mere speculative grounds, decline availing themselves of this means of mitigating dreadful suffering, will very properly be but rarely put into requisition. This is a matter the public must and will decide for itself.

General Treatment of Lithotomy Patients.—Mr. South observes that lithotomy cases may prove fatal, however dexterously an operation may have been performed, and he is disposed to refer this to some error in the

prior or after-treatment. Upon this subject he offers some very judicious directions, going into minutiæ not to be found in our works on surgery, and for the want of being acquainted with which practitioners may lose patients upon whom the operative procedures have been conducted with skill. He deprecates the placing of patients with stone in the ordinary wards, where they are not quiet enough, and the sisters of which are not habituated to watch various circumstances it is important for the surgeon to be made acquainted with. The following is a brief abstract of the account he furnishes of the practice which has been long followed at St. Thomas's Hospital. The patient is placed in a small ward containing only half a dozen beds, and which, after the operation, is kept very private and quiet; and here he remains under the watchful eye of an experienced sister for ten days or a fortnight, to get accustomed to place and attendants. If in tolerable health his diet and habits only demand ordinary attention, occasionally resorting to a hip-bath when his suffering is great, especially after sounding. Rarely is bloodletting or other depleting means practised. The bowels are however to be kept clear; and if the patient have been accustomed to take gin and water to increase the action of the kidneys—a practice very prevalent among such persons—this is to be only discontinued gradually, or even not at all. Temperate weather should if possible be selected, for great heat exercises a very depressing effect on these patients; while, if it be very cold, he may easily become chilled while exposed at intervals during the after-treatment. The day prior to the operation a dose of castor-oil is given, and the diet restricted to rice-pudding and milk, with plenty of barley-water or gruel, especially the former. If the motions are hard and lumpy, an injection of castor-oil and gruel is to be thrown up on the morning, but if not, the gruel alone suffices.

After the operation the patient is put to bed with his legs straight and close together, for the purpose of gently maintaining the edges of the wound in contact, and checking slight hæmorrhage. A napkin is passed around the pelvis just as it is put on an infant, and throughout the treatment this is carefully changed when wet by the urine passing from the wound. In the evening, or next morning, if there be no bleeding, a piece of lint folded on the end of the finger is introduced into the wound, and pressed up the depth of the perineum. It is changed each time the patient wets, and is continued until the healing is accomplished, with the intention of securing this taking place from the bottom, under which plan the formation of fistulous passages is a matter of very rare occurrence. "A handful or two of chamomile flowers thrown into a basin, are sprinkled with spirits of wine, well mixed, so as to be equally moistened, and then put into a thin flannel bag, and having been well heated on a warming-pan, are applied over the belly as hot as the patient can bear, on the evening of the operation-day, if there be no bleeding; and this is continued for a week or ten days." If the wound on the second day is swollen and no urine flows, no lint is introduced, but a bread-and-water poultice is applied until the swelling subsides and the water increases. Cold is not applied, and an opiate is rarely given. The diet for the first two or three days consists of farinaceous food and free libations of barley-water; and a little castor-oil is given on the third day. If there is pain in the belly or sickness, however, these may usually be relieved by giving the oil a little

earlier. About the third or fourth day urine usually passes by the urethra as well as through the wound, and in a week or ten days it flows through the natural passage alone, and then the wound is dressed with wax and oil spread on the lint, introduced as before. If the urine does not flow through the wound at first, and the patient becomes irritable and uneasy, the finger should be passed in to remove any obstruction which may be offered by a coagulum. If the patient flag, the resumption of his gin and water may be advisable. He should be kept in bed for some days after the urine has ceased flowing from the wound; and if the formation of a small sinus retard cicatrization, a piece of lint of corresponding size should be twisted up, dipped in a solution of sulph. cupr., and screwed up to its bottom; but generally the wax and oil-dressing suffices. In from three to four weeks the cure is completed, the diet having been in the mean time gradually improved, and porter or wine added if deemed necessary.

Comparison of Lithotomy and Lithotrity.—This question has lost none of its interest and importance, and from recent discussions at the French Academy, seems as little settled among our brethren across the Channel as heretofore. Lithotrity is there as warmly espoused by the specialists as the preferable operation, as its eligibility is stoutly denied by some of the best surgeons in that country; and the discovery of Etherization, which seems likely to be a valuable adjuvant in the one operation, and a most questionable means in the other, where the retention of sensibility often affords most useful guidance for the operator, will doubtless render the substitution of lithotrity less common. In our own country, some of our best surgeons, as Brodie, Liston, Key, &c., &c., have shown that, in many cases, it is the preferable procedure; and we now have to give an account of the opinion entertained by Chelius. He observes that many of the objections which were tenable at first have lost their validity by reason of the subsequent perfection of instruments and procedures; but that we have not even yet data upon which any positive opinion can be delivered. The statistics are eminently defective to this end, constructed, as they are, on a faulty basis, while their accuracy and good faith have been warmly and effectually contested at Paris. "If the possible evils which may occur on and after lithotomy and lithotrity be compared, they are found to have a certain degree of equality as to their number and danger; only that in lithotomy the wound especially gives rise to symptoms which in lithotrity are absent, whilst the latter occasions considerable irritation of the bladder, and dangerous symptoms resulting therefrom." After enumerating the various untoward circumstances which may attend either operation, Chelius goes on to say:—

"If these various circumstances in lithotomy and lithotrity be compared in regard to their cause, to wit, the wound in lithotomy, and the injury of the bladder in lithotrity, it must be presumed that pain and nervous symptoms may be equally present in both, but their frequent repetition in lithotrity is of importance; that bleeding, wound of the rectum, injury to the peritoneum, which are very much to be dreaded in lithotomy, cannot happen in the modern practice of lithotrity; that infiltration of urine, so frequently fatal after lithotomy, is almost impossible in lithotrity; that phlebitis and peritonitis are observed not unfrequently after lithotomy, but very rarely after lithotrity, which also applies in like manner to the continuance of fistula; that, on the other hand, inflammation of the bladder, inflammation and abscess of the prostate, are more common after

lithotrity. Bruising or tearing of the mucous membrane of the bladder, as also breaking of the instruments in the bladder, is at the present time scarcely possible with the improved instruments.

"Further, if lithotrity be considered in reference to the condition of the urinary organs, the age, sex, and constitution of the patient, and the nature of the stone, it follows that a diseased change and swelling of the prostate, purulent catarrh, great sensibility and contraction of the bladder, render lithotrity quite impossible, or considerably increase its danger. Although lithotrity was formerly considered inapplicable to children, and numerous experiments by Civiale, Amussat, Leroy, and others have proved its practicability in little children; yet, however, the result of lithotomy at this age is so favourable, and the employment of lithotrity so difficult, that lithotomy should undoubtedly be preferred. In advanced age, on the contrary, the results of lithotomy are far more unfavourable than those of lithotrity. (How does the author reconcile this with his statement, that an enlarged prostate contra-indicates lithotrity?) In females, the less difficulty in the introduction of instruments is compensated by the difficulty of keeping the bladder distended; but lithotrity, although lithotomy in woman is much more rarely fatal than in man, has this great advantage, that no incontinence of urine remains after it, an infirmity the importance of which in woman cannot be too seriously thought of. It must finally be remembered that, for very stout persons, who are always the most unfavourable subjects for lithotomy, lithotrity is far less dangerous.

"If now, after the consideration, founded on experience, of the advantages and disadvantages of lithotomy and lithotrity, the particular cases in which the one or the other practice is specially indicated, be reviewed, it follows that lithotrity appears preferable—1, in small stones, or those of no great size; 2, where there are two or several little stones; 3, in stones of moderate size, and when they can be easily broken, and if in all these cases the bladder be healthy, or only in a trivial degree affected. These indications are important, when such cases occur in old persons, in females, or in very stout people. On the other hand, lithotomy is decidedly to be preferred—1, in childhood; 2, with large and hard, and especially mulberry stones; 3, when there are several large stones; 4, when large stones entirely fill, or are completely locked in by a contracted and unextensible bladder; 5, in diseased prostate, or great affection of the bladder; 6, in very great sensibility of the bladder, so that the patient can bear neither its distension, nor the motion of the instruments; 7, with stones of which the nucleus, as for example, when it is a bullet or the like, cannot be destroyed by the lithotriptor. It is also not to be overlooked that, in the general employment of lithotrity, the patient should be subjected to it early, by which its results are more certain, and its use will become more easy and general. On the other hand, however, it must not be unnoticed, that under directly the same circumstances which are favourable for crushing, does cutting for the stone, if performed with ability, lose much of its danger." Vol. II., p. 623—5.

After detailing the opinions entertained by some of our best surgeons favourable to lithotrity, Mr. South goes on to say:—

"I do not propose to offer any opinion of my own as to the preference which should be awarded to lithotrity or lithotomy, as I have had little practical experience in regard to the former, and am therefore not qualified to give one. But I may be permitted to say that the results of the practice of lithotomy, both with gorget and knife, and by various operators on patients of all ages and under various circumstances, during the course of a long series of years at our hospital, have been so favourable, as to afford little cause for making it give place to lithotrity. I think it is proved that lithotomy, when properly conducted, is not the dangerous operation it is too commonly held to be; and it is no trifling advantage it possesses, that the patient is relieved at once, with a few minutes' suf-

fering, sharp indeed it must be acknowledged to be, instead of being subjected to several operations, which, the more frequent in their repetition, become, as generally admitted, greater in severity, and occasionally leave the necessity for resorting to the cure by lithotomy. I may here also add the testimony of some patients who have undergone both operations, that the suffering during lithotomy was less than in lithotrity, and that knowing both, they would, if needful, prefer undergoing the former. It is well, however, that we have the opportunity of employing lithotrity in cases where patients are too fearful to submit to the knife; but I am by no means sure that, under all circumstances, lithotomy is not at least as free from danger as lithotrity, and certainly more speedy as regards cure." P. 629.

A tolerably strong opinion this for one who professes his inability to offer any; and one which we will venture to say will not receive general assent. Although the operations at St. Thomas's may have proved as successful as stated, and we have heard of individual operators, both in this and other countries, boasting of their never, or hardly ever, losing a case; yet the untoward events which attend lithotomy are of too frequent occurrence to be disposed of in this way. So too the few minutes' sharp suffering, here so quietly spoken of, is too often prolonged, and that sometimes under the best hands, to a far more protracted period of intense agony. Mr. South limits his preference to the operation to examples of it "properly conducted;" but it is but just to mete out a similar restriction to the rival procedure, which most certainly could never have embraced the cases he alludes to. Nor do we see how this is ever to be the case if suitable instruction be not given to the students of our hospitals in the use of lithotriptic instruments—the more dashing results of lithotomy even yet almost exclusively occupying their attention. It seems to us certain enough that if we were restricted to the one operation, lithotomy would necessarily be the one preferred, as embracing many cases lithotrity is not competent to cope with: but, enabled as we now are to adapt each procedure to appropriate cases, it is surely but reasonable to select the least painful and least formidable one for such as satisfactorily admit of its being put in force. The grounds of the selection may yet occasion some difference of opinion; and we certainly cannot agree with Chelius, who prefers lithotrity in the aged, or with other surgeons who employ it in the cases of children. Perhaps the greatest benefit which lithotrity has conferred is the inducement it offers for patients to submit and surgeons to undertake the curative treatment of stone-cases at a far earlier stage of the case than was formerly the practice—thereby preventing much of that secondary disease of the urinary organs which rendered the results of operations more doubtful and the recurrence of the disease more probable.

Mr. South furnishes us with a table of the 295 operations performed at St. Thomas's Hospital from 1800 to 1846; and a second table containing the dates and results of most of those which have occurred since 1822; the table being compiled from various sources and yet confessedly very imperfect, owing to the disgraceful negligence which has prevailed, until quite a recent period, at this and our other hospitals in the keeping minute and accurate records of the cases treated within their walls. It is singular enough to find one of the surgeons of that hospital now acknowledging the assistance he has derived from the "*Lancet*," in rescuing several of these from oblivion. With all its early faults, who can doubt the great

services that publication has rendered to the profession by laying open the rich veins of information buried in those establishments, or only existing therein for the advantage of the favoured few ? In the 144 cases of this latter table we find 124 cures and 15 deaths recorded, and five cases in which the results are not reported ; but, to make up the number of operations which have taken place in the hospital since 1822 (the date of this last table), viz. 159, we have to add another 15 cases of which no record whatever seems to have been kept. From a so-called statistical table like this, it is obvious that no safe conclusion can be drawn, and we cannot therefore agree with Mr. South, that meagre as the reports are, "they are highly important, as showing that the lateral operation is neither so dangerous, nor so much to be dreaded, if the after-treatment be well attended to." A statistical table, which gives us no account whatever of the results of 20 out of 159 operations is obviously useless, and may become, if relied upon, positively mischievous. As age is an important item in the estimate of this operation, we may mention that, of the 125 cases in which the age is recorded, 58 operations occurred in children 10 years old and younger ; 26 in young persons from 10 to 20 years of age ; 17 in persons aged from 20 to 50 ; and 24 in persons aged more than 50 years.

Radical Cure of Hydrocele.—Chelius acknowledges a preference of incision of the tunica vaginalis to its injection, which will certainly meet with no assent from surgeons in this country. He says :—

"It is not advisable to produce upon the testicle any irritation like that in the vaginal tunic. By incision, all the complications can be most distinctly made out, at the same time any existing intestinal rupture can be properly treated, the inflammation be more properly excited, and effect a more safe cure. The bleeding which occurs in this operation is easily staunched ; the severe symptoms occurring after it are most commonly the result of bad practice. After the cut it is in most cases necessary to insert a half-unravelled piece of linen between the wounded edges of the vaginal tunic. Injections operate uncertainly, as the irritability of the individual cannot be previously determined. They act as violently on the testicle as on the scrotum ; if a part of the injection be poured into tde cellular tissue, which is possible, even with the greatest care and attention, very dangerous symptoms may arise therefrom ; in a diseased state of the testicle, which cannot always be decidedly made out, injections are necessarily hurtful. The superiority of injections, to wit, that by their use the cure follows more quickly, and that the patient does not need to be kept quiet so long, is of no value, as even after injection the cure is often longer protracted than after incision, and with the latter keeping so long quiet is not necessary." Vol. II., p. 506.

Few surgeons, we imagine, who have witnessed the usual successfulness of so simple an operation as the injection of a hydrocele, will be disposed to adopt a procedure which requires the opening up the whole length of the tunica vaginalis, "care being taken that the testicle do not protrude, and if it should, it must be gently returned," and stuffing the wound with a "half-unravelled piece of linen." Mr. South speaks highly of the use of *Iodine*, so strongly recommended by the Indian practitioners and by Velpeau as the material for injection.

' "The injection of tincture of iodine diluted with water, I am convinced, by repeatedly practising it for some years, is the most effectual and least painful to the

patient. Velpeau has the credit of introducing the practice, but I am informed by medical friends who have been in India, it has been long practised there (this is acknowledged by Velpeau and every one else), and, if my recollection be not treacherous, without drawing the injection off again. And this mode I have adopted, making use of a very fine trocar and canula, drawing off the water, and injecting an ounce of fluid containing 3 ij. Tr., and 3 vj. W., and then immediately withdrawing the canula, to which the wound always clings very tightly, as the solution is very astringent. The patient most commonly suffers no pain, or at least a very trifling degree; and although on the following day the scrotum is a little reddened and rather firm, yet the patient is not thereby prevented from moving about with ease and comfort to himself. Indeed, I am informed that as soon as the injection is made the person walks away, and requires no further attention. The scrotum, according to my own observation, increases a little, and becomes rather more solid for three or four days, and then begins to subside, and in the course of a fortnight the cure is completed without confining the patient more than two or three days, rather as precautionary, than that I really believe it necessary. I have employed this treatment both in large and small hydroceles, merely injecting the quantity mentioned or a little less; and never either shaking the scrotum about or distending the cyst by repeated injection, and afterwards drawing it off, as Velpeau practises and recommends. I am quite sure whoever employs the iodine injection as I have mentioned, will not treat a hydrocele for the radical cure by any other means." P. 504.

Question of Removing a Scirrhous Breast.—We believe the experience of the best informed surgeons is increasingly adverse to the undertaking this operation. Formerly it was believed that the unsuccessful results depended upon its being too long delayed; but more accurate and extended observation proves that this is only one of the elements necessary to be taken into consideration. Mr. South quotes the statistics of Leroy d'Etiolles (*Bulletin de l'Academie*, vol ix.), from which it appears that, taking the mean of the results of 300 operations in men, the duration of life was found to have been 3 years 9 months between the appearance of the disease and the operation, and 1 year 5 months after the operation. In 412 operations on women, it was 3 years 6 months before, 2 years 6 months after the operation. Of 801 cases 117 were operated upon in less than a year after the appearance of the disease, and in 61 the disease was known to have returned. Great differences prevailed according to the tissue affected. Of 633 men suffering from cancer 165 were attacked in the *lip;* while of 2148 women but only 54 were so affected—the difference probably depending upon the use of the pipe, which may also serve to explain the greater proportion of relapses which occur in women than men. In 22 operations upon women there were 7 returns; in 114 operations upon men 15. Cancer of the *tongue* is also of more frequent occurrence in man; but the termination in either sex is equally unfavourable. Of 633 cancers in men 18 were seated in the tongue; and of 2148 in women 2 only attacked that organ. Of 9 operations 6 died of relapse, a year not having yet elapsed since their performance in the other cases. Of 277 operations on the *Breast*, 73 were performed within less than two years, and M. Leroy could not therefore furnish the results. Of the re-
ning 204, there died 22 in the year after the operation, and 87 had a
n, making the whole relapses 109, or more than a half.
elius sanctions the operation in cases wherein few would undertake it

in this country. "Where the scirrhus is already in the state of concealed cancer, the nipple much drawn in, the skin less free and moveable, the general health affected, menstruation irregular or entirely ceased, the result of the operation is indeed doubly doubtful. It is, however, the only remedy to prevent certain breaking. If the scirrhus be already ulcerated, if it be immoveably connected with all the pectoral muscles, if there be also hardening of other organs, no cure is indeed to be expected from the operation. It may, however, in so far, in such a case, be considered as a palliative, as the patient is at least free from the great inconvenience attendant upon the destruction of a scirrhous tumour by ulceration. I have not noticed a quicker progress of the disease after the operation, but, on the contrary, considerable relief for a long while." This would seem to justify its performance at any stage; but a few lines farther on we find a qualification of the advice. "It must not be overlooked, in deciding upon the removal of a scirrhous breast, that in cases where cancer has been very slowly developed and accompanied with no great pain, that after the operation the ulceration again proceeds even quickly, and thus the operation only hastens the fatal result." That it does so likewise by urging into activity dormant cancer of the internal organs numerous examples prove. After citing the experience of A. Cooper and Brodie in proof of the great liability to quick relapse and speedy death after operation, and alluding to two or three cases in which such relapse has been delayed for many years, Mr. South continues:—

"Notwithstanding these few favourable instances, surgeons should be cautious in urging a patient to submit to an operation for a scirrhous tumour, and still less, when it has become a cancerous sore, and the neighbouring glands have in either case become affected. He cannot promise a cure by the operation; nor can he even say that the patient's condition will not be made worse. I have often heard it stated that though the operation will not cure, it will put off the evil day, and retard the ulcerative process: but this I do not believe, for I have known many instances to the contrary. The only thing an operation can do is temporary palliation, if the patient be subject to severe shooting, stabbing pain, which is not indeed very commonly the case, unless the disease be worried by local attempts to cure. The practitioner ought, when consulted under these circumstances, to break to the patient cautiously the nature of her complaint; should inform her that all which can be done by operation is at least merely palliative; and should leave her to decide upon whether she will yield herself to the operation, knowing the risk and slender hope connected with it, rather than urge her to an operation which is without doubt, as regards scirrhous swellings, the most unsatisfactory in the whole course of surgical practice." Vol. II., p. 797.

Application of Blisters.—Mr. South supplies some useful hints under this head. He observes that the most cleanly and efficient mode of forming a blister is to sop a fold or two of lint in the acetum cantharidis and apply it to the part for a few hours. When the skin is irritable and thin, it suffices to freely apply it by means of a camel's hair pencil. When the blistering plaster is, however, employed, a piece of tissue paper should intervene between it and the skin, which, if the blister be closely applied, will not impede its action, while it will prevent small portions of plaster adhering to the skin or cutis and causing much unnecessary torment. The length of time the blister is to be kept on is too often left to the nurse's

decision. It should not be retained on longer than the skin becomes fairly raised, which is usually the case in from six to eight hours. (And we may observe that the blister acts much more rapidly if its surface be previously well moistened with Spt. of Turpentine.) In *children* it should be taken off as soon as a bright redness of the skin is induced, which is generally in two or three hours. "Indeed with children I am by no means sure that, in most cases, a mustard poultice is not preferable to the application of a blister. It should be made with mustard (not such as has been long in the house) and warm water, rather thinner than if for the table, as if made stiff it is much less active. It should then be spread about a quarter of an inch thick on fine muslin, and another layer of muslin being put upon it, applied to the part, and kept on ten, fifteen, or twenty minutes, according to the redness and pain. In some persons it will even blister. When removed, the skin should be carefully sponged clean with warm water, otherwise the irritation, which is very great, will continue. In the few persons whose skin is blistered with difficulty, it is best to apply previously a mustard poultice till the skin becomes reddened and painful." Mr. South disapproves of open blisters, as giving only unnecessary pain, and in cases where these seem indicated prefers a rapid succession of small blisters about the size of half-a-crown. "Another very excellent and very gentle mode of blistering is with *croton-oil*, ten or twelve drops of which should be gently rubbed over the surface with the finger, protected in a piece of oiled silk, for two or three following nights. Usually slight stinging is felt, accompanied with puffiness of the part on the second or third day, and this is followed by a crop of small vesicles, which speedily mature, in a day or two after dry up and fresh cuticle is formed. It is one of the best modes of blistering (?), if not required to be speedy."

Amputation.—Mr. South gives some useful directions for the use of the *Saw*, which, if we may judge from the bungling manner we have seen this instrument employed by hands not otherwise inexpert, are not uncalled for.

"A good saw should have its teeth well set off, as the carpenter's expression is, that it may neither clog nor hang in its track; and it should have, proportionally to its size, a heavy back, which renders its steadying more easy, and affords all the weight the saw requires to be loaded with. The too frequent mode of using it, is to drop its end, whilst the handle is raised, so that when moved it works obliquely; the operator, at the same time, throwing as much of his own weight as he can conveniently spare upon the handle, as if with the intention of forcing the blade of the saw at one or two strokes through the bone, and then driving it downwards and upwards, as violently and as quickly as he can, and often using about as much of the toothed edge as a young violinist does of his fiddle-bow. The consequence is, that the saw works badly, is continually jumping out of its track, makes another, and finishes by splintering the bone, and often cutting through it below where it was purposed. To use a saw properly, it should always, where possible, be held and worked horizontally, moving it forwards and backwards without any pressure of the hand, but allowing merely its own weight to keep it on the appointed place; and as it is moved forwards, even its own weight should be lessened, by slightly supporting instead of pressing down the saw. After drawing the toothed edge at first backwards, and then moving it forwards lightly on the bone till a shallow track is made, it may be

moved freely, so that at least two-thirds, or even more, of the saw shall act. The strokes should not be quick but long; and if so made four or six of them will cut through the thigh or shin-bone more quickly and more cleanly than twice as many short, hurried strokes, and without any risk of splintering the bone, or slipping from the part chosen to saw through." Vol. II. p. 893.

Results of Amputation.—The result of Mr. Philipps' statistical enquiry is, that the mortality in France, Germany, America and England put together, is $23\frac{7}{15}$ per cent. In 276 cases of amputation which have occurred at the Glasgow Infirmary, Dr. Laurie states there have been 176 recoveries and 100 deaths. Potter states that among 66, performed at University College Hospital in 1835-41, there were but 10 deaths, three of these occurring in instances of immediate amputation after accidents. Mr. South has performed 54 amputations at St. Thomas's in 1835-40, and among these 41 patients lived and 13 died, which he says is the general proportion at St. Thomas's. "Putting these together with the cases at University College Hospital, it must be evident that the mortality is a long way below 50 to 75 per cent., which has been stated by some surgical writers as the ordinary average of amputations. It will be observed, also, that the largest mortality is among the cases operated on for accidents, and on the lower extremities. In 7 amputations through the thigh I lost6; and of 9 through the leg 3 died; whilst of 6 primary and 1 secondary amputations in the upper extremity, not a single case was lost. This excess of mortality in operations after accidents is to be ascribed, when the patients die early, to the conjoined shock of the accident and operation. Besides which, the persons admitted into hospitals for such injuries are commonly free livers with broken-down constitutions, the like of whom are not unfrequently destroyed by the results of trivial accidents, which run either into erysipelas, or diffuse inflammation and gangrene."

Circumcision in Phymosis.—The mode of performing circumcision among the Jews is thus described. The child being held across the thighs of a man who is seated, the circumcisor grasps the prepuce with the thumb and fore-finger of his left hand, draws it forwards, and inserts it in the cleft of an instrument similar to a silver spatula. The penis is held upright, and the prepuce cut close off to the spatula with a single stroke of a button-ended knife. The circumcisor then, as quickly as possible, seizes the inner fold of the prepuce with his thumb-nails, which have been specially cut for the purpose, and tears it up to the corona glandis. He then spirts some water from his mouth over the wound, takes the penis in his mouth, and sucks the blood out of it a few times. A strip of fine linen is now wound round the corona and the cut surfaces, as a dressing, and the penis laid upon the pubes, in a ring to prevent its being touched.

Mr. South observes, in respect to this ancient and effectual procedure:

"Many years ago I was present at a Jewish circumcision, and was so much struck with its facility and appropriateness to the purpose, that I have ever since performed the operation in the same manner, except that, instead of inserting the prepuce in the cleft spatula, I merely grasp it with a pair of dressing forceps, as close as possible to the glans, and then cut it off before them. The tearing up the inner part of the prepuce to the corona is a very important part of the operation, and far preferable to its division to that extent with the knife, as whilst the inflammation is

subsiding, the cut edges, especially near to the angle of the wound, are prone to adhere together by quick union; and even if this do not extend far it causes a girthing of the glans, which is inconvenient and often requires a second division to complete the cure. By tearing the inner skin, which should always be torn completely behind the corona, or the operation will be useless, the edges of the wound become slough, and disposition to quick union is prevented. From having repeatedly performed circumcision in this way I am sure it is the best mode. And I may add that, as regards circumcision or slitting up of the prepuce, the former is in every case much to be preferred. I never, however, put in any stitches, as they are not merely superfluous, but add to the necessary inflammation without sufficient reason."—Vol. II., p. 345.

Reduction of Paraphymosis.—We have so frequently seen the operation of paraphymosis—so painful in its performance and oftentimes followed by such tedious ulceration—harshly and unnecessarily resorted to, that we are desirous of transcribing Mr. South's excellent remarks upon its reduction, although they are rather long.

"The reduction of paraphymosis is often exceedingly difficult, and always excessively painful, so that frequently a strong-minded person will scream like a child from the pain. I have, however, scarcely ever known it fail, and hardly remember it needful to perform any operation with the knife. It requires, however, great patience and perseverance, often for the space of half-an-hour, at the very least, and I have often succeeded when the prepuce had been everted six or eight days, and it might have been supposed that the adhesive inflammation would have prevented the replacement of the skin. Although the diminution of the bulk of the glans, by pressing the blood as completely as possible out of it, is a very important part of the proceeding, yet squeezing out the fluid effused in the prepuce is no less so; and unless both be done, there is great hindrance to the replacement of the skin. I therefore always first squeeze gently but steadily, for a few minutes, the prepuce, till it become somewhat flaccid, and then firmly press the whole glans with the thumbs of both hands, whilst the two fore-fingers grasp the penis behind the prepuce, like a collar, and draw it forwards, whilst the thumbs empty and thrust back the glans. Or if I do not so succeed, I grasp the whole penis with my left hand, making the thumb and fore-finger a collar behind the inverted prepuce, which thus rests against it, whilst, with the thumb and fingers of the other hand, the glans is emptied, and thrust within the constricted ring, by pushing first one part and then another of the corona glandis till it gets beneath the constricting band; and this done, the rest soon follows. Immediately that the least bit of the corona has been thus got in, that next it must be poked in (no better expression than this can be used) with the finger end, and so the next, until the greater part has been thus returned, and the reduction is speedily completed. It must, however, be remembered, that directly the return has commenced, the poking must be continued without intermission, as otherwise the whole proceeding will have to be repeated, as, on the least cessation, the glans again fills, and protrudes. If the everted prepuce do not relax by pressure, the constriction being so great that the effused serum cannot be dispersed easily upon the body of the penis, it will be found very convenient to make a few punctures through the skin, by which the squeezing presses out the fluid, and then the prepuce is rendered flaccid. It is always desirous to try this mode of proceeding, even if a part of the prepuce should have become gangrenous, as this is often merely superficial, and the replacement puts a stop to its progress. After the reduction, it is well for some hours to wrap the penis up in linen, and to keep it constantly wet with cold water, for the purpose of preventing the disposition to erection, and reprotrusion of the glans, but after that time, a warm poultice will be most agreeable to the patient's feelings, and most favourable to the disper-

sion of the inflammation. The soreness, however, will commonly continue for many days, proportionate to the severity of the constriction and its duration. No attempt at retraction of the prepuce, to see what is going on inside, should be made for several days, or the mischief will probably recur." Vol. II., p. 350.

Curvatures.—Chelius takes a comprehensive general view of these before entering upon a description of the different varieties. As the erect position of the body and its various organs depends upon the *equal antagonizing operation of the muscles*, and on the *firmness of the bones*, whatever interferes with the muscular antagonism or operates changes in the osseous structure, is competent to induce curvature. Muscular antagonism may be disturbed either by the preponderating activity of certain muscles, or by the mere normal action of others operating against an enfeebled condition of their antagonists. This last condition may result from palsy, wounds, debility of the muscles, their continued rest, spasm, continued exertion in certain positions, especially in children, or from diseased changes in muscle produced by gout, rheumatism, ulceration, ossification, &c. The activity of the flexors, especially in the fœtus, exceeding that of the extensors, the greater number of congenital and original curvatures occur in the course of the former. Muscles which produce the curvature always undergo more or less contraction or shortening, so as to be capable of little or even of no extension; and if this condition persist they lose their fulness, become thin or even cord-like, and are at last converted into a fibro-cellular or fatty mass. These changes are always the same whatever cause may have produced the contraction of the muscles, and are due to the *continued rest* of these parts—the tissue becoming atrophied, just as occurs in other unused parts.

"The rest of a muscle, when its contraction has once taken place, is therefore continual, because all voluntary motions which the patient attempts with the curved part, can occur only in such one way and direction, that thereby no outstretching and extension, but only a greater shortening of the contracted muscles, can be effected. A close observation of the motions in curvatures, especially in the feet, shows this remarkably. It is clear, that under such circumstances, the nervous influence and nourishment must be diminished in the muscles, and the diminution of the nervous influence may increase up to actual palsy, although the contraction of the muscle continue. In this way also is explained the reason why in palsy, which originates from the brain, and loss or diminution of the voluntary influence depending on the muscles, the muscles are contracted, whilst in palsy, proceeding from the spinal marrow, they are lax and atonic. Spasm, produced by topical causes in the muscle itself, or by reflected activity of the spinal marrow, may be the first origin of contraction of the muscles, and of the curvature thereon depending; but the continued contraction of the muscle is not to be considered as a consequence of *continual* spasm, but of the sustained rest of the muscle, and its diminished voluntary action. The same is observed in inflammation, and in all painful affections, when certain muscles are kept in a continued quiet state. The most direct proof of this opinion is given by the bearings of the limb, if its natural direction be restored, in which case the recapability of motion and the voluntary influence again gradually returns, and in the same measure the nutrition of the muscles is increased, and their bulk enlarged, as I have especially observed, after the cure of curvature by cutting tendons." Vol. II., p. 150.

The normal condition of the bones may be disturbed by [illegible] osteo-

malacy, scrofula, inflammation, &c.; when the softened bones yield to the action of the muscles, or the weight of the body.

The *prognosis* of curvatures depends upon their extent and duration and the manageable character of their original causes—being the more favourable the younger the subject and the less the curve. In old and bad cases our agency will be limited to the prevention of an aggravation of the malady; while, when organic changes of the bones and joints have occurred, the case becomes incurable. "Curvatures resulting from muscular contraction usually admit of a more favourable prognosis than those which proceed from diminished connexion of the bones. But if the muscles have become so wasted by long-continued curvature that their lengthening can be of no use, which is, however, difficult to determine, they are incurable."

The *cure* of curvature depends on the removal of the causes and the restoration of the natural direction to the parts. In a diseased condition of the bones no benefit can result from a mere mechanical treatment, unless such condition be rendered amenable to the therapeutical procedure adapted for it. When dependent upon a disturbed equilibrium of the muscular activity the cause of this must be sought for. The frictions usually recommended are chiefly useful from the motion and extension of the contracted parts which their employment renders necessary. On account of the diminished nervous influences in the parts, counter-irritation may act beneficially by quickening and increasing the vitality, especially in actual palsy. Kneading, rubbing, and stretching the muscles, and suitable gymnastic exercises, will, with due attention to the general health, remove the slighter curvatures, but where these are considerable, suitable machinery and apparatus are requisite.

"If with long-continued curvature from shortening of the muscles, and change of their tissue have been produced, that by the treatment proposed it can be removed either with extreme difficulty, or not at all, the subcutaneous cutting through the shortened muscles, or their tendons and aponeuroses (*myotomia tenotomia*) if possible, is the most proper remedy. Between the two ends of the divided tendon which retract, the upper more strongly than the lower, blood is effused, which coagulates and unites with the whole internal surface of the wound, and especially with the ends of the tendon. Exudations of plastic lymph soon occur, particularly from the ends of the tendon, presenting whitish thread-like streaks, running from one to the other, and gradually form a mass resembling fibrous tissue, which is capable of due extension, and sufficiently strong to answer the function of the muscles. This operation is therefore especially indicated under the above-mentioned conditions, if there do not at the same time exist such considerable changes in the bones, and from the long continuance of the disease, such a degree of wasting in the muscles and the whole limb, that by the mere lengthening of the muscles, the restoration of their natural position cannot be effected, which, however, it is often difficult previously to determine, and when the causes giving rise to the contraction, gout for instance, still exist. The various objections made to this operation, the repeated shortening of the tendon by the gradual contraction of the newly-formed intersubstance, as observed in every scar, as well as the injury to the natural direction and motions of the part from excessive activity of the antagonizing muscles, are without foundation, and contradicted by the large experience of modern times. The pain and wound are usually slight, and no particular symptoms occur. If in rare cases such be observed, as violent inflammation, with destruction of the cellular tissue, exfoliation

of tendons, and so on, they must be ascribed rather to the peculiar re[illegible] constitution of the patient, or to the proceedings in the operation and t[illegible] ment, than to the operation itself. The straightening of the part, and the stretching of the tendons and proper apparatus, is most properly commenced some days after they have been cut through, when the external wound is healed, to which time a light bandage covering the parts keeps it in a proper position. The employment of extension immediately after the division is improper, as thereby the two ends of the tendon are too far separated, and bad symptoms may be brought on. Too late use of extension, when the intermediate substance has attained firmness, renders the lengthening difficult, and even impossible." P. 151.

In taking leave of Mr. South's labours, we must join in the regret we have frequently heard expressed, that with so much industry and so large an accumulation of experience at command, he did not rather produce an original work than undertake this translation. We cannot compliment him upon the elegance of his rendering, many of the sentences being very obscure, and others ungrammatical, as our readers may perceive by our extracts. As we before stated, however, the work will be found very useful to both student and practitioner; and we heartily wish the spirited speculation of its production the success it deserves.

On the Causes and Treatment of Abortion and Sterility; being the Result of an extended practical Enquiry into the Physiological and Morbid Conditions of the Uterus, with Reference especially to Leucorrhœal Affections and the Diseases of Menstruation. By *James Whitehead*, F.R.C.S., Surgeon to the Manchester and Salford Lying-in Hospital. 8vo. pp. 426. London and Manchester, 1847.

The simple title of this work gives a very imperfect idea of its contents. The subject of Sterility occupies a mere fraction of space, and upwards of one-half of the whole volume is taken up with an elaborate account of Menstruation as a physiological process, and of the disorders which its deviations from health are apt to produce. That there is a good deal of repetition and unnecessary prolixity in the details on many of the topics that are discussed, must be admitted. Nevertheless the work is, on the whole, valuable and instructive, and one that reflects much credit alike on the industry and practical skill of the author. It contains ten chapters, of the leading contents of which we shall now endeavour to give a faithful summary, reserving most of our comments to the close of the article.

Chap. I. is devoted to the subject of Menstruation, more especially in reference to the physical properties of the menstrual flux, and to the source or seat from whence it is derived. As the properties of this fluid, when it escapes from the body, are believed to be very essentially modified by ad-

mixture with the secretion of the passage along which it is discharged, it will be useful briefly to notice the latter in the first place.

Nature of the Vaginal Mucus.

"The mucus of the vagina, in its normal state, always exhibits acid properties that of the uterus is as constantly alkaline. In no instance have I found the secretion of the vaginal mucous surface produce an alkaline reaction, except in gonorrhœal affections, and in inflammation from other causes, resulting in the secretion of pus. On the other hand, the discharges from the interior of the uterus, whether diseased or healthy, with the exception of those of an ichorous nature, have been, in every instance where I have had an opportunity of testing them, invariably alkaline. In cases of retention of a clot of blood or of a piece of the placenta, or where a portion of the ovum or the whole of it, remains for a length of time unexpelled, after it has ceased to exist, a circumstance which often happens during the early months of pregnancy, the discharge exhibits acid properties, emitting at the same time an offensive odour: this obviously arises from the process of putrefaction, by which these morbid fragments are frequently thrown off.

"Mucus like albumen has the property of coagulating on the addition of an acid, owing, probably, to a small proportion of albumen which, in its healthy state, it is always found to contain. Mucus, however, as well as albumen, will bear the admixture of a small quantity of acid, or of alkali, without undergoing any change in its appearance. But the acid secreted along with the vaginal mucus is, under normal circumstances, in quantity sufficient slightly to coagulate it, and in this state the fluid is generally found; except during menstruation, or when the uterine mucus happens, which is extremely rare, to be produced in unusual abundance.

"The parts being in a perfectly healthy condition, the whole surface of the vaginal membrane is covered by this form of mucus, which, when in very small quantity, is transparent; but in places where it is found collected in larger quantity, and especially when collected within the speculum tube during its introduction, it has a milky appearance, is perfectly opaque, and much less viscid than other mucus. It is the mixture of this acid product with the true catamenial fluid in its transit outwards, which gives to the latter the peculiar properties by which it is generally said to be characterized." P. 20.

The acid in the vaginal mucus is, as we shall presently see, said to be similar in its properties to the Acetic. It "enters readily into combination with the protein compounds contained in the blood and the secretions, dissolving them in all proportions, and communicating to the product with which it is associated, strong antiseptic properties. Moreover, it deprives the vaginal mucus of the viscidity which other mucus possesses, and thus renders it less liable to thicken and accumulate between the folds of the membrane; and it is to this agency, likewise, that effused blood may be for a much longer period arrested in the vagina than within the uterus, without undergoing the putrefactive change." This remark will prepare the reader for our author's views respecting the catamenial discharge, the properties and characters of which, according to him, depend in a great measure upon its admixture with the acid mucus of the vagina. Let us first see what is the *nature of the true* (i. e. unmixed with this mucus) *menstrual secretion*. And, first, how is it to be obtained?

"For the purposes of examination, it may be procured by means of the speculum, care being taken always to remove, by the aid of a piece of lint or sponge

of tendons, and so on, they must be ascribed rather to the peculiar re[illegible] constitution of the patient, or to the proceedings in the operation and t[illegible] ment, than to the operation itself. The straightening of the part, and the stretching of the tendons and proper apparatus, is most properly commenced some days after they have been cut through, when the external wound is healed, to which time a light bandage covering the parts keeps it in a proper position. The employment of extension immediately after the division is improper, as thereby the two ends of the tendon are too far separated, and bad symptoms may be brought on. Too late use of extension, when the intermediate substance has attained firmness, renders the lengthening difficult, and even impossible." P. 151.

In taking leave of Mr. South's labours, we must join in the regret we have frequently heard expressed, that with so much industry and so large an accumulation of experience at command, he did not rather produce an original work than undertake this translation. We cannot compliment him upon the elegance of his rendering, many of the sentences being very obscure, and others ungrammatical, as our readers may perceive by our extracts. As we before stated, however, the work will be found very useful to both student and practitioner; and we heartily wish the spirited speculation of its production the success it deserves.

On the Causes and Treatment of Abortion and Sterility; being the Result of an extended practical Enquiry into the Physiological and Morbid Conditions of the Uterus, with Reference especially to Leucorrhœal Affections and the Diseases of Menstruation. By *James Whitehead*, F.R.C.S., Surgeon to the Manchester and Salford Lying-in Hospital. 8vo. pp. 426. London and Manchester, 1847.

The simple title of this work gives a very imperfect idea of its contents. The subject of Sterility occupies a mere fraction of space, and upwards of one-half of the whole volume is taken up with an elaborate account of Menstruation as a physiological process, and of the disorders which its deviations from health are apt to produce. That there is a good deal of repetition and unnecessary prolixity in the details on many of the topics that are discussed, must be admitted. Nevertheless the work is, on the whole, valuable and instructive, and one that reflects much credit alike on the industry and practical skill of the author. It contains ten chapters, of the leading contents of which we shall now endeavour to give a faithful summary, reserving most of our comments to the close of the article.

Chap. I. is devoted to the subject of Menstruation, more especially in reference to the physical properties of the menstrual flux, and to the source or seat from whence it is derived. As the properties of this fluid, when it escapes from the body, are believed to be very essentially modified by ad-

mixture with the secretion of the passage along which it is discharged, it will be useful briefly to notice the latter in the first place.

Nature of the Vaginal Mucus.

"The mucus of the vagina, in its normal state, always exhibits acid properties that of the uterus is as constantly alkaline. In no instance have I found the secretion of the vaginal mucous surface produce an alkaline reaction, except in gonorrhœal affections, and in inflammation from other causes, resulting in the secretion of pus. On the other hand, the discharges from the interior of the uterus, whether diseased or healthy, with the exception of those of an ichorous nature, have been, in every instance where I have had an opportunity of testing them, invariably alkaline. In cases of retention of a clot of blood or of a piece of the placenta, or where a portion of the ovum or the whole of it, remains for a length of time unexpelled, after it has ceased to exist, a circumstance which often happens during the early months of pregnancy, the discharge exhibits acid properties, emitting at the same time an offensive odour: this obviously arises from the process of putrefaction, by which these morbid fragments are frequently thrown off.

"Mucus like albumen has the property of coagulating on the addition of an acid, owing, probably, to a small proportion of albumen which, in its healthy state, it is always found to contain. Mucus, however, as well as albumen, will bear the admixture of a small quantity of acid, or of alkali, without undergoing any change in its appearance. But the acid secreted along with the vaginal mucus is, under normal circumstances, in quantity sufficient slightly to coagulate it, and in this state the fluid is generally found; except during menstruation, or when the uterine mucus happens which is extremely rare, to be produced in unusual abundance.

"The parts being in a perfectly healthy condition, the whole surface of the vaginal membrane is covered by this form of mucus, which, when in very small quantity, is transparent; but in places where it is found collected in larger quantity, and especially when collected within the speculum tube during its introduction, it has a milky appearance, is perfectly opaque, and much less viscid than other mucus. It is the mixture of this acid product with the true catamenial fluid in its transit outwards which gives to the latter the peculiar properties by which it is generally said to be characterized." P. 20.

The acid in the vaginal mucus is, as we shall presently see, said to be similar in its properties to the Acetic. It "enters readily into combination with the protein compounds contained in the blood and the secretions, dissolving them in all proportions, and communicating to the product with which it is associated, strong antiseptic properties. Moreover, it deprives the vaginal mucus of the viscidity which other mucus possesses, and thus renders it less liable to thicken and accumulate between the folds of the membrane; and it is to this agency, likewise, that effused blood may be for a much longer period arrested in the vagina than within the uterus, without undergoing the putrefactive change." This remark will prepare the reader for our author's views respecting the catamenial discharge, the properties and characters of which, according to him, depend in a great measure upon its admixture with the acid mucus of the vagina. Let us first see what is the *nature of the true* (i. e. unmixed with this mucus) *menstrual secretion*. And, first, how is it to be obtained?

"For the purposes of examination, it may be procured by means of the speculum, care being taken always to remove, by the aid of a piece of lint or sponge

held between the blades of a sponge-holder, all the secretion from about the *os* and *cervix uteri*. This must be done immediately after the instrument shall have been properly adjusted, and will be found necessary in order that the fluid may be free from admixture with that exuding from the surrounding surfaces. In consequence of the irksomeness of the procedure, however, the patient will seldom be able to remain in the required posture longer than until from ten to twenty grains have accumulated. This quantity, although too small for the purposes of chemical analysis, will nevertheless be sufficiently large for determining its sensible properties, as well as for microscopic investigation." P. 21.

Now, without saying anything for the present, as to the decency of the proceeding here described, we shall only remark, that Mr. Whitehead believes, from his experiments, that the *true* menstrual secretion very nearly approximates to, if it be not identical with, the blood circulating in the capillary vessels, and that "the properties which are said to characterize *common* menstrual blood—namely, its want of power to clot, attributed to the absence of fibrin, its acid reaction, and its peculiar colour and consistence—are altogether acquired, and variously modified, by admixture with the vaginal mucus after the fluid has escaped from the uterine vessels;" the *true* menstrual fluid readily coagulating, and invariably exhibiting an alkaline reaction, as ordinary blood is found to do. It is obvious, therefore, if this view of the case be correct, that the menstrual discharge is to be regarded rather as a simple exudation than as a proper secretion.

That the change of the *true* into the *common* or *crude* menstrual fluid is owing to the action of the acid mucus of the vagina is attempted to be proved by some experiments.

"If the blood that has been drawn from the basilic or some other vein, or collected from a scarified surface, be allowed to flow into a cup containing pure or diluted acetic acid,—the same acid which exists in a free state in the vaginal mucus,—in the proportion of one part of pure acid to sixty parts of blood, and if the acid be properly diffused through the blood, the latter will remain uncoagulated, and become a darker colour if the blood be arterial, but lighter if venous, and no traces of uncombined fibrin can then be found in it." P. 25.

That the menstrual discharge proceeds from the internal surface of the uterus has been taken for granted in the preceding observations, and indeed is a *fact* that is now generally recognised in physiological science. "I have witnessed," says Mr. Whitehead, "in numerous instances the menstrual blood issuing from the *os uteri;* the labia, cervix, and all the vaginal surface being perfectly normal, and furnishing no such product. Some of these observations were made upon females affected with *procidentia uteri;* but, in the majority, the information was obtained by means of the speculum." He had an opportunity, about two years ago, of confirming these observations by a *post-mortem* examination. A young girl died from the effects of a menorrhagic attack; the hæmorrhage had ceased 24 hours before death.

"An early post-mortem examination discovered no organic lesion in any part of the body, which was everywhere drained of blood. The uterus was rather larger than natural; its parietes were less firm, but nearly of the usual thickness. Its interior contained a clot of blood which occupied the entire cavity. This clot, which

was an exact mould of the uterine cavity, measured, from its lower extremity, which terminated at the *os tincæ* to the part situated at the *fundus uteri*, two inches and a quarter; and between its two horns, one inch and three quarters. Each horn had a rounded extremity, was semi-transparent and fibrinous, and terminated at the corresponding Fallopian orifice. The Fallopian tubes were perfectly empty, and of the natural dimensions. The right ovarium was a little larger than the left, and presented several distinct cicatrices in different stages of reparation. One of these was recent, depressed, puckered, and appeared at first sight to have an opening in its centre; but it was afterwards found to be impervious. The cavity over which this was situated was occupied by a firm, reddish coagulum, traversed by yellow striæ, having an irregularly concentrical arrangement. Immediately beneath the next most distinct cicatrix was a firm yellow body, smaller in size than the preceding, and less striated. Another paler formation of a similar kind, but still smaller and more deeply seated, and marked by a faint indentation of the surface above it, was observed in the same ovary. There were also two vesicles of different sizes containing a thinnish glairy fluid, one of which was near the surface.

"The left ovarium presented on one of its sides a large bluish vesicle, projecting its peritoneal covering considerably above the surrounding surface; this was the size of a small hazel nut, and contained apparently nothing but serum. Two smaller vesicles more deeply seated, and two well-marked yellow bodies of different dimensions having corresponding cicatrices, were also observed in the same ovary.

"The *labia* and *cervix uteri* were perfectly healthy. The inner surface of the uterus presented numerous openings scattered over every part of it, obvious to the naked view, some being sufficiently large to admit a good-sized bristle, or the end of a lachrymal probe. The largest and most numerous were at each side of the fundus near the horns of the uterus, and at the contracted part of its body near the commencement of the cervix. The openings had a valvular arrangement, a great number passing downwards towards the cervix, while those at the upper part of the organ appeared to pass towards the Fallopian orifices." P. 35.

The appearances, discovered in this case, are fairly regarded by our author as confirmatory of the opinion that menstruation is in short nothing more than "a simple exudation of blood from the arterial capillaries in communication with the valvular orifices naturally existing upon the inner surface of the uterus."

But, if the *true* or proper catamenia invariably proceed from the inner surface of the womb, it is to be borne in mind that a sanguineous discharge, having the very same appearance, and possessed of the same physical properties, and returning, moreover, at regular monthly intervals, may proceed altogether and exclusively from the labia and outer portion of the cervix of the uterus, and this too whether the cavity of that organ be occupied with the product of conception or not. It is to this state of things that our author applies the term of *spurious menstruation*. And now comes the important (most important, certainly, if confirmed by subsequent enquiries) ætiological position, which he labors to maintain; viz. that "it (the discharge in question) is invariably associated with a morbid condition of the parts external to the cavity of the uterus, generally of its cervix and labia; sometimes of a portion of the vaginal mucous surface." Several facts are adduced in the way of proof: we shall presently have occasion to allude more particularly to them.

It has been already stated, that pregnancy may co-exist with this spurious form of menstruation. Not unfrequently, however, it is a mere sham or mock pregnancy that is present under such circumstances. This state is thus described by Mr. Whitehead:—

"Spurious menstruation occurring in the absence of pregnancy is accompanied with enlargement of the abdomen and of the mammary glands, nausea and occasional vomiting, alternate rigours and flushes of heat, languor, loss of rest, precarious appetite, and other symptoms tending to encourage a suspicion of the existence of pregnancy. The abdomen is sometimes enormously and painfully distended, as if caused by the accumulation of flatus, and then suddenly subsides, but seldom to the dimensions of the unimpregnated state; there is constant aching of the loins, hips, and hypogastrium; a sense of bearing-down, and inability to retain the urine the ordinary length of time. Pressure of the distended parts upon the large arterial trunks produces a sensible throbbing over the whole region, which, together with the visceral movements consequent upon the shifting of the confined flatus from one portion to another of the lower bowel, strengthens the belief in the existence of a fœtus in utero; and it is often a very difficult matter to convince the patient, under such circumstances, that she is not with child." P. 37.

Three examples are related in illustration of these remarks; but we must candidly confess that, while we admit that they certainly attest the importance of examining the state of the uterus with the speculum in some obstinate and doubtful cases, we are by no means quite satisfied that the monthly-recurring sanguineous discharge in all of them was not the genuine catamenia, and merely an exudation of blood from the ulcerated surface of the cervix uteri; as is alleged by our author, in consequence of his finding upon ocular inspection that the *os uteri* appeared to be closed and linear. That the reader, however, may judge for himself, we shall give the report of one case entire, especially as its details will serve to illustrate some other points, which will subsequently come under our notice.

"Sarah Neale, a married woman, twenty-six years of age, was admitted a patient of the Manchester Lying-in Hospital in January, 1846. She had borne two living children at the full term of gestation, and subsequently, one still-born at the end of the seventh month. At the time of her admission she stated herself to be in the seventh month of her fourth pregnancy; and sought relief at this period for an abundant leucorrhœal discharge which had existed ever since her miscarriage, and which she believed to be the result of a gonorrhœal affection, contracted from her husband, when between five and six months advanced in that pregnancy. To this cause she referred her premature delivery. Although nearly seven months advanced, she had felt the fœtal movements only very slightly and at long intervals; and she had menstruated regularly every month during the whole period, the discharge being precisely the same both as to its appearance, the number of days which it continued, and the attendant symptoms, as when not pregnant. She complained of unusual weight and fulness of the abdomen, a fixed pain of the right hypogastrium, constant aching of the loins, and a sense of bearing-down. The vaginal discharge which, in the absence of menstruation, was a yellowish matter, communicating a deep stain to the linen, exhibited a faint acid reaction, but was sometimes decidedly alkaline. The abdomen was of a medium size, somewhat larger on the right than on the left side, and the skin appeared a good deal loaded with adipose deposit. The mammary glands were enlarged, and the areolæ and follicles more than ordinarily distinct. A tumour, which appeared to be the uterus, was sufficiently palpable above the pubis; but no fœtal impulse was communicated, on careful manipulation, to the hand; nor could the placental *souffle*, or the beat of the fœtal heart be detected. To the touch, the uterus, although somewhat enlarged, was loose and floating; its cervix hard and hypertrophied, and the *labia* expanded and irregular. Examined with the speculum, the *os uteri* was found perfectly linear, and closed; the whole circumference of the *labia* was one mass of granulations of a purplish colour, covered with pus, and exuding a little blood from several points, caused pro-

bably by temporary pressure of the instrument. The adjacent vaginal reflection was thickened and varicose. My opinion was at once given that pregnancy did not exist which, however, the patient was unwilling to believe.

"During the following menstrual crisis, I examined the parts again with the speculum. The ulcerated surface was covered with blood, which, being carefully removed by means of lint, the parts were brought more distinctly to view. The *os uteri* was still completely closed, and gave no escape to any fluid; but the diseased parts were again covered with a sanguinolent exudation, and this was repeated as often as the product was cleared away; the uterine orifice still remaining closed and free from discharge as before. A course of alterative medicine, with repeated small bleedings by leeches from the hypogastrium, and, subsequently, the application of nitrate of silver to the ulcerated surfaces, effected a complete cure. The abdomen gradually subsided, the pain and bearing-down disappeared, and the menstrual function was restored to its normal state." P. 39.

It will be observed that the "normal state" here mentioned alludes not to any alteration in the physical properties, or in the periodic recurrence of the discharge, but simply and altogether to its seat or origin, as discovered by the use of the speculum.

CHAP. II.—*Conditions which principally influence Menstruation at its commencement.*

The following table is given by Mr. Whitehead, to show the age at which this function was established in 4000 individuals.

"At the age of	10	years,	9	first menstruated.
"	11	"	26	"
"	12	"	136	"
"	13	"	332	"
"	14	"	638	"
"	15	"	761	"
"	16	"	967	"
"	17	"	499	"
"	18	"	393	"
"	19	"	148	"
"	20	"	71	"
"	21	"	9	"
"	22	"	6	"
"	23	"	2	"
"	24	"	1	"
"	25	"	1	"
"	26	"	1	"
		Total	4000	

"Average age of the first menstrual crisis, fifteen years and nearly seven months." P. 46.

In treating of the influence of the habit or diathesis of the system on menstruation, a good deal of space is most unnecessarily occupied with common-place remarks on the general physiological subject of temperaments; and, not satisfied with this discussion, our author actually proceeds to discuss the origin and ætiology of Scrofula. That his reasoning is occasionally apt to be rather loose and illogical on some of the questions he undertakes to solve is pretty apparent from the story, which is recorded

" to show in what manner Scrofula may be induced by the action of mercury, independently of previous disease."

Mr. Whitehead is certainly not warranted, according to general experience, in attributing the development of scrofulous disease so frequently as he seems inclined to do to a venereal taint of the system. No sooner is this subject broached than forthwith he proceeds to narrate cases of syphilitic disease in infants being followed, at a subsequent period, by the appearance of scrofulous cachexy. If space permitted, it would be easy to show the inconsequential nature of several of the assertions. He is probably more correct, when he impugns the accuracy of many of the statements, that have been published by Commissioners and others, touching the injurious effects of factory labor upon the health of young females.

" It has been asserted for instance—and the assertion is quoted in every European country as referring to an absolute fact—that factory employment has a particular tendency to induce premature development of the sexual organs, and that a precocious womanhood is more frequently witnessed among the manufacturing population than among other classes of people. Until lately, I also entertained a similar belief, based, it must be confessed, upon the authority of others, whose statements I now believe to have been advanced at random, or founded, at best, upon a very few isolated cases, altogether inadequate to a general conclusion. A more extended inquiry into the subject has convinced me of this popular fallacy. The subjoined facts will show that, in this respect, mill employment has an effect the very opposite of that which it is generally believed to have.

"Of the 4000 individuals before quoted, 2127 were employed in mills, warehouses, and places connected with the various processes of manufacturing, at the period of puberty and for a length of time previous to its advent. Their collective ages amounted to 33,296, affording an average of *fifteen years* and nearly *eight months*; of whom 511, or 24·02 per cent. suffered under disease consequent upon undue retardation of the functional changes at the commencement.

" The remainder, 1873 in number, included all that were otherwise circumstanced; as domestic and farm servants, hawkers, sempstresses, and shop-women; together with a number of educated females not necessarily engaged in any pursuit, except school exercises. The sum of their ages was 28,982, giving *fifeeen years* and about *five months* as the average age of puberty; and 375, or 20·02 per cent. experienced functional difficulty in form of amenorrhœa at the onset." P. 81.

Mr. Whitehead declares, as the result of extensive observation, that the high temperature maintained in mills is far less injurious to health than has been represented by many writers. Nay, he does not hesitate to affirm that, were it not for the hurtful effects of exposure to a cooler, and often a cold, temperatureupon leaving the mills, " he feels persuaded that a more efficient or more valuable auxiliary could scarcely be employed in these latitudes, in the treatment of some strumous affections, than this artificial climate is capable of affording."

According to our author's observations, young females, the daughters of persons in easy circumstances, and who are therefore not engaged in any other occupation save the usual educational exercises, arrive at the epoch of puberty, as indicated by the advent of the catamenia, somewhat earlier than those who are labouring at some manual work. In the former, the change took place at the age of about 14 years and a half, " being more than a year earlier than the general average; and they suffered un-

der disease consequent upon functional difficulty at the rate of 32·30 per cent., a proportion far exceeding that of any of the classes already noticed."

Every medical man will recognise the soundness of the following remarks :—

"The cause of this peculiar susceptibility to disease in the class of patients now under consideration, becomes a question of paramount importance; and is, without doubt, to be found in the faulty system of education pursued at boarding schools. The mental faculties are called into premature activity, and tasked to a degree beyond their power of endurance, and the mind is crowded with a mass of matter under the fictitious denomination of accomplishments, which Nature never designed that it should entertain; while the proper training of the physical constitution is totally disregarded. Moreover, the indulgence of a propensity, irresistibly active at this period of life, to the reading of novels, romances, and works of a similarly vicious character, has likewise a very injurious tendency. Witness for instance, the violent and often dangerous paroxysms of hysteric emotions occasionally consequent upon the mere perusal of an affecting narrative, having probably no foundation save in overstrained analogy, or in fiction. These are agencies to which the unlettered are entire strangers, or which they experience but seldom, and only to a very limited degree.

"It thus appears that an occupation of whatever kind, and wherever pursued, which requires no more than a moderate application of mere physical power for its performance, will bear a very favourable comparison in regard to its effect upon bodily health, with one in which the exercise of these faculties is neglected. The fact is, the muscles were designed for a purpose; and their due employment is no less necessary to the harmonious working of the economy of which they constitute an important portion, than for maintaining a healthy relationship with the objects of external nature. An employment requiring moderate and regular exercise, especially if pursued in an open untainted atmosphere is of the first importance; while inactivity, whether the individual reside in town or country, is almost invariably followed by ill consequences. In no class of people is the menstrual function so healthily performed as in those necessitated to earn the means of subsistence by personal exertion; provided always that the employment be suitably adapted to the constitutional powers of the individual; while the converse of this is known to obtain with those in easy circumstances. Amongst farm servants and others, the period of puberty is seldom accompanied by any derangement of the general health; but in the higher grades of society, it forms an era of the greatest anxiety, and is often the commencement of serious and irreparable organic mischief." P. 90.

Chap. III.—*Diseases of Menstruation.*

There is much in this chapter, too, that might have been omitted without any detriment. A great portion of the details, connected with the subject of Amenorrhœa, serves only to swell the bulk of the volume. Witness, for example, the minute details of six or seven cases recorded to illustrate the effects of retarded menstruation in young girls, and of others to show the ill consequences of the sudden stoppage of the discharge from exposure to wet and cold, or from some violent emotion of the mind, &c.

The account given of Dysmenorrhœa is exceedingly incomplete. Mr. W. alludes to the occasional presence of ulceration of the *os uteri* in this morbid state, but without attributing the one to the other as its producing cause. Dr. H. Bennett, however, has recently endeavoured to show that, even in virgins, painful menstruation is occasionally connected with structural lesion of the uterine orifice, that a correct diagnosis cannot be formed

without the aid of the speculum, and moreover, that the disease cannot be effectually cured without appropriate topical treatment. He does not hesitate even to propose that the hymen should be divided, when the speculum cannot be easily introduced. Surely such an extreme measure as this is scarcely warrantable under almost any circumstances; but, as Dr. B. has promised to publish the particulars of some cases in which he deemed its adoption necessary, we shall only, for the present, express our earnest hope that no medical man will ever have recourse to the step in question, save with the sanction and concurrence of another professional man, as experienced at least as himself.

We are uncertain whether Mr. Whitehead has used the speculum in virtuous unmarried females. No distinct mention is made of it in the body of his work; but there is a strange allusion to it in the introductory remarks, in these terms:—"Some are admitted (into the Manchester Lying-in Hospital) who are neither pregnant nor suckling, as well as a number of young unmarried females, labouring under retention or suppression of the menses, leucorrhœa, prolapsus, hysteria, chlorosis, and similar affections connected with disordered menstruation. In all such cases, I made it a point, whenever practicable, to examine the uterus with the speculum, and almost invariably found the existence of disease of this organ sufficient in extent to bring the other symptoms under the arrangement of sympathetic disturbances merely. This view was confirmed by the results of the treatment adopted. I have now examined upwards of two thousand such cases, and the result has almost invariably been the same." Are we to understand from this passage that there is organic or structural disease of the uterus in all cases of "retention or suppression of the menses, leucorrhœa, hysteria"? &c. Surely not; the very supposition would be monstrous.

The section on *Menorrhagia*, or, as our author calls it, Metrorrhigca Menstruation, contains but little that calls for notice. Considerable confusion has been most gratuitously introduced into our author's remarks by his using the term *metrorrhagia* in a different sense from metrorrhagic menstruation; the former he employs to designate "uterine hæmorrhage, altogether unconnected with menstruation." It must be to this description of sanguineous evacuation that the following remarks on "metrorrhagic discharges" apply:—

"These are not unfrequently witnessed in young girls before the age of puberty; and they are by no means uncommon in those who have finally ceased to menstruate. In the former, such phenomena are usually associated with the hæmorrhagic diathesis; in the latter, with disease of the uterus, which when not of a malignant character, consists in ulceration about the *cervix*, often the result of varicose inflammation, or of *endo-uteritis*." P. 141.

Whether the "abnormal discharges of blood," referred to in a subsequent paragraph, belong to metrorrhagia exclusively, or whether they apply in part to metrorrhagic menstruation, it is not quite easy to determine:

"Abnormal discharges of blood frequently come on during the early months of pregnacy; or supervene upon suspension or irregularity of the menstrual function, arising from other causes, and being accompanied with abdominal en-

largement and the general indications of pregnancy. When this state of things happens in the young unmarried female, the investigation is often fraught with peculiar difficulty; the employment of the only means by which a satisfactory knowledge of the case can be arrived at, being obstinately objected to. And even should every facility for the procedure be afforded, whether the existence of pregnacy be substantiated or disproved; it is still one of the most delicate positions in which either patient or practitioner can be placed, involving, on one part, the discharge of a very disagreeable, difficult, and often thankless duty; on the other, sacrifice, to some extent, of character, and perhaps exclusion from society. For although the calumny which led to the investigation be proved to be unfounded, it is difficult to remove altogether the impression from the mind of the multitude: a woman's virtue cannot bear even to be suspected. Should an unfavourable opinion be pronounced and found erroneous—for after the most rigid scrutiny, the ablest enquirer may fail to discover the truth—the mistake will never be forgotten, but will remain a blot upon his character ever afterwards. Under all circumstances the practitioner cannot be too scrupulously guarded in delivering an opinion; and it should ever be his aim, however culpable his patient may be, to shield her fame, as far as it is practicable, from the censorious taunts of an unfeeling public." P. 144.

Mr. Whitehead has omitted to inform his readers how this awkward blunder may be best avoided.

Chap. IV.—*Last Menstrual Crisis.*

The particulars are given of a case, in which menstruation continued to be regularly and uninterruptedly performed from its first establishment in early life up to the 75th year. The woman was, at the beginning of the present year, an inmate of the Manchester Union Hospital. "It is remarkable that during every pregnancy (13 in all) she continued to menstruate until the last month of the full time, the discharge being similar in quantity and character to that of the non-gravid state; and that the discharge re-appeared, notwithstanding that she suckled her children, in two or three months after the birth of the child."

"With the exception of the pregnant periods just referred to, the menstrual discharge has suffered no interruption from its commencement until the present time, and there have been no particulars in which it has differed during the latter years of life from its state during her youth. At both times it continued for three days; and now, as then, it is preceded and accompanied by a sense of lassitude and pain in the back, which apprise her of its approach. The character, too, of the discharge as it occurs now, does not differ either in appearance or quantity from that to which she has been accustomed throughout life. She further states that her mother, who attained to a great age, was the subject of the same peculiarity; continuing to menstruate until the close of life; and that she had always heard a similar account of her maternal grandmother."

Mr. Whitehead mentions a still more curious case, in which a regular monthly discharge occurred in an old lady, 91 years of age, for upwards of a twelvemonth before her decease, although she had ceased to menstruate in her 54th year.

"In February, 1845, thirteen months before her decease, having suffered for several weeks previously from distension and general uneasiness of the abdomen, aching of the loins, and slight dyspeptic symptoms, a quantity of blood, bearing

all the characters of ordinary menstrual fluid, was observed escaping from the vagina, and at first occasioned considerable alarm. It came on in the night-time, an hour or two after she had taken, with the view of promoting perspiration, a cup of hot beer-whey, and it continued uninterruptedly three or four days, then ceased; leaving the patient perfectly free, however, from the pain and uneasiness before complained of. About three weeks afterwards a similar train of symptoms appeared; the discharge, preceded by the lumbar and abdominal uneasiness as on the former occasion, continuing from three to four days, after which she expressed herself completely restored. In this manner the functional phenomena were periodically developed at intervals of twenty-three or twenty-four days, during the remainder of her life; the health being in a good state the whole time, with the exception of some slight sympathetic disturbance at the critical periods. She died at the thirteenth or fourteenth accession, while the organic product was still escaping. On each occasion the discharge, both as to quantity, appearance, and the period of its duration, together with the premonitory and accompanying symptoms, were every way similar, so far as she was able to judge, to what they had formerly been during the activity of the catamenial function. An inspection of the body, after death, was not permitted." P. 148.

In another case, related by our author, menstruation has continued to recur regularly up to the 66th year; the lady is still alive.

From a tabular list of 69 cases, of whose history a brief abstract is given, it appears that the average age, at which menstruation ceases in this country, is between the 47th and 48th years of life. With respect to the diseases which more immediately appertain to this important change, our author remarks:—

"The forms of uterine disease commonly met with at the period of the last change of life, may, for practical convenience, be arranged under three heads, each distinguishable from the other by the character of the discharge with which, early or late, it is almost invariably accompanied; and also, by the kind and degree of suffering which it occasions in the organ immediately implicated, and the sympathetic disturbances simultaneously awakened in remote parts of the body. The *first*, and most common of these affections, is characterised by a muco-purulent discharge from the vagina, generally denominated *leucorrhœa*, or the whites; but which differs from simple leucorrhœa in several important particulars; the *second*, is often accompanied by vaginal hæmorrhage, the discharge differing, both as to its properties and in its source, from the menstrual product; the *third* form, is characterised by a watery, sanious, serous, or ichorous discharge, which is sometimes mixed with blood, sometimes with pus, mucus, or albumen-like shreds, and occasionally with small portions of fleshy matter, the product of the organic change upon which it depends. It generally emits an offensive odour." P. 155.

The *first* of these states is almost always associated with, and indeed dependent upon, the morbid state of the womb that is thus described:—

"The lesion usually consists in hypertrophy of the *cervix*, or of the whole organ, with an ulcerated state of one or both *labia*. The most common form of ulceration is that of a simple granulating surface with a defined margin. Sometimes there is induration of the anterior *labium*, which, when considerably enlarged, projects downwards in a conical form, and is generally excoriated or superficially ulcerated, especially on its posterior surface; the abrasion extending in an upward direction towards the inner *cervix*. At other times both labia are indurated, with or without abrasion; and, accompanying this state of parts, there is often also an ulcerated fissure, more or less deep, occupying one or both *com-*

missuræ labiorum. Occasionally, the *labia* appear but little, or even not at all enlarged, or in any way diseased, but the margin of the *os* presents a ring of intense redness. This is the sure indication of inflammation of the lining membrane of the uterus, or *endo-uteritis*; and is accompanied with enlargement of, and pain upon pressure being applied to, the body of the organ. That these morbid conditions of the uterus constitute the principal cause of all the disturbances before enumerated, is sufficiently clear, from the circumstances of their being most signally, often instantaneously relieved, by the mere use of local remedies, even without the assistance of constitutional treatment." P. 158.

For the cure of such cases, the general health must be improved by the use of alteratives and tonics; the local disease will require the occasional application of the nitrate of silver, either in the solid state or in that of a very strong solution—3 j. to 3 iij. of water.

When there is reason to believe that chronic Endo-uteritis is present, the treatment recommended by our author is thus explained :—

"It consists in the application of leeches to the *hypogastrium* or over the upper part of the *sacrum;* and in the exhibition of remedies similar to those employed in Case XXI. In addition, when all febrile excitement has become completely subdued, a more decidedly tonic plan will probably be indicated; and considerable benefit may be derived from injecting within the *uterus*, a weak solution of nitrate of silver in combination with *extractum conii*, or by the introduction of an ointment of the same material. The latter form of the remedy is applied by means of a small piece of lint fixed upon the end of a long probe, and fastened to the handle of the instrument with a thread, with a view of securing its safe withdrawal. Not only does no disturbance or discomfort of any kind ensue upon the employment of these local measures; but on the contrary, a beneficial change is often evident after the first application, and I have witnessed the suspension of pain to be instantaneous. The operation of injecting is done with the aid of the speculum, by means of a long syringe similar to Clarke's female syringe, having a nearly straight tube, with but one hole at its extremity. If carefully managed, from one to two drachms of the remedy, (which may be composed of different materials, varied in form and strength according to circumstances)—will pass within the organ at each operation, most of which returns almost immediately. The temperature of the fluid previous to being used, should be raised to about ninety degrees, Fahr." P. 163.

The *second* condition alluded to is, in very many cases, connected with the existence of a varicose ulcer of the cervix of the uterus. "The hæmorrhage, which is sometimes alarmingly profuse, may cease after a few hours; but it often continues many days, or weeks, and sometimes several months. On subsiding, it is replaced by a light-coloured discharge which is observed, occasionally, to be more or less mixed with blood, but consisting essentially of a combination of mucus and pus in variable proportions; the vaginal mucous membrane participating, in greater or less degree, in the generally increased action of the surrounding parts, thus furnishing its peculiar secretion in unusual abundance. The affection under this form, familiarly known as 'the whites,' may harass the patient for years, or, by gradually undermining the constitutional powers, may keep her in a state of suffering and misery to the end of life. In some instances the congestion, which at the onset was confined to the lower part of the organ, does not terminate in varicose ulceration, but, becoming re generally diffused, assumes the form of uterine phlebitis, accompa-

nied with abdominal enlargement, and alarming constitutional disturbance. Inflammation of the peritoneum is often a consequence of the further extension of this state of the affection, with effusion into the peritoneal cavity, constituting a common form of abdominal dropsy."

As an illustration of this varicose ulcer of the uterus, we may take the following instance:

A poor woman, 50 years of age, and who had been 14 times pregnant, ceased to menstruate in her 48th year. Five months afterwards, "the patient being in a very infirm health, with abdominal enlargement, creating a belief in the existence of dropsy, a profuse uterine hæmorrhage came on which passed away in clots of small size. This continued in a moderated degree, without ceasing, more than four months, when it was replaced by the leucorrhœal discharge, in its accustomed form. Her linen, which she exhibited, was covered with thick yellow stains, and the recent secretion was strongly alkalescent. She complained of a fixed, deep-seated pain of the right hypogastrium, near the groin; severe and constant aching of the right hip and thigh, and across the sacrum; frequent desire to void the urine, with sense of constriction in the region of the bladder; disordered digestion, and irregular bowels; attacks of alternate chills and flushes of heat; and great physical prostration. Examined with the speculum, the *cervix uteri* was found unusually tumid, and was traversed by a number of prominent venous branches of different sizes; the anterior *labium* was hard, hypertrophied, and excoriated; the posterior *labium* was also enlarged and completely occupied by a flabby, purple-looking sore with irregular surface, from which a small quantity of blood oozed out during the operation. The treatment consisted in the application of leeches to the hypogastrium; and subsequently in the use of the solid nitrate of silver to the ulcer and to the whole of the lower part of the uterus. She also took, night and morning, a grain of calomel combined with half-a-grain of opium, until the mouth became slightly affected; and a dose of the compound decoction of sarsaparilla, containing three grains of iodide of potassium, thrice daily for six weeks. The nitrate of silver application was four or five times repeated, at intervals of a week; and after relinquishing the sarsaparilla, she was ordered to take small doses of quinine and aloes, for several weeks more. The improvement progressed favourably from the first. She was discharged cured, on the 15th of July, three months after her admission; but appeared sufficiently recovered several weeks earlier."

Of the *third* morbid state to which we have referred, it need scarcely be remarked that fœtid sanious discharges from the uterus, occurring in women advanced in life, are justly considered as generally indicative of some serious organic mischief of the uterus. No prudent medical man, however, will even venture to give a definite opinion upon such a case, until he has been permitted to have not only a digital, but a specular, examination of the suspected parts. "Diseases of a malignant character," says our author, "are, generally speaking, preceded and attended by a similar train of symptoms, both constitutional and local, in each case; varying of course from time to time, according to the nature of the structure implicated, and the extent and severity of the local affection. A notable preliminary con-

dition is a plethoric state of the portal system of vessels, with occasional functional impediment of the organs in immediate relation therewith, and especially with symptoms denoting determination towards the region of the uterus. The final catamenial change is remarkable for profuse menstrual and leucorrhœal discharges, the latter usually persisting after the former has ceased to appear. Hæmorrhoids are a common affection under this state of the system; disordered digestion; pain of the head, especially at its summit and back part; erysipelas of the face; anasarca; disordered state of the urinary organs; and sometimes partial paralysis."

In the narration of a case of the "cauliflower disease" of the cervix uteri, it is remarked that "the speculum, the employment of which was so strenuously objected to at the onset, became the medium of administering, in the most gratifying manner, to the patient's comfort during the latter months of her life. The sore, by its aid, being fully brought within view, the whole of the accumulated, offensive secretion could be effectually removed, and the surface treated with a soothing anti-septic agent, which rendered her comparatively comfortable for a length of time afterwards. These applications, which were composed of chloride of lime combined with *opium*, sometimes with extract of *conium* or of *belladonna*, became necessary every second or third day. On two or three occasions, when the operation was too long delayed, she experienced an attack of shivering, with increased pain of the *sacrum* and *hypogastrium*, vomiting, headache, and hectic fever. Such accessions were frequently repeated during the last few weeks of her life, and were always found to be most severe when a portion of the ulcerated surface had become changed (which was several times observed) into a black, powdery, melanotic matter of a putrid and extremely offensive odour. This deposition could always be completely removed by means of lint held between the blades of a sponge-holder; and it was, doubtless, owing to the timely removal of this putrid substance and the application of powerful anti-septic remedies, that life was prolonged and rendered less painful: the melanotic change being thus prevented from implicating the deeper structures of the organ."

In the chapter (VI.) on the Signs of Pregnancy, two cases are related where, in consequence of co-existent ascites, the presence of pregnancy was not discovered even up to the last moment; and, in one, the patient was tapped only a few hours before "a full-grown living child was expelled." As the patient, in whom this unhappy accident occurred, recovered favourably, we may very fairly dissent from our author's opinion, that there could be "no doubt that the uterus was pierced by the instrument used in the operation, and that the fluid drawn off was the *liquor amnii*, as no evacuation of the kind by the natural passages preceded or accompanied the expulsion of the fœtus."

That the inexperienced may not be too much cast down by an error of diagnosis in such a complication, it may be some satisfaction for them to learn that, in the preceding case, no fewer than three medical men "of high standing in the profession" were most grievously mistaken.

Passing over our author's remarks as to the probable cause of Quickening, we proceed to notice what he says respecting the

Appearance of the Os Uteri during Pregnancy.

"The only test capable of revealing with certainty the existence of pregnancy during its early stages—from a few days after conception to the middle or end of the fourth month, when auscultation first becomes available—is that which the appearance of the os uteri presents to specular examination. It was before stated that, during menstruation, the labia uteri were in a state of high vascular turgescence, and the os tincæ, although elongated, and having its boundaries somewhat relaxed, was nevertheless closed and linear, except during the escape of the small menstrual clots before noticed. At the time of conception, the parts are thrown into a precisely similar condition; but no escape of fluid occurs to relieve the turgescence, which consequently continues to increase. In from ten to twenty days afterwards, the whole organ is found considerably enlarged, and the circulation through it augmented both in force and volume; the labia are thickened and apparently elongated, the commissures less distinct, and the *os* appears to be sunk in, or dimpled, owing to the distension and consequent projection of the labia below the level of the orifice. In the fourth week, the *labia* at the centre of their margins, are permanently separated to the extent of one or two lines; and the *os tincæ*, which was before a mere chink with parallel boundaries, is now seen to be an elliptical, or sometimes rounded aperture, which is occupied by a deposition of transparent, gelatinous mucus. At six or eight weeks it becomes decidedly oval, or irregularly circular, with a puckered or indented boundary, having a relaxed and lobulated character." P. 202.

"These changes of form of the lower uterine orifice are evidently owing to distension of the surrounding textures, caused by the increased flow of blood into their structure. The whole circumference of the cervix is enlarged in all its dimensions; the labia become less and less distinct by the simultaneous expansion of the commissures, so that at the stage above-mentioned the existence of the latter is altogether obliterated. After this period, the parts present a great variety of appearances, depending principally upon the state of the circulation through the uterine veins. The characteristic trait, however, is always maintained: namely, the patulent state of the orifice, occupied by a transparent gelatinous plug of mucus, and its relaxed, irregular boundary." P. 203.

That Mr. Whitehead is quite right in attaching so much importance to the alterations in the uterine orifice, here described as one of the surest signs of pregnancy during the first ten or twelve weeks of gestation, is more than probable. Some of the best obstetrical authorities of the day agree with him. Dr. Rigby, for example, in his excellent system of midwifery, says that "he is inclined to think that the soft feel of the portio vaginalis (of the os uteri) is one of the earliest signs of pregnancy which can be detected by examination:" the transverse fissure or slit of the orifice being, at the same time, rounder and more depressed than it is in the unimpregnated state. And, in another passage, he expresses himself still more emphatically. "In a primipara, the changes which pregnancy produces upon the os and cervix uteri, are generally sufficient to lead to an accurate conclusion. The round dimple-like depression which the os uteri forms, the soft cushiony state of the cervix, are changes which we consider as peculiarly the effects of pregnancy; but their distinctness and certainty cease when the patient has had several children; the irregular shape of the os uteri—its thickened edges, hard here and there—the os tincæ itself more or less open—the cervix scarcely, if at all, shortened, even at a late period of gestation, tend not a little to perplex the diagnosis furnished by this mode of examination; and where disease is complicated

with pregnancy, the difficulty is greatly increased, and not unfrequently so much, that scarcely a single satisfactory point will be obtained."

It may be here worthy of remark, that the purple colour of the mucous lining of the vagina at its external orifice, which has been so much dwelt upon by some continental obstetricians as a sign of pregnancy, has attracted but little attention in this country; chiefly, we believe, from the unwillingness of our medical men having ever recourse to indecent exposure, when not absolutely necessary.

After Quickening has taken place, changes in the form and dimensions of the *os uteri* are not sufficiently characteristic to enable the physician to determine the exact period of the existing pregnancy; "the orifice is sometimes as widely dilated, and its boundaries as much relaxed at the end of the fourth month, as it usually is two or three months later."

Examination with the finger alone can very seldom be trusted to, to determine the existence or non-existence of pregnancy from the state of the uterine orifice; and for these reasons:—

"The boundaries of the *orificium uteri* are not constituted of the identical material which formed the margin of the *os tincæ* of the unimpregnated uterus. They consist of the loose cellular and superjacent mucous tissue which was situated, in the unoccupied uterus, two or three lines external to the proper uterine orifice. After conception, these parts become infiltrated with serum; or sometimes the venous loops ramifying within them, are distended with blood, or become varicose; and the whole is projected more or less beyond the level of the parts as they existed before impregnation, thus completely obscuring the real *os tincæ*. On examining with the finger, this soft external boundary is very liable to escape notice; the more resisting part of the cervix being sought for, and found higher up in form of a firm ring, dilated certainly, in a very characteristic manner; but not conveying the same idea which the more relaxed external orifice gives when viewed through the speculum. The whole length of the cervix is expanded, from an early period of pregnancy, into a rounded tube, the size of an ordinary writing quill, and in this state it remains, but very slightly altered, until within a month or two of the completion of the term, when it merges itself, by little and little, in the general uterine cavity.

"A circumstance is also noticeable in reference to this subject, of which the touch can take no cognizance, and which, even when the speculum is used, may possibly lead the enquirer into error in cases where the state of the *os* alone is referred to as an evidence of pregnancy. The gelatinous plug which occupies and appears to distend the cervix from an early period after conception, does not remain in that cavity unrenewed: it is always in process of being replaced by a new secretion which is constantly going on; the old deposition being at the same time pushed downwards and dissolved, as it descends, in the vaginal mucus; but in so small a quantity as to escape the notice of the patient under ordinary circumstances. Sometimes, however, the action of the parts is suddenly increased, and the character of the secretion changed. The cervical plug is thus not unfrequently thrown off in a mass, leaving the cavity which contained it unoccupied, and in some measure collapsed, and the external labia fall together. This state of matters, however, is only of temporary duration, as a new supply of mucus is soon furnished, and the cervix becomes distended as before: but if an examination should be instituted at such a juncture, the practitioner, not being aware of the contingency, may be easily led to an erroneous conclusion." P. 207.

Some of Mr. Whitehead's remarks on the Auscultatory evidence of Pregnancy are liable to exception. Few will agree with him in regarding the sound, to which the term *placental souffle* has been improperly applied, as

"an unerring sign." But we shall not say more upon this point, but rather direct our readers' attention to some valuable illustrative remarks which he has made on the subject of *suppression of the menses*, as one of the signs to which undue importance is apt to be attached. He adduces several cases to show how fallacious this may prove, if accompanied with other indications of the pregnant state. We shall briefly epitomise them.

1. A woman, 29 years of age, had borne six living children at the full term of gestation. In each case, about three weeks after conception, the menses (or, at least, a sanguineous discharge) appeared in unusual abundance, sometimes being mixed with small coagula, and continuing to flow for six or seven days.

2. A young lady menstruated at four successive periods, viz. until the period of quickening, during her first and only pregnancy. She was delivered of a full-grown child.

3. A woman, who had had 14 previous pregnancies, "menstruated, to my knowledge, five times during her last pregnancy, which terminated favourably, and at the full period."

4. A woman had a discharge of blood, every way similar to the menses both as to quantity, appearance, and the period of its duration, in the third month of her last (the fourth) pregnancy, which terminated successfully. The discharge returned thrice afterwards, at the end of the fourth, fifth, and sixth months. There was also a leucorrhœal discharge.

5. A woman, who had been long affected with a leucorrhœal discharge, white and glairy before marriage, and yellow and puriform after it, became pregnant with the first child about three years after being married. Menstruation continued in its usual form during the whole period of pregnancy and lactation, and she was unaware of her condition until within a few weeks of delivery, believing herself to be dropsical.

6. In this case, during the fourth pregnancy, "the menses appeared at the accustomed periods, continuing the usual length of time, and, as far as could be ascertained on a superficial examination, bearing the ordinary characters. She was believed to be suffering from internal disease, for the relief of which, after undergoing a course of treatment, change of air was recommended. In due time, at the return of a catamenial period, she was delivered of a fine healthy child at the full term. This patient has, for several years, been troubled with a leucorrhœal discharge, accompanied with bearing-down and irritable bladder."

7. A woman, 40 years of age and mother of eight children, was admitted into the Manchester Lying-in Hospital, in the eighth month of her ninth pregnancy. She was menstruating at the time of her admission, and said that she had done so regularly every month since the commencement of the process. She was delivered a month afterwards, of a full-grown healthy child, the menstrual discharge having appeared two days previously. Menstruation was also regularly repeated during the whole period of lactation; and the same train of phenomena were stated to have occurred in her last, but not in any of her previous pregnancies. She had for three or four years been troubled with leucorrhœa, lumbar and hypogastric pains, bearing-down, and occasional micturition.

Respecting the nature and cause of the discharges in these cases, the reader will, in some measure, have been prepared by what was stated,

several pages back, on the subject of *spurious menstruation*. The extract, which we now give, will serve to throw additional light upon the views then propounded :—

"A considerable number of cases, similar to the preceding, have occurred to myself, and have been submitted, whenever practicable, to specular examination." P. 222.

"On examination with the speculum, inflammation or ulceration of one or both labia, or of the cervix uteri, complicated, in some instances, with warty excrescences growing from the cervix, or from some part of the vaginal membrane, vaginitis, &c., was met with in every case, without an exception. Fifteen cases were submitted to this kind of examination at the time the blood was flowing. In not one of these did any fluid whatever escape from the interior of the uterus; the orifice being completely occupied at the time by a plug of transparent mucus. On removing the accumulated secretion by means of a piece of lint, the parts were immediately afterwards covered by a coating of blood, which was distinctly seen issuing from innumerable pores on every part of the diseased surfaces, and soon being in sufficient quantity to trickle down into the speculum. This blood was widely different, in its sensible properties, from that collected in the tube during its introduction, or at the os externum; being more florid, more strongly alkalescent, and soon subsiding into a dryish clot, which could be separated from the interior of the instrument in form of a small cake of crassamentum. This was never the case with the former, which remained fluid or soft, for a considerable time.

"The evidence now produced appears sufficient to establish, as a general rule, to which I am not as yet acquainted with an exception, that the blood discharged in cases of alleged menstruation during pregnancy, is furnished, not by the lining membrane of the uterus, nor by any healthy secreting surface—except sometimes perhaps the inferior part of the inner cervix; but by the lower extremity of the uterus external to its cavity, or by the contiguous vaginal reflection, being in a state of suppurative inflammation. The fact is always demonstrable by the aid of the speculum. And where specular investigation is found impracticable, there is still no difficulty in forming a diagnosis, so long as the linen of the patient can be submitted to ocular inspection." P. 223.

In a subsequent passage he maintains—

"1. That *menstruation during pregnancy* is, for the most part, perhaps always associated with an abnormal condition, generally with ulcerative disease of the uterus; requiring, at all times, active remedial interference.

"2. That hæmorrhage during pregnancy is not necessarily associated with an altered relation of the parts within the uterus, and by timely care, need not interfere with the integrity of the ovum.

"3. That menstruation, during the early period of lactation, is not always normal menstruation, but that it is generally associated with morbid conditions which are amply adequate to the satisfactory explanation of the phenomenon; that secondary hæmorrhage is, in the majority of instances, not owing to imperfect contraction, or atony of the uterine fibres; and that the discharge very probably proceeds, under these circumstances, not from the inner surface of the uterus, but from diseased surfaces situated upon parts external to the cavity of the organ." P. 332.

It remains to be seen whether the observations of other obstetrical physicians will warrant the accuracy of these somewhat novel positions.

It is scarcely necessary to say that pregnancy has been repeatedly known to occur before the catamenia had ever made their appearance. Mr. W. relates a good many instances in point. One will suffice; it is instructive.

"A young woman, seventeen years of age, a dressmaker, was brought to me by her mother, in June, 1844. She had been, for several months previously, in a weak state of health, the principal symptoms being languor, nausea, loss of appetite, and swelling of the belly. She was thought to be suffering from retention, never having menstruated. From some hints that escaped in her relation of a previous course of treatment, which had been administered by a female practitioner, and with which circumstance the mother had been hitherto unacquainted, I expressed my suspicions of the existence of pregnancy; this was strenuously denied, however, by the patient. The breasts were considerably developed, although flaccid; the nipples and areolæ dark and well defined. The abdominal tumour was hard, circumscribed, without fluctuation, and situated low in the cavity. The umbilicus was prominent. No sound was elicited by the stethoscope. An active saline aperient was prescribed. The following morning labour pains came on, and in due time, she was delivered of a still-born fœtus, about seven months grown." P. 225

Chap. VI.—*Statistics of Abortion.*

The following table gives the respective periods of abortion in 602 cases, which have occurred in our author's experience. It may be observed that each figure in the first column embraces a period of four weeks, extending from a fortnight before to the same length of time after the month indicated. And, as abortions happening earlier than the seventh week of uterine life are often so nearly simulated by certain menorrhagic discharges, events said to have taken place at this early period—except when an ovum had been clearly made out—have not been included in the report.

Period of pregnancy at which abortion occurred.	Number of births at each period.	Number still-born.	Number living at birth.	Number living at the end of a month after birth.
2 months	35	—	—	—
3 months	275	—	—	—
4 months	147	—	—	—
5 months	30	—	—	—
6 months	32	24	8	0
7 months	55	38	17	3
8 months	28	23	5	1
Total	602	85	30	4

It is astonishing how readily abortion is induced in some women, and how strongly it is resisted in others. As an instance of the latter case, our author tells us of "a poor woman who was admitted in the eighth month of pregnancy into the lying-in hospital, having been recently discharged from the Royal Infirmary, where she had been nine weeks an inmate for fracture of the skull, caused by a blow from a hatchet, which she had received in a quarrel. She was delivered of a healthy child, at the full term of gestation."

With respect to the *causes of abortion*, the experience ofour author is in strict accordance with the observations of Dr. H. Bennet and other

recent writers as to the co-existence of uterine disease in a very great number of cases. This occasionally consists in simple Congestion or vascular plethora of the organ. The symptoms indicative of this state are, "immoderate and painful distension of the abdomen, generally attributed by the patients to accumulation of wind in the bowels; a pulsatile movement extending over the whole cavity, the beats being synchronous with the heart's action; sense of weight and bearing-down; intermittent pains of the loins, like those of labour; and, occasionally, escape of blood from the vagina. There is also distension of the pudic, spermatic, hæmorrhoidal, and all the pelvic veins, and sometimes of those of the lower extremities. On examination, the vagina is found hot and turgid, and the cervix uteri tumid and varicose."

But a very much more frequent cause of abortion is disease of the cervix uteri. This is apparent from the following table of the results of our author's observations in 378 cases.

Accidental agencies.	Placenta prævia.	Constipation of the bowels.	Retroversion of the uterus.	Incurable disease.	Vascular congestion.	Disease of the lower part of the uterus.	Obscure causes.
44	8	3	3	1	15	275	29

He subseqnently remarks, that "of every hundred cases of abortion, not more than thirty could be attributed by the patients themselves to accidental or any other appreciable agency: the rest being vaguely referred to a weak state of health, especially to the condition denominated an 'inward weakness,' to previous abortion, difficult labour, protracted recovery, and, in some instances, to diseases of a specific nature. The two hundred and seventy-five individuals ranged under this head in Table X., were, with a very few exceptions, examined with the speculum, either before, or within three or four weeks after the event took place; and in every case thus submitted to examination, disease of the lower, or of the internal part of the uterus, and in a few instances, of the vagina, was found to exist. Some of those, who had the disease in a severe form and of long standing, underwent the necessary course of treatment at the time, and recovered: but a greater number disappeared after being two or three times prescribed for; unwilling, apparently, to believe that further attention was necessary."

The forms of uterine disease said to predispose to, or to induce abortion, are inflammation and superficial erosion of its mouth and cervix; varicose ulceration of one or of both labia of the *os tincæ;* œdema of the cellular structure of the cervix; fissured ulceration of one or both commissures, of the anterior or posterior labium, or implicating all these parts at the same time, together with inflammatory hypertrophy of the adjacent structures; induration of the cervix, with or without abrasion of surface; endo-uteritis, or inflammation of the lining membrane of the uterus; follicular ulceration; gonorrhœal inflammation of the os and cervix, often

extending into the cavity of the uterus; syphilitic disease, both in its primary, secondary, and tertiary stages; and, lastly, prolapsus of the uterus.

The symptoms denoting a diseased condition of the lower part of the uterus are the presence of a leucorrhœal discharge, whether the matter discharged be simply mucous, or have an admixture of pus or blood; a deep-seated uneasiness in the hypogastrium; a fixed pain in one or in both inguineal regions; aching of the loins; a sense of distressing "bearing-down;" rigors, lassitude, and remittent feverishness. There is often also some uneasiness or other inconvenience in passing water, hæmorrhoids, an acute smarting, or stabbing pain about the coccyx, accompanied with a feeling akin to tenesmus, nausea, loss of appetite, irregularity of bowels, cramps, palpitations, hysteric fits, convulsions, &c. Mr. Whitehead describes with great minuteness the various kinds of leucorrhœa, and points out the different conditions of the uterus and vagina which they serve to indicate. Whenever there is any admixture of puriform matter with the discharge, there is strong reason to suspect some diseased condition of the lower part of the uterus.

So frequent is leucorrhœa in one form or another during pregnancy, that it was found by Mr. W. to be present in 1116 out of 2000 women; and in 936 of the former number, or 83 per cent., the discharge bore undoubted evidence of the presence of pus or of sanies, and occasionally of blood; of these 544, or 58 per cent., had previously miscarried. The following data will be read with interest:—

"Of forty-five women pregnant for the first time, all suffering under leucorrhœal affections, twenty-eight had the discharge of a decidedly purulent character. I examined the uterus in twenty-five of these, and all, with but one exception, had ulcerative disease of the lower part of the uterus; in the exceptional case, the vaginal membrane was studded with warty excrescences which were known to have had a syphilitic origin. I examined several of those also, who declared the discharge to be colourless, and found suppurative disease to be equally prevalent in them. Of more than two thousand individuals labouring under leucorrhœal affections, in whom I have examined the uterus with the speculum, I have, with comparatively few exceptions, found the existence of structural lesion sufficient in degree to account for all the symptomatic phenomena. In the great majority of the cases, the cervix and labia uteri were the seat of disease; in some there was excoriation, erysipelas, high vascular congestion, or a warty state of the vagina; in others, the labia, free from ulceration, were thickened, with their inner margins callous and flabby; or else they were tense, and presenting a vivid redness around the orifice of the uterus; both which appearances indicate the existence of disease within the organ." P. 287.

We may remark, *en passant*, that the puriform leucorrhœal discharge may give rise to blenorrhagia in the husband. The infant too, born under such circumstances, is apt to be affected with purulent ophthalmia, and other affections of a similar nature.

We strongly suspect that a good deal of the description which our author gives of *Endo-uteritis* (the name tells the nature and seat of the disease) is drawn from fancy:—

"The form of disease now proposed for consideration is one of a strictly local character, of every-day occurrence, and very amenable to treatment. It nevertheless acts as a common cause of abortion during the early months of pregnancy; and it constitutes, in the majority of instances, the pathology of that species of disordered or difficult menstruation known as dysmenorrhœa. It does not occur as a necessary

consequence, however, that dysmenorrhœa, as commonly witnessed in early life, indicates the existence of a condition likely to create an inaptitude for child-bearing afterwards; on the contrary, the symptoms in the virgin are often of a purely nervous, or what is understood in common parlance, of an hysterical character, unaccompanied with inflammatory action, and they frequently undergo complete cure by marriage: an instance of which is given in Case XVI., illustrative of the article on this subject.

"Endo-uteritis is a term employed to signify inflammation of the lining membrane of the uterus. The affection sometimes implicates the cavity of the cervix, or that of the lower part of the organ only; at other times the whole lining membrane is involved, and it not unfrequently extends within the Fallopian canals to their outer extremities. The inflammation is generally of a chronic, although of a very irritable character; and under certain states of excitement, as febrile irritation resulting from the application of cold, or of accidental violence; inordinate venereal indulgence; the action of the gonorrhœal or syphilitic virus, &c.; the deeper textures of the uterus and neighbouring organs may be seriously implicated." P. 352.

The symptoms of this morbid condition of the uterus are alleged to be "distension of the hypogastrium, accompanied with a constant, deep-seated aching behind the pubis; irritable bladder; pain of the loins, of the inguinal regions, and of one or both sides of the abdomen on a level with the umbilicus; languor; irritative fever; and vaginal discharge. The whole uterus is often found in a state of inflammatory hypertrophy, and, unlike the affections previously noticed, is extremely painful upon pressure, especially at the *back part of its body*, where it impinges upon the rectum. The cervix is hard and less sensitive, but slight succussion made upon this part develops a painful sensation about the inguinal or umbilical regions, or across the loins. Examined with the speculum, the labia present a tense, glistening appearance, and a ring of vivid redness surrounds the orifice; this, in some cases, is seen to extend upon the surface of the *posterior* lip. Sometimes one or both labia are excoriated, eroded, or fissured."

Accumulation of air within the uterus (*physometra*) is said to frequently accompany inflammation of this organ. "It is commonly discharged in the form of bubbles, which may often be seen to form and burst in rapid succession during specular enquiry. There is reason to believe that it sometimes collects in considerable quantity, causing great suffering by distension of the uterine walls, and being expelled by sudden contraction of the organ, accompanied with severe forcing pains like those of labour. Generally speaking, it emits an offensive odour, owing in most instances, probably, to decomposition of the small coagula liable to be retained after menstruation, or which may also be thrown out at other times. It may possibly be, on some occasions, the product of secretion."

Mr. Whitehead asserts that abortion is, in many cases, owing to inflammation of the lining membrane of the uterus, resulting in an imperfectly organised condition of a portion of the decidual membrane.

The following passage, professing to give a description of *Gonorrhœal Inflammation of the Uterus*, will be found to contain statements which will require confirmation, before they command general assent:—

"Gonorrhœa in the female is much more frequently an affection of the uterus than of the vagina. This, although totally at variance with the opinions hitherto

entertained, is nevertheless what might have been reasonably anticipated. In the first place, the gonorrhœal virus, from physiological causes, is liable to be carried immediately to the highest part of the canal, and forcibly projected upon the lower extremity of the uterus, which organ also, at this juncture, is in a state eminently calculated speedily to absorb it; in the second place, the normal secretion of the vagina possesses properties which are capable, to a certain extent, of destroying, or of materially modifying the virulency of the morbid product, and of thus protecting the vaginal surface from its immediate influence. The urethral orifice, however, seems to be provided with this means of protection in a much less perfect degree, and is, therefore, more highly susceptible of the action of specific inoculation. In nine unimpregnated women afflicted with gonorrhœa, seven had inflammation, with abrasion of the labia uteri, and in the remaining two, the inflammatory action was confined to the vaginal surface; the parts beneath the arch of the pubis being most severely affected: in one of these the urethral orifice was also involved.

"The first change operated upon the uterus after gonorrhœal inoculation, consists in superficial inflammation of one or both labia at their most depending part, or at the boundary of the os and commencement of the internal cervix. The inflammation seems to affect principally the small mucous follicles with which the surface is closely studded—(probably not the Nabothean bodies already noticed.) A small red patch is first perceived; sometimes there are two or three isolated spots which extend and soon run together, forming one patch, of variable size in different cases and in different stages of the complaint, and generally of irregular shape. On removing the thick secretion with which this is covered, the surface appears to consist of minute granules, equally dispersed over every part of it; the abrasion is bounded by a margin not very distinctly defined, running imperceptibly into the erysipelatous redness which surrounds the sore; this extends to some distance upon the cervix, the whole of which is more or less tumid, but not painful to the touch." P. 362.

He subsequently adds:—

"I have seen no small number of cases of this disease, and have almost invariably found the lower part of the uterus inflamed or ulcerated; where the urethra or the adjacent structures have been involved, the affection of these parts has been proved to be of secondary character, resulting from a transfer of the morbid product from the seat of the primary inoculation—the uterus." P. 365.

Gonorrhœal inflammation is said to be extremely liable to extend within the cavity of the womb, and give rise to chronic Endo-uteritis; a distressing affection, which may exist for years, "rendering the whole of each succeeding pregnancy a period of suffering and misery, in many instances occasioning abortion at different stages of the process; sometimes it effectually prevents the accomplishment of impregnation, and it is not an uncommon cause of dysmenorrhœa. The symptoms are always aggravated during pregnancy; the inflammation involving the deeper textures of the cervix, and resulting in interstitial deposit, and consequent induration."

Our author seems to be of opinion that a very large proportion of the cases of ulceration of the uterine mouth and neck are of gonorrhœal origin. We need scarcely say that, in his opinion, the application of the nitrate of silver and other astringents must be made directly to the os and cervix uteri in the treatment of gonorrhœal inflammation. He says that the patient herself may be taught how to apply any fluid remedy, with the aid of a speculum. Injections he declares to be of little use.

Passing over the subject of Syphilitic disease of the uterine orifice, we shall briefly notice one or two points in our author's description of *Prolapsus uteri*. He maintains that this disease of the organ, like abortion, is more frequently owing to inflammation and ulceration of the lower part of the organ than to any other cause. With respect to the treatment of this very distressing affection, Mr. Whitehead expresses his strong objection to the use of pessaries in the generality of cases; nay, he goes so far as to assert that "they invariably aggravate the disease for the relief of which they are employed." After pointing out the necessity of attending to the general or constitutional health of the patient, he remarks:—

"The local treatment should consist in applications of nitrate of silver, or other suitable remedies to the diseased surface, and in the insertion of medicated tents by the aid of the *Prolapsus tube*. This latter procedure may be practised immediately after the nitrate has been applied, although the remedy with which the tent is charged be of a very different nature from that of the caustic.

"The manner of using the *Prolapsus tube*—which will be found of equal service, in the management of prolapsed displacement of the uterus, as in most other forms of uterine disease, and enables the patient safely and efficiently to apply the remedies herself, without the interference of the practitioner—is extremely simple. The charged tent, to which a length of thread has been previously attached in the manner before directed, must be placed in the tube, the upper orifice of which is to be applied against the protruded portion of the uterus, in such a manner as to receive the os uteri within it. The instrument, previously smeared with some unctuous material, and having its curved arm placed anteriorly, in a direction towards the abdomen, is now to be forced gently and steadily backwards, until the whole, or greater portion of it, has passed within the canal, or until a moderate degree of resistance is felt to oppose its further ingress." P. 394.

"The uterus being thus restored to its natural position, the tent or pledget must be pressed upwards against the cervix, and held in that situation by means of a skewer, or other suitable instrument, the tube at the same time being gently withdrawn. The recumbent posture should be strictly maintained for several days, and very little exercise taken for some weeks afterwards. The cases recorded in the accompanying table were those of poor women, obliged, during treatment, to pursue their domestic duties, which constantly and seriously interfered with the favourable effects of the remedies; hence the reason, probably, why most of them were so long under treatment. The application should be renewed daily, or twice a day if convenient." P. 395.

The lotions, used for moistening the lint-tent, are strong solutions of nitrate of silver, sulphate of zinc, sulphate of copper, matico, opium, and tannin. The metallic proportions should not be employed oftener than every third or fourth day, the vegetable applications being used intermediately. An emollient injection should be made use of after the removal of each tent.

We shall now briefly advert to the contents of Mr. Whitehead's last chapter, that which professes to treat of Sterility. His remarks upon the fatal influence of abnormal states of the vaginal mucus on the vitality of the spermatic animalcules need not detain us; most of them are mere conjectures; not his, by-the-bye, but those of M. Donné whom he quotes. As to the morbid conditions of the uterus, which he enumerates among the causes of Sterility, endo-uteritis—*endo-metritis* would be a better word—and ulcerative inflammation of the cervix are declared to be the most

frequent. It will be our best plan briefly to analyse a few of the cases adduced by Mr. W. in illustration of his views :—

1. A woman, aged 41, was admitted into the Manchester Lying-in Hospital, eight months advanced in her second pregnancy. She had been married when 18 years of age, and borne her first child a year and a half subsequently. After this period, she was seldom free from leucorrhœa, which was often of an acrimonious character, during the menstrual intervals. The catamenia were tolerably regular, but always attended with great suffering. Her husband survived sixteen years; and, a twelvemonth after his death, she married a second time, her health having somewhat improved in the interim. "Two years and three quarters after marriage she experienced an attack of fever, for which she was an in-patient of the Manchester Fever Hospital, after which she enjoyed excellent health: 'the fever having set her up.' All symptoms of uterine disturbance disappeared on the accession of the febrile affection, and did not afterwards return. Pregnancy took place eight or ten weeks after convalescence, and she was delivered in due time of a living child at the full term of utero-gestation. The uterus, which was examined previous to her dismissal from the Lying-in Hospital, exhibited traces of former disease, but was perfectly healthy."

We may pass over the next case, as there was unfortunately no "cure" effected. The one that follows is headed thus: *Sterility fifteen years; secondary syphilitic affection of the uterus of sixteen years' duration; sanious leucorrhœa; painful menstruation; pregnancy; cure.* By-the-bye, is not the cart put before the horse here?—was not the cure before the pregnancy? The circumstances of the case are these. A young woman was infected with syphilis by her husband soon after marriage; she was never pregnant by him. He died twelve years afterwards; and then, after a twelvemonth's widowhood, "being then in a better state of health," she married a second time. Her general health became much improved, and, as might be expected, the leucorrhœal discharge, with which she had been affected for many years, was much diminished. Two years subsequently, she became pregnant for the first time. As she was threatened with abortion soon after quickening, she consulted Mr. Whitehead. He found, on specular examination, the following appearances :—

"The cervix uteri was unusually large and firm; the boundary of the orifice was covered with granulations which appeared to extend to the interior of the organ: the outer boundary of this ulcer was marked by a raised margin, beyond which the surface of the cervix was of a dark-red colour and mottled; a fissure occupied the right commissure, from which and from the adjoining granulations blood was exuding. On communicating my suspicions respecting the syphilitic nature of the complaint under which she was labouring, her history as above given was related in a straightforward manner; but she was unwilling to believe that any trace of the venereal affection remained in her constitution, so that no treatment was adopted having reference to that particular purpose. Her delivery took place on the 11th of July, 1846, at the full period of utero-gestation." P. 411.

The child subsequently exhibited indubitable traces of venereal taint.

The following case is certainly more satisfactory than either of the preceding :—

A lady, married at the age of 21, became affected a few weeks after marriage with purulent leucorrhœa, which was suspected to be of gonorrhœal origin and treated accordingly. After the active symptoms were subdued, the discharge continued, more or less aggravated from time to time by exposure to cold, or after severe exercise; it was, also, always augmented a few days previous to each return of the catamenia. Menstruation was attended with greater suffering than formerly; the secretion was more abundant, continued several days longer than it was wont to do, being sometimes clotted, or grumous and offensive, and occasionally, towards the end of the period, mixed with membranous substances.

This unpleasant state of things had continued, with varying degrees of severity, for six years: she had never been pregnant. The general health was much impaired. The local malady is thus described:—"The whole uterus was found in a state of inflammatory hypertrophy, painful under pressure; the cervix was slightly indurated; and an angry-looking granulating ulcer of irregular shape occupied the lower and inner parts of both labia, and appeared to extend within the orifice of the uterus, whence flowed a thin ichorous secretion."

Although the ulcer is here described as *angry-looking*, we subsequently read that the speculum was used for the first time about a twelvemonth afterwards. How is this discrepancy to be accounted for? Under the use of alterative and tonic medicine, with the addition of astringent injections, the leucorrhœal discharge became considerably diminished, and the general health was much improved. Six months later, she miscarried in the third month of pregnancy: this was in March. In the following November she again miscarried, "at the end of the 5th month." The rest of the report we shall give in our author's own words:—

"She now expressed an anxiety about her case, and begged that the necessary means might be adopted for its efficient cure. The leucorrhœal affection was said to have become considerably aggravated previous to miscarriage, and appeared to be in a similar state a fortnight afterwards when the treatment was commenced. On the introduction of the speculum, which was now used for the first time, the cervix appeared unusually large, hard, exhibiting several patches of excoriation: two deep ulcerated fissures occupied, one the left commissure, the other the middle of the posterior labium; the orifice was surrounded by a bright red, inflamed surface, and the whole was covered with a sanio-purulent secretion, which became considerably mixed with blood during examination. The nitrate of silver was freely applied to all the diseased surfaces, as well as within the cervix, and was afterwards several times repeated at intervals of six or seven days. She also resumed the use of iodide of iron combined with extract of cinchona. At the end of eleven weeks, the cure was complete; every part of the uterus was perfectly healed and of normal dimensions and consistence; the mind was cheerful and happy, the health vigorous; and, for the first time since marriage, she was free from leucorrhœa.

"She was delivered of a living child at the full period of utero-gestation, on the 5th December, 1846." P. 418.

In the case that is next reported there was obstinate leucorrhœa of many years standing, and there was presumed to be "ulceration of the cervix uteri and probably endo-uteritis;" the speculum was not used. A strong solution of the nitrate of silver—3 j. to ℥ vj. of water—was ordered to be used as an injection, directions being given that, in using it, the syringe

should be introduced as high up as possible. After being employed five times, severe inflammation of the vagina came on; but this, in the course of a few days, subsided, and the patient improved rapidly. She afterwards spent a few weeks in the country, and returned in better health than she had experienced since marriage. The so-called mistake had completely cured the leucorrhœal affection, and consequently the uterine disease also: she shortly became pregnant, and was delivered of a living child at the full term of utero-gestation, eleven months after the injection had been used.

In the case that follows this one, the symptoms of chronic endo-uteritis were mistaken, our author says, by several practitioners for those of "spinal irritation;" in consequence of there being, along with a fixed uneasiness in the left groin and an almost constant leucorrhœal discharge, "a violent pain, extending from the third dorsal vertebra to the sacrum." Upwards of four years had passed since the lady had miscarried about six months after marriage; she had never been pregnant again. "The lower part of the uterus was in a state of inflammatory hypertrophy; the labia were thickened and projecting, and presented the ring of vivid redness around the orifice indicative of internal inflammation. To these parts the solid nitrate of silver was freely applied; leeches were applied alternately to the sacral and hypogastric regions, and remedies of an alterative character were administered. At the end of three months, her condition being remarkably improved, although the leucorrhœal discharge was still present, she became pregnant." It seems, however, that, as the pregnancy advanced, all her former distress returned. "Soon after quickening, attacks of intermittent pains similar to those of labour came on, the discharge was mixed with blood, and my attendance was solicited for the purpose of facilitating delivery, which was considered as being near at hand. The cervix uteri was large, inflamed, and excoriated, and a granulating surface occupied the boundaries of the orifice. To all these parts the nitrate of silver was again freely applied; two grains of opium combined with an equal quantity of *hydr. subm.* were given, and the strictest quietude enjoined. On the following morning she was free from pain: she had scarcely slept at all, but had been perfectly tranquil the whole night. Anodyne remedies and applications of caustic, with due attention to the alvine functions, were found occasionally necessary during the remainder of the process, which terminated in the birth of a living child of full growth on the 5th of April, 1846."

Subsequently, there was a renewal of the symptoms that had so long affected the health. Believing that it was the internal surface of the uterus that was the primary and chief source of the mischief, Mr. Whitehead ordered a weak solution of the nitrate of silver to be forcibly injected into its cavity. It caused no pain or inconvenience, and its use was followed by the most gratifying results. The operation was repeated ten days afterwards. "The first injection was introduced on the 24th September, since which, now more than eight months, she has not had the least symptom of her former or of any other ailment, nor has she since taken a single dose of medicine. She has the appearance of one in robust health."

These cases will doubtless suggest some useful hints to the reflecting practitioner, and the successful treatment in two or three of them is highly

creditable to the skill of our author. He cannot, indeed, claim any originality as to the discovery of the connexion that so often exists between ulcerative inflammation of the lower segment of the uterus and Sterility; for on this point, as indeed on most others treated of in the volume before us, he has been quite anticipated by Dr. Henry Bennett, to whom unquestionably belongs the almost exclusive merit of having, within the last few years, drawn the attention of the profession—in this country at least—to the important part which this morbid state plays in the production of some of the most troublesome complaints to which women are subject, both in the pregnant and the unimpregnated condition. Mr. Whitehead seems, rather strangely, to be but very imperfectly acquainted with the results of his predecessor's researches. The followiug propositions, with which Dr. Bennett concluded a series of very interesting and valuable papers which appeared in the *Lancet* during the course of last year, may be appropriately introduced here. Their general accuracy has been strikingly confirmed by the subsequent enquiries of our author as well as of other observers.

"1. Inflammatory ulceration of the uterine neck is not an uncommon disease in the gravid uterus, although hitherto entirely overlooked by uterine pathologists and by accoucheurs.

"2. When this disease exists in the pregnant state, its symptoms are the same as in the non-pregnant condition, but obscured, and more or less modified, by the pregnancy.

"3. It is a frequent cause of disordered health during pregnancy, or of 'laborious pregnancy.' It is also one of the most frequent causes of Abortion, both in the early and in the later months of pregnancy. It may occasion abortion, either directly, by reflex action, or indirectly, by giving rise to disease of the ovum or placenta, or to uterine hæmorrhage.

"4. The instrumental examination of females labouring under inflammatory ulceration of the cervix during pregnancy is unattended with any risk, either to the mother or to the fœtus; and it is absolutely necessary, in order not only fully to recognise the disease, but also to treat it.

"5. The treatment of these forms of uterine inflammation must be governed by nearly the same rules in the pregnant state as in the non-pregnant state. A properly conducted treatment is nearly always successful in preserving the life of the child and the integrity of the pregnancy, as well as in curing the inflammatory and ulcerative disease. It is also the only means we possess of warding off the imminent danger of abortion to which the patient is exposed.

"6. This form of uterine inflammation being, generally speaking, the cause of those repeated and successive miscarriages which prevent females giving birth to a living child, it is only by curing it that we can hope to make them bear the product of conception to its full period.

"7. The serious inflammatory and hæmorrhagic symptoms which sometimes follow abortions are generally occasioned by unrecognised inflammatory ulceration of the uterine neck. On investigation, it often becomes evident that this disease existed previous to the abortion, and occasioned it. The same remark may apply to some cases in which the above-mentioned symptoms precede and follow labour at the full time, as ulcerative inflammation of the cervix in the pregnant state is by no means *necessarily* followed by abortion.

"8. Although inflammatory ulceration of the cervix seems generally to be a cause of Sterility, yet, as will appear from the above essay, there are frequent exceptions to the rule. In some females, the tendency to become impregnated is so great that no amount of uterine disease appears to prevent conception taking place."

In taking leave of our author we must again enter our *caveat* against all uncalled-for or unadvised use of the speculum in the complaints of females. Though most valuable in many cases, and absolutely necessary in others, it need scarcely be said that the employment of this means of exploration *may* become the source of no trifling mischief, in more ways than one. We cannot but think that Mr. Whitehead has set a very questionable example to his younger brethren in the profession, by his frequent adoption of it in that condition of the female system, when the instinctive feelings of Nature shrink from exposure; nor can we think very highly of an author's feelings of decorum, who could deem it at all necessary or proper to proclaim to the public his belief "that the danger of copulation during the period of menstruation to be in a great measure assumed and fanciful, or, at all events, to be far less imminent than has been represented."

If Mr. Whitehead's volume reaches a second edition—which we think not unlikely, considering the value of a great part of its contents—it must undergo a very thorough revision, in the way of retrenchment, compression, and collation with what has been done by other writers in the same department of pathological enquiry. We would particularly direct his attention to a very valuable paper from the pen of Dr. Evory Kennedy, in our excellent cotemporary, the Dublin Medical Journal, for last February.

I. Researches on the Chemistry of Food. By *Justus Liebig*, M.D. Edited from the Manuscript of the Author. By *William Gregory*, M.D. 8vo. pp. 156. Taylor and Walton, 1847.

II. The Chemistry of Vegetable and Animal Physiology. By Dr. *G. J. Mulder*. Translated from the Dutch by Dr. *P. F. H. Fromberg*, with an Introduction and Notes. By *James F. W. Johnston*, F.R.S. L. & E. Part III. 8vo. pp. 267. Eight coloured Lithographs. Blackwood, Edinburgh, 1847.

Although these recent works, or rather portions of works, of the two celebrated rival chemists, deal too much in special details too admit of lengthened notice, valuable as these may prove for the purposes of reference, at least when some of the statements they contain have been corroborated by farther investigation, they also refer to matters of a more general character which may advantageously be adverted to here. While there is such discrepancy in important statements made as to the results of minute analyses by men who should be incontrovertible authorities upon subjects to which the whole energies of their lives have been devoted, we may be well excused detailing that portion of their labours, in the hope that renewed investigation, patiently and charitably conducted, may reconcile differences.

Professor Liebig commences his Essay with an introductory chapter upon the "*Methods of Investigation in Animal Chemistry;*" in which he endeavours to demonstrate that, if the active spirit of chemical research

which has now been going on for the last ten years is so barren in actual and assured results, this has arisen principally from the faulty mode in which the investigations have been carried on, in consequence of which erroneous, insufficient and premature conclusions have been arrived at. As an example of these, Mulder's Proteine Theory is put prominently forward, in a tone of assumed superiority, not altogether becoming one who once acknowledged its validity, or very well calculated to heal breaches of good understanding already made.

One source of error arising from the isolation of there searches of the chemist from those of the physiologist and pathologist, is thus touched upon:

"In modern times this method (hypothetical systems of medicine) has been abandoned as entirely unproductive; but, on the other hand, men commit an error not less grave, inasmuch as, instead of acquiring by their own researches the knowledge necessary for the solution of their difficulties, they leave this duty to others, who, fully occupied with the cultivation of their own branch of science, have neither interest in the questions to be solved, nor inclination for the task. From the chemical analysis of blood, of urine, or of a morbid product, they expect an aid which these analyses can never afford, as long as the results of the chemist are not brought into the true connection with the conditions which they are to explain, or with the causes which have produced these conditions. All the new facts daily ascertained by the chemist are regarded by pathologists as being exactly those which are of no direct use to them, because they have no clear idea of that which they require; because they are unable to connect with these chemical discoveries any question to be solved, or to draw from them any conclusion.

"What an inconceivable delusion, what a confusion of ideas must exist, when a physician thinks, that from the complex results of an analysis of the blood, he can draw a conclusion as to the nature and the cause of a disease, and can found on this a method of treatment, when we have not yet advanced so far in physiology as to bring into relation with the digestive process one of the simplest chemical facts, namely, the absence of alkaline phosphates in the urine of the herbivora! What pathologist has ever yet attempted to fix and define the notion of bad or spoiled food, in its full signification, by means of a logical comparison with good and wholesome food? and yet the former are regarded as the proximate causes of diseased conditions. I really admit, that for such an investigation chemical knowledge is indispensable; but the investigation itself has no value in reference to chemistry, and constitutes no object of research for the chemist, as such.

"From this state of things, which depends on the want of connection between the labours of chemists and those of physiologists, it has happened, that Animal Chemistry, during the last ten years, has gained little more than a more accurate knowledge of those compounds which the animal organism applies to no further purpose in its economy; and that, at the present time, it seems as if all the wonderful properties which it exhibits were produced only by means of albumen, fibrine, gelatine, some cerebral or nervous matter, and a little bile. It is universally felt, that we are as far from a true animal chemistry as the anatomy of the last century was from the physiology of the present day. Indeed the animal chemistry of our time cannot be compared to modern anatomy, since microscopic researches have established the existence of structures which had entirely escaped the earlier investigators; of structures, as is now known, on which alone the function of those formerly observed depends." P. 5.

The knowledge derived from *ultimate Analysis* which has of late been so industriously pursued, is here shown to have been more apparent than real—the formulæ thus accumulated too often proving data of little worth.

"About ten years since, the ultimate analysis of organic bodies furnished physiology with a result highly important, in order to the easy understanding of the digestive or nutritive process, by demonstrating, that fibrine, albumen, and caseine have the same composition. Misled by this result, many chemists thought that the chief problem to be solved by chemistry was to ascertain, by ultimate analysis, the composition, in 100 parts, of all the constituents of the body; and thus many were induced to act on each of these constituents, without a more minute study of its chemical relations and its properties, with alcohol, ether, and acids; and with the aid of the known resources of organic analysis, to determine the percentage of carbon, nitrogen, and oxygen. They believed that they had thus, by means of these numerical results, done a real service to physiology, although the only addition thus made to the name of the substance analysed was an empty formula, of the accuracy of which there was no evidence whatever. Now that we have been for ten years in possession of these formulæ, every one must perceive that we have made no real progress. The cause of this is obvious to all who know the true value of ultimate analysis. Ultimate analysis is a means of acquiring knowledge, but is not itself that knowledge. Even supposing, what no one will seriously maintain with regard to the constituents of the animal body, that analysis had made us acquainted with the exact proportions in which their elements are united together, yet this knowledge gives us not the least information as to the arrangement of these elements, or the way in which they group themselves, under the influence of chemical agencies. Now it is the knowledge of both these things together which alone can lead us to definite views as to the part which these compounds play in the vital processes, or the changes to which they are subjected up to the period of their expulsion from the body; and this is essentially the problem which Chemistry has to solve in reference to the vital progess.

"Ultimate analysis, by itself, has this peculiarity, that in the case of very complex substances it cannot secure the chemist against errors, because there is no other control for the accuracy of the analysis than the analysis itself; and because the errors are equal at different times, and escape notice when we cannot change the methods of determining the individual elements. Now there is as yet no means of determining the weight of carbon otherwise than in the form of carbonic acid, or that of hydrogen otherwise than in the form of water.

"The only way to attain an accurate expression for the composition of those substances, which, like the constituents of the animal body, contain a very large number of elementary molecules in the complex atom of the compound, is to endeavour to resolve it into two or more less complex compounds, and to compare the composition and the amount of these products with those of the body from which they have been derived." P. 12.

The main part of Liebig's work is occupied in detailing his analytical investigation of the *Constituents of the Juices of Flesh*. After alluding to the crude views formerly entertained respecting the presence of Lactic Acid, he dwells at some length upon the properties of *Kreatine* discovered by Chevreul in 1835 to be a peculiar crystallizable body obtainable by boiling flesh with water. Since then other chemists have obtained it in small quantities, while others failing to do this have considered its presence accidental. Liebig details the process by which it may be obtained, providing largish quantities are operated upon. From 8 to 10 lbs. of flesh must be employed, and even from this, after the coagulation of the albumen and colouring matter, soluble matters to the amount of 4 ozs. only will be obtained for investigation, and that but by the use of a good press. The fluid obtained possesses an acid re-action, and even in those animals in which it has only been obtained in conjunction with blood, the alkali of this does not neutralize the free acid of the flesh. "Indeed, I believe,

that in most animals, if we suppose the whole mass of blood in the vessels to be mixed with the whole fluid of the muscles, the mixture would retain, not a neutral or alkaline, but an acid re-action." The free acid is removed by means of a solution of baryta, giving rise to precipitates of the phosphate of baryta and of magnesia. The filtered fluid is then gently evaporated until the crystals of Kreatine are deposited. The amount of this substance produced varies not only in the flesh of different kinds of animals, but even in animals of the same class—the quantity being always much less in fat flesh than in lean flesh. 1000 parts of the flesh of lean fowls yielded 3·05 of Kreatine: 1000 of the horse 0.72; 1000 of the ox 0·697. Obtained and purified Kreatine is neither acid nor basic. When Kreatine is dissolved in heated strong hydrochloric, sulphuric, phosphoric, or nitric acid, and the solution evaporated, crystals are obtained which are very soluble in alcohol, which those of Kreatine are not. By this contact with the strong mineral acids a portion of these enters into combination with the Kreatine, and a new body, of totally different chemical properties, a true organic alkali, and termed by Liebig *Kreatinine* is produced. This is far more soluble than Kreatine; and in its chemical character is quite analogous to ammonia.

The compound discovered in the urine three years since by Pettenkoffer contains the same proportions of carbon and nitrogen as do Kreatine and Kreatinine; and a repetition of Pettenkoffer's analysis by Liebig shows this substance to be a mixture of Kreatine with a little Kreatinine: and although neither of these bodies exists in great amount in the urine, yet may they be more conveniently procured from it than from flesh.

After describing the Salts of Kreatinine, Liebig gives an account of another organic base termed by him *Sarcosine*, produced by adding to a boiling saturated solution of Kreatine ten times the weight of crystallised hydrate of baryta. Ammonia is disengaged and carbonate of baryta precipitated, while a colourless fluid containing Sarcosine and caustic baryta is produced.

"If from the elements of crystallised kreatine we subtract those of sarcosine there remains a formula exactly identical with that of urea.

From 1 eq. Kreatine	$= C_8\ N_3\ H_{11}\ O_6$
Deduct 1 eq. Sarcosine	$= C_6\ N_4\ H_7\ O_4$
There remains 1 eq. Urea	$= C_2\ N_2\ H_4\ O_2$

"It is consequently obvious that, in the decomposition of kreatine by baryta, carbonic acid and ammonia are secondary products derived from the decomposition of urea. I have ascertained that a solution of urea in barytic water is resolved by long boiling into carbonate of baryta and ammonia with the same appearances as those above described; and I have also ascertained that urea is present in the liquid when kreatine is boiled with baryta, if examined before the whole of the kreatine is decomposed." P. 75.

By the agency of alcohol upon the liquid from flesh, whence all the Kreatine has been deposited, crystals of an acid, termed by Liebig *Inosinic*, may be procured. After describing this and the compounds it forms with baryta, potash, &c., and detailing a simple process by which Kreatinine

may be extracted from muscle, the author next describes *Lactic Acid* as forming one of the constituents of flesh, and passes on to the consideration of the *Inorganic Constituents of the Juices of Flesh*, which he treats of at very considerable length. Into his ingenious speculations upon the important agency of the saline constituents, the alkaline phosphates, of the juice of flesh, and upon the function of phosphate of soda in the blood—that of absorbing carbonic acid and giving it out in the lungs—our limits forbid our following him. An extract or two will exhibit the interesting character of the subjects adverted to. After stating that *Lactic Acid* cannot be detected in the urine even of those to whom it has been administered, he goes on to observe :—

"From this it plainly appears, that the lactic acid in the organism is employed to support the respiratory process, and the function performed by sugar, starch, and in general all those substances which, in contact with animal matter, are convertible into lactic acid, ceases to be an hypothesis. These substances are converted in the blood into lactates, which are destroyed as fast as they are produced, and which only accumulate where the supply of oxygen is less, or where some other attraction is opposed to the agency of that element. When we consider that the urine of graminivorous animals contains a large quantity of free alkali, which is secreted from the blood; that, consequently, in the blood a current of dissolved alkalies is carried through the whole mass of the body, and especially through the substance of the muscles, while the fluid which is in contact with the external part of the blood-vessels and lymphatics (the juice of flesh) retains an acid reaction, we perceive that a cause must necessarily be in action at these points, which prevents the removal of the free acids, or, if they are removed, reproduces them at each moment of time.

"The blood-vessels and lymphatics contain an alkaline fluid, while the surrounding fluid, that of the flesh, is acid; the tissue of which the vessels are composed is permeable for the one or the other of these fluids. Here then are two conditions favourable to the production of an electrical current, and it is far from improbable, that such a current takes a certain share in the vital processes, although its action be not always indicated by proper electrical effects." P. 104.

"From the great difference of chemical nature and qualities in the fluids circulating in the different parts of the organism, it follows, that there must be a very remarkable difference in the permeability of the parietes of the vessels for these fluids. Were this permeability in all cases the same, there must have been found as much of the salts of soda and potash in the juice of flesh as in the blood; but the blood of the ox and fowl contains nearly a third of its whole saline contents of chloride of sodium, while hardly a trace of this compound occurs in the juice of flesh.

"The vessels which secrete milk must stand in a similar relation to the blood-vessels; for in the milk of the cow, the salts of potash predonderate very greatly over those of soda, and are present also in much larger quantity than in the saline constituents of blood.

"In some pathological conditions there has been observed, at points where bones and muscles meet, an accumulation of free lactic and phosphoric acids, which has never been perceived at those points in the normal state. The solution and removal of the phosphate of lime, and therefore the disappearance of the bones, is a consequence of this state. It is not improbable that the cause, or one of the causes, of this separation of acid from the substance of the muscle, is this—that the vessels, which contain the fluid of the muscles, have undergone a change, whereby they lose the property of retaining within them the acid fluid they contain.

"The constant occurrence of chloride of sodium and phosphate of soda in the blood, and that of phospate of potash and chloride of potassium in the juice of flesh, justifies the assumption that both facts are altogether indispensable for the processes carried on in the blood and in the fluid of the muscles.

"Proceeding on this assumption, the necessity for adding common salt to the food of many animals is easily explained, as well as the share which that salt takes in the formation of blood, and in the respiratory process." P. 109.

Practical Applications.—In this section Professor Liebig furnishes some useful hints upon the modes in which animal substances can be most advantageously prepared for human sustenance. From raw flesh intended to reproduce itself in the animal frame, none of its constituents should be withdrawn during the process of cooking. "The water in which flesh has been boiled contains soluble alkaline phosphates, lactates, and isoninates, phosphate of magnesia, and only traces of phosphate of lime; the boiled flesh contains chiefly, with the fibrine, &c., the insoluble inorganic constituents, phosphate of lime and phosphate of magnesia." If therefore, the flesh be eaten without the broth, it becomes the less adapted for nutrition, and that in proportion to the quantity of water employed, and the duration of the boiling. "It is clear that all the sapid and odorous constituents of flesh exist in the flesh itself in the soluble state, and consequently when it is boiled are transferred to the soup. The smell and taste of roasted flesh arise from the soluble constituents of the juice, which have undergone a slight change under the influence of the higher temperature. Flesh, which has been rendered quite tasteless by boiling with water, acquires the taste and all the peculiarities of roasted flesh, when it is moistened and warmed with a cold aqueous infusion of raw flesh, which has been evaporated until it has acquired a dark brown colour." It is upon those soluble constituents the peculiar flavour of each kind of flesh depends, so that if to boiled beef a concentrated cold aqueous infusion of venison or fowl be added, and the mass warmed, the beef can no longer be distinguished from venison or fowl. "A slight addition of lactic acid (a very little sauer kraut, for example), or of chloride of potassium, which is an invariable constituent of all infusions of flesh, heightens the piquancy of the flavor of the meat; as, on the other hand, an alkaline liquid, or the addition of blood, renders the soup of meat utterly insipid and mawkish." By the agency of cold water, also, all the *albumen* which everywhere surrounds the muscular fibre in a liquid form may be obtained; the quantity obtainable bearing an inverse ratio to the age of the animal. Old animals are, however, richest in fibrine, which, when deprived of its albumen, becomes hard and horny in proportion as it is boiled. The tenderness of cooked meat, therefore, depends upon the amount of albumen coagulating between the fibres, and thus preventing the hardening of these. The duration of the boiling is another point to be attended to; for when this is too prolonged the albumen becomes harder, though not tough.

The following are the directions given for the best method of *boiling meat*:—

"If the flesh intended to be eaten be introduced into the boiler when the water is in a state of brisk ebullition, and if the boiling be kept up for some minutes, then so much cold water added as to reduce the temperature of the water to 165° or 158°, and the whole kept at this temperature for some hours, all the conditions are united, which give to the flesh the quality best adapted to its use as food.

"When it is introduced into the boiling water, the albumen immediately coagulates from the surface inwards, and in this state forms a crust or shell, which no longer permits the external water to penetrate into the interior of the mass of flesh. But the temperature is gradually transmitted to the interior, and there effects the conversion of the raw flesh into the state of boiled or roasted meat. The flesh retains its juiciness, and is quite as agreeable to the taste as it can be made by roasting; for the chief part of the sapid constituents of the mass is retained under these circumstances, in the flesh.

"If we reflect that the albumen of the juice of flesh begins to coagulate at a temperature of 105·5° and that it is completely coagulated at 140° (Berzelius), it might be supposed that it would not be necessary, in the cooking of flesh, to expose it to a higher temperature than 140°. But, at that temperature, the colouring matter of the blood is not yet coagulated; the flesh, indeed, is eatable, but when it contains blood, it acquires, under these circumstances, a bloody appearance, which it only loses, when it has acquired, throughout the whole mass, a temperature of 150° to 158°.

* * * * * * *

"The introduction of the piece of raw flesh into water already boiling is the best process for the dressing of the meat, but the most unfavourable for the quality of the soup. If, on the contrary, the piece of raw meat be placed in cold water, and this brought *very gradually* to the boiling point, there occurs, from the first moment, an interchange between the juice of the flesh and the external water. The soluble and sapid constituents of the flesh are dissolved in the water, and the water penetrates into the interior of the mass, which it extracts more or less completely. The flesh loses, while the soup gains in sapid matters; and by the separation of albumen, which is commonly removed by skimming, as it rises to the surface of the water when coagulated, the surface of the meat more particularly loses its tenderness and shortness (as it is called), becoming tough and hard. The thinner the piece of flesh, the more completely does it acquire the last-mentioned qualities; and if in this state it be eaten without the soup, it not only loses much of its nutritive properties, but also of its digestibility, inasmuch as the juice of the flesh itself, the constituents of which are now found in the soup, are thus prevented from taking part in the digestive process in the stomach. The soup, in fact, contains two of the chief constituents of the gastric juice." P. 128.

It has long been supposed that the chief properties of soup depended upon the amount of gelatinous matter dissolved during boiling; but, in fact, gelatine is tasteless, and exists in a very small quantity in properly prepared soup. The nutritious and sapid ingredients exist ready formed in the flesh, and are in no-wise the products of the boiling. They may be most promptly extracted by following this formula for the preparation of soup :—

"When 1 lb. of lean beef, free of fat, and separated from the bones, in the finely-chopped state in which it is used for beef-sausages or mince-meat, is uniformly mixed with its own weight of cold water, slowly heated to boiling, and the liquid, after boiling briskly for a minute or two, is strained through a towel from the coagulated albumen and the fibrine, now become hard and horny, we obtain an equal weight of the most aromatic soup, of such strength as cannot be obtained, even by boiling for hours, from a piece of flesh. When mixed with salt and the other usual additions by which soup is usually seasoned, and tinged somewhat darker by means of roasted onions or burnt sugar, it forms the very best soup which can in any way be prepared from 1 lb. of flesh." P. 132.

By concentration and slow evaporation a valuable extract of flesh

may be prepared, half-an-ounce of which converts 1 lb. of water into a strong well-flavoured soup. This portable soup (a very different one from that ordinarily so called which consists of gelatine), although too dear to form an article of commerce, is well worthy the attention of governments, as a restorative for wounded soldiers, and in the provisioning of ships and fortresses, as a means of preventing or relieving disease arising from a dearth of fresh meat and vegetables.

The effects of *salting meat* have been carefully investigated by Liebig, and he has ascertained that the brine in contact with the meat contains the constituents of a concentrated soup or infusion of meat; so that by this process the composition of meat becomes much more changed than by boiling. In the latter, "the highly nutritious albumen remains in the coagulated state in the mass of flesh, but, in salting, the albumen is separated from the flesh; for when the brine from salt meat is heated to boiling, a large quantity of albumen separates as a coagulum. It contains likewise, lactic acid, the phosphates, kreatine and kreatinine; and, in proportion as these are abstracted from the meat does it lose its nutritive properties. A portion of such flesh is converted into an element of respiration; and health cannot be maintained upon salt meat, unless a much larger quantity be given, "inasmuch as it cannot perfectly replace, by the substances it contains, those parts of the body which have been expelled in consequence of the change of matter, nor can it preserve in its normal state the fluid distributed in evey part of the body, namely, the juices of flesh."

We conclude our notice of Professor Liebig's interesting work with the following extract :—

"If we consider that the juice of flesh, in all animals yet examined, possesses a constant character; that, exclusive of those constituents which are derived from the blood unavoidably mixed with it, as well as of small quantities of odorous and sapid substances on which the characteristic secondary or by-taste of the juice or soup of the flesh in each kind of animal depends, the juice of ox-flesh is in no way distinguishable from that of the fox, it seems justifiable to conclude, that the quantity and the nature of the soluble constituents in the muscular system are essential to the functions of the muscles. It appears further to follow, that in judging of the nutritive qualities of any kind of food, the composition of the blood cannot be selected as the proper datum from which to argue, because there are a number of factors which must be brought into the calculation, and which are either wanting in the blood, or present in it only in trifling quantity.

"Some experiments have lately been made by Lehmann on the gastric juice of dogs, fed on bones and lean horse-flesh, which fluid he has studied more minutely than had previously been done. He obtained from it a crystallized salt of magnesia, combined with an organic acid, not containing nitrogen. This salt yielded 16.6 p. c. of magnesia, and 21 p. c. of water of crystallization. Now that we know that lactic acid forms a constituent of the chief mass of the body, it is evident that Lehmann's magnesian salt, which agrees with lactate of magnesia in the proportion of base and of water of crystallization, really was lactate of magnesia. In that case the gastric juice contains lactic acid, and thus the problem of the digestive process in the stomach would appear in its chemical aspect, to be completely solved.

"The experiments of all who have studied the gastric juice agree in this, that that fluid contains, along with an organic acid, free phosphoric acid or an acid phosphate, and in this respect its similarity with the juice of the muscles is

strikingly obvious. That portion of the gastric juice which is soluble in alcohol is, in its reaction, identical with the alcoholic extract of soup, as Tiedemann and Gmelin have already shown; and the soup or infusion of meat, free from gelatine and fat, the preparation of which I have described, may perhaps admit of being employed as a valuable remedy for many dyspeptic patients, with the view of increasing the activity of the stomach, and promoting digestion. Again, if the blood or the muscular substance of emaciated convalescents cannot supply the matters necessary for digestion in sufficient quantity for a rapid reproduction of the lost strength (that is, the lost parts of the organism), the benefit derived from well-made soup during convalescence admits of a simple explanation." P. 139.

Although forming but a single chapter in continuation of his work upon the "Chemistry of Vegetable and Animal Physiology," Professor Mulder, as Mr. Johnston remarks, has in fact produced a distinct treatise upon a novel subject—CHEMICO-HISTOLOGY. "The anatomical doctrine of minute structures can only be investigated by the skilful use of the microscope. Their chemical nature, and the changes of composition which they undergo during progressive growth, or by the agency of disease, can only be determined by the joint use of the microscope and of varied chemical re-agents. The skill and dexterity of the ordinary histologist must be united to the knowledge and refined manipulations of the practised chemist, before any results can be obtained, which shall be of real service to vegetable and animal physiology." In this point of view the present work is not merely the best but the only one of the kind existing in which the student will find the histological and chemical characteristics of the various vegetable and animal tissues set forth with remarkable perspicuity and completeness, and illustrated by some excellent coloured engravings. Whatever may eventually be the fate of the author's views concerning proteine and its oxides, these researches and observations must ever remain a monument to his analytical skill and unwearied industry. The instructive details in which this work abounds must be referred to in the book itself, which will become an indispensable companion to every chemist and microscopist, who is desirous of acquiring a correct knowledge of structures hitherto imperfectly described or erroneously confounded with each other. One or two of the more general observations are all we can attempt to lay before our readers. Treating of the importance of considering the form, as well as the composition of parts, he observes:—

"The invaluable investigations, which of late years have been made with the microscope, have proved, that organs performing the same functions consist of the same kind of tissue; whilst those performing different functions consist of different tissues. This leads to the just conclusion, that the form of the parts of which an organ is built up, or by which a function is performed, is in close connection with that function itself, and that, on the other hand, the function is dependent upon the form. If the form be really dependent on matter and the forces residing therein, the function is dependent upon this also. In organic nature, therefore, matter, form, and function go together, and the one being different, the other will necessarily be different also.

"This view of the subject applies as well to all the sickly conditions of any organ, or of the whole organism, as it does to all the healthy functions of plants and animals. The connection existing between different organs of the animal body by nerves and vessels, and both of the animal and vegetable body by cells, is of such a kind, that Bichat's wish—that the organism might some day be

made divisible into such simple parts, that conceptions of vital action might be attached to them, equally simple as those of gravity, elasticity, &c., connected with inorganised bodies—will probably never be realised. But although these conceptions will never attain the greatest possible simplicity, as regards the connection of the constituent parts in an organic whole; we can, nevertheless, approach to that simplicity, and we do approach to it now that we have commenced to establish a connection between form, matter, and function.

"For these reasons a brief survey of the elementary forms of organised parts is at present peculiarly suitable. Where we have as yet but insufficient knowledge of the mutual connection of form, matter, and function, which is too often the case, it will be useful to become acquainted with the forms, though it be only as a part of the chemical history of the organic body. The chemist ought to see something more than gelatine in gelatinous tissue, and the histologist something more than fibres of a determined form. It appears to me, that it is equally necessary for the chemist to know the elementary forms of the tissues, as to know that alum crystallises as an octohedron. It is chiefly to the investigations of Schleiden and of Schwann that we are indebted for our knowledge of these elementary forms." P. 353.

There then follows a concise and excellent account of the Cell-theory as developed by those enquirers, as well as an appreciation of the various objections and modifications that have been subsequently advanced. An extract or two illustrative of this subject may be given.

"But whatever conception we may have of the cell-formation, it is entirely undeniable, 1° That, as there are soluble inorganic substances, which in certain conditions crystallise, so there are soluble organic substances, which in certain conditions form nucleoli, kernels, and cells. 2° That these little organs maintain themselves, and preserve their specific characters, as individuals do by generation, according to the universal law of organic nature, that, the conditions being the same, like produces like. 3° That the endogenous formation of cells is the result of the conversion of organic substances into such as are similar to those of the generating cell itself. The substance must first be made similar to that which has a form, before it can assume that form. 4° That the generating cell, therefore, has but one function to perform—the preparation of a substance identical with that of which itself consists. This substance being once prepared, its organisation is as inevitable a consequence, as crystallisation is a consequence of the mixing of nitric acid and baryta water,—of sulphuric acid and potash, &c. 5° That the parent cell, therefore, does not give a form to the young cells any more than a mother gives a form to her child. A generating being does nothing more than secrete a substance, which, under certain circumstances, can assume a form similar to that of its parent. In this respect there is no difference whatever between the propagation of the higher animals, and the multiplication of cells." P. 367.

Upon the differences between the vegetable and animal cells, Dr. Mulder thus remarks:—

"The cells of plants present some points of similarity with those of animals, but at the same time they are characterised by important differences. The most essential point of similarity is, undoubtedly, the independence of their existence and action.

"But the differences between vegetable and animal cells are as great as the difference of the orders of beings to which they respectively belong. A vegetable cell cannot produce an animal tissue, neither can an animal cell give rise to a vegetable one. The substances from which the former cells are built up, are entirely different from those of the latter. In not one animal cell has even a

trace ever been found of that substance which constitutes the whole of an unincrusted vegetable cell, viz. cellulose. The latter being in all plants, without exception, the material of the original cells, is the cause of the similarity in the structure and functions of plants; and its absence in animals is the cause of the difference between vegetable and animal cells. However cells may be changed by age, the cellulose never entirely disappears, though other substances are often deposited upon it.

"Another remarkable difference between the cells of plants and those of animals is, that the former have a great tendency to preserve—the latter to lose—the cellular form. With the exception of the different kinds of epithelium, feathers, cartilaginous tissue, and a few other substances, the animal tissues, when full grown, always consist of transformed cells or other original particles, produced from cell kernels. These particles have often retained nothing of what they were during their development, except the cell kernel or nucleus, and sometimes not even this; whilst, on the other hand, the great mass of plants, both vascular and cellular, continues to consist of real cells, of which only the walls increase in thickness. Even those parts which are called vessels by botanists,—with the exception of the laticiferous and the spiral vessels,—are nothing but a kind of elongated cells, and to them, therefore, the name of cells is equally applicable.

"There still rests, alas! a thick cloud over our knowledge of the cells of plants, although the most acute philosophers have, with the greatest exertion, directed their attention to the subject. Different cellular systems, though similar in form, though apparently of the same chemical composition, though drawing their food from the same source, produce very different substances. In general the greatest conformity prevails in regard to the cells of plants, notwithstanding the variety in the form and substance of their products. One universal substance, cellulose, exists in *all* plants, constituting the basis, the material of their cells. But this very fact, that one single substance can assume the most different forms, prevents us as yet from giving any satisfactory exposition of this part of natural science." P. 399.

FURTHER REPORT OF THE COMMISSIONERS IN LUNACY TO THE LORD CHANCELLOR. Presented to both Houses of Parliament. 8vo. pp. 500. 1847.

THE article on Insanity in our present number was already in type when this "Report" came to hand; but so gratifying and interesting are some of its contents that we do not deem it advisable to postpone our notice of them. We have repeatedly had to bring under our readers' consideration the present imperfect provisions for the reception, management, and supervision of lunatics; and it is very gratifying to find that the Acts which recently received the sanction of the Legislature, have already done much to remedy some of these evils, and possess a sufficient power to cope with others yet remaining. We certainly were not prepared to anticipate that so much would have been accomplished in so short a time, and its having been so, furnishes ample proof that the Commissioners have entered upon their task in good earnest, and are determined that their posts shall remain no sinecures. So far from this, a brief statement of the extent of

the field over which their labours have to extend will show that these are very onerous, and the responsibilities incurred proportionally weighty.

Thus: in England and Wales alone there were at the beginning of the present year 177 County Asylums, Hospitals, and Licensed Houses; 437 separate establishments for single patients; and 596 Union Workhouses, containing imbecile or insane paupers, varying in numbers frome 1 to about 100 in each workhouse. On the 1st Jan. 1847, these various establishments contained 26,516 lunatics: so that the aggregate number of these, together with the various committees, visitors, medical officers, attendants, and servants, cannot be fairly estimated at less than 30,000 persons. On a rough estimate, the aggregate amount of money annually expended for the maintenance of lunatics, or administered on their behalf, exceeds £750,000; and if to this amount be added the expense of maintaining many families cast upon the parish, and the interest of the large sums invested in the Public Lunatic Establishments, the total expenditure can be little less than 1,000,000 annually; while the capital invested in private and public establishments must amount to several millions. If to the 30,000 English lunatics above mentioned, we add the 12,397 returned as appertaining to Ireland, and the 3,413 lunatic Scotch poor; together with the private patients, attendants, &c. in each country, we have an aggregate of nearly *fifty thousand persons* who are, in the United Kingdom, directly or indirectly, involved in the subject of Lunacy.

The Commission is thus composed: Lord Ashley (Chairman), Lord Seymour, Mr. Vernon Smith, Mr. Gordon, Mr. Barlow, Dr. Turner, Dr. Hume, Dr. Prichard, Mr. Proctor, Mr. Mylne, Mr. Campbell, and Mr. Lutwidge (Secretary.) Three of these gentlemen being physicians and three barristers, constitute the Visiting Commission, all visitations, according to the Act, being made conjointly by a physician and barrister. Ordinary meetings of the Commission are held at least once a week, at which the attendance of one legal and one medical Commissioner is required; while the most important subjects are referred to monthly meetings, at which all the Legal and Medical Commissioners, not otherwise urgently employed on the business of the Commission, attend. Respecting the business hitherto transacted, we find the following observations:—

"The necessity of visiting the numerous Workhouses containing persons of unsound mind, has added exceedingly to the labours of the Visiting Commissioners. In effect, the whole time of the Legal and Medical Commissioners has been absorbed by the business of the Commission, by visitations; by examination of plans, estimates, and accounts; by forwarding the official correspondence; by long and frequent interviews with magistrates, architects, and private individuals; and by attending the Boards held for the despatch of business. Questions of considerable nicety have frequently occurred—some relating to the existing law, as it affects lunatics: others as it relates to County Asylums: others having reference to the state of mind of individual patients, the legality of their confinement, their fitness for liberation, their treatment whilst in confinement, and the due application of their property, &c.

"The amount of ordinary business transacted at this office has far exceeded our anticipations, and has rendered necessary the constant employment of several additional clerks. Besides which, the time and attention of the Board and of individual Commissioners have been much taken up by various enquiries of a special nature, which have considerably impeded the general business of the

Commission. In the case of the Haydock enquiries alone, the time of the four Commissioners was occupied (at two different periods) for eighteen days; ten days at least, in addition, were devoted to the Reports consequent upon the enquiries; and eight meetings of the Board (each occupying, upon an average, six hours) were held, and entirely devoted to the purpose of receiving and considering evidence relating to this particular subject. Several other cases have also occurred which necessarily occupied considerable time, and required much deliberation.

"Without taking into consideration various meetings of Commissioners (not being regular Boards) or the daily attendance at the Office of the Legal and Medical Commissioners, when not engaged on Circuit, there have been held, in the course of the first eighteen months of their Commission, 107 regular Board Meetings (our Chairman almost invariably presiding): in addition to which, the Legal and Medical Commissioners have each, upon an average, visited 409 Asylums and other places receiving Lunatics; have seen 17,749 patients; and have travelled 10,776 miles." P. 135.

Satisfactory is it to state that all this labour has not been in vain; and that, if in some points they call for amelioration, as a whole the new Acts work well and efficiently. Not only is supervision much more active and regular than heretofore; but it is more effectual, since the Commissioners can, through the medium of the Lord Chancellor or Secretary of State (and have done so), revoke the licenses of private houses, and compel the adoption of improvements in those intended for paupers. To them, too, are referred the plans, estimates, and sites for the new asylums, in order that opinions resulting from their own extensive experience of the requirements of the Insane may be pronounced. Their interference, as the present Report exhibits, has been very beneficial in several instances. The result of the increased vigilance and power is, that a great improvement has been manifested in some of the establishments which, by the Report of 1844, were pronounced to be in a very faulty condition. The general disposition, on the part of magistrates, proprietors, and others who have to do with the insane, seems to be the yielding a cheerful acquiescence in suggestions, which the Commissioners seem to exert a praiseworthy forbearance in not converting too hastily into peremptory orders; well knowing that, if adopted by the convictions of those concerned, they are far less likely to be evaded than if forced upon them. In extreme cases, however, they do not hesitate to exert their full and large powers. Many new county asylums are now in progress of erection; and the time allowed by the Act is rapidly approaching, when those of the magistracy who have neglected providing for their insane, will be compelled forthwith to do so. Then the disgraceful neglect of the insane, unavoidable under present circumstances stigmatised in the Report, will no longer exist; and we are pleased to find that the Commismissioners are fully alive to the necessity of selecting the acute cases for prompt treatment, and providing due accommodation for those of a chronic character, which are now fast overwhelming and nullifying the advantages of present establishments. That these poor creatures will meet with all the attention and alleviation their cases admit of, we feel persuaded; as we do that, with such an inspection and control as the Commissioners will be enabled to supply, the humane fears entertained by Dr. Conolly will prove groundless.

Of the improvement which has already taken place, the Commissioners thus speak:—

"We are now desirous of satisfying your Lordship, as far as we are able, that, whatever defects may still be found in Lunatic Establishments, the amonnt of improvement that has taken place of late years, in the comforts and accommodations provided for the Insane, have been great and general. The Public Asylums have been in advance of the rest. The funds by which they are raised and supported, and the causes which influence those who have control over them, necessarily give them a superiority over private establishments. Indeed, we are fully convinced that the Lunatic poor will never be altogether properly provided for, until Public Asylums for the benefit of every county shall have been erected. At the same time we must observe, that there are some Private Asylums in which the pauper-patient is exceedingly well taken care of, and is as judiciously treated as in County Asylums; whilst, on the other hand, there are a few County Asylums which are inferior to many licensed houses.

"The improvement, as we have said, has been general in almost all the existing establishments. It is true that the progress has in no case, perhaps, been altogether regular and undeviating. Defects have frequently been observed; causes for animadversion have occasionally arisen; and every Establishment, however well conducted, has exhibited fluctuations, in respect to the improvement of the patients, easily accounted for, and depending upon various causes in no-wise affecting the character of the Institution. Thus the number of patients occupied varies with the season, a larger proportiou than usual finding employment in those times when gardening and field-labour are especially required. The numbers subject to restraint sometimes exceed the average proportion, owing to the influx of new and violent cases, to the excessive heat or severity of the weather, the latter excluding them from exercise or recreation out of doors; whilst the number of recoveries will obviously depend on the number of recent cases received into the Asylum, or the bodily condition of the patients at the time of admission. Notwithstanding these and similar deductions, however, the improvement upon the whole is, in our opinion, undeniable." P. 64.

As the best evidence of the truth of the above statements, the Commissioners contrast the present state of the various Asylums, Hospitals, and Licensed Houses which have been stigmatised in the different Parliamentary and other Reports, with their former condition, which procured for them such unenviable notoriety. At the same time that they gratefully acknowledge the very great services and assistance rendered by that "most zealous, able, and intelligent body of men," the Medical Superintendents of the Public Asylums, and the ready co-operation of several of the medical officers and proprietors of Licensed Houses (the improvements often involving great outlays of money by these last)—yet the Commissioners properly state that to the *vigilant inspection of the insane* are the originating and maintaining improvements chiefly due, and that, if this is suffered to relax, no security whatever exists against even retrograde movements.

A portion of the Report is devoted to a brief account of the Special Investigations the Board has undertaken. Some of these, such as the Haydock Lodge Asylum, the illegal detention of a lunatic by a person calling himself Dr. Quail, and the prosecution of the attendants at Grove Hall, Bow, and the Nottingham Asylum, have been fully and recently noticed in the public prints; but to one or two other cases we may briefly advert. One of these, relating to the *arrest and imprisonment of a confirmed lunatic for debt*, filled us with astonishment and disgust, ignorant as we were, and we doubt most of our readers are, of the state of the law upon this point. In January last the Commissioners were informed that

Lieut. F., who had been for many years confined in Haslar Hospital, had been brought to Winchester Gaol under an arrest for debt, he being both insane and blind! Dr. Anderson, the Deputy-Inspector of Haslar, and the creditor, were summoned before the Board. Dr. Anderson stated that the unfortunate Lieutenant had inhabited Haslar for twenty years, during all which time he continued insane. He was entitled to a pension of five or six shillings *per diem* half-pay, from which one and sixpence were deducted, and the remainder paid over to his wife. By one of those violations of common sense which law seems privileged to commit, it seems that, although this poor creature was not responsible for his own actions, he was for those of his wife; and she having got into debt, he was arrested, the Lords of the Admiralty having commanded, under the advice of their solicitor, Dr. Anderson, to refrain from preventing process being served on him. The creditor who caused the imprisonment was a dissenting clergyman (alas!) and schoolmaster, to whom the wife had become indebted for the education of her children. Upon the strong remonstrance of the Commissioners this man consented to the liberation of his victim, after an incarceration of some weeks, which would, without such interposition, have been prolonged for three months more, during an inclement winter, until the arrival of the Insolvent Commissioners. After examining the state of the law upon the subject, the Commissioners observe—"It appears, therefore, that Lunatic Debtors are deprived of the benefits which the law extends even to criminals;" and add:—

"In reference to this case, we must observe, that attempts to proceed against lunatic patients are by no means infrequent, and that we have upon all occasions felt it our duty to interpose, as far as we are able, for the protection of the Insane. It appears to us that the *person* of a Lunatic should in every case be privileged from arrest and execution, and that some means should be taken (either by the appointment of a Guardian as Trustee or otherwise) to ensure him sufficient means of defence to any suit or action that may be brought against him. Without some safeguard of this sort, any Lunatic Patient, however urgently he may require medical treatment, may be seized within the limits of an Asylum, and thrown, like Lieutenant F., into prison, to make good a debt for which he was never liable; and even where the person of a lunatic may not be taken, his property is liable to be distributed for a debt to which he may have a valid legal defence." P. 150.

It is to be hoped that humanity and common-sense will not be again violated before an enactment embodying this suggestion can be passed. That such a one should still be wanting seems incredible.

The Asylum or Hospital for Lunatics at *Lincoln* is a strange place, and an account of an investigation of its condition entered into by the Commission is here given. It may be taken as proving by the *experimentum crucis* the utility of efficient and frequent inspection, for being a subscription asylum, it is *not* visited by the magistrates of the county, nor are its rules submitted to the Secretary of State, as is the case with County Asylums. The duties of inspection are self-performed by the body of persons who choose to constitute themselves Governors for Life by the payment of twenty guineas. And a despotic set they are, as may be judged of by the fact, that they pass decrees prohibiting their medical officers employing narcotics, and in various modes interfering with the

other measures they may think fit to prescribe. There are three physicians attached to the place, each of whom successively takes the patients for a month. The same practice does or did prevail at the County Hospital: so that a patient might easily be prepared for an operation by one surgeon, undergo it at the hands of another, and have the after-treatment directed by a third! As in no town in the kingdom is the adage "Doctors differ" so completely verified as in this one, the patient has the full advantage or disadvantage of such a condition of things. The Governors seem to regard it as an excellent arrangement, and in their defence in answer to the criticisms of the Commissioners, observe: "The effect of the rotation is that of a standing consultation. The pointed attention of each Physician is drawn to the case by his individual responsibility (?); successful modes of treatment are adopted and continued; a course of unsuccessful treatment is discontinued; points of doubtful practice are forced into discussion." The physicians have the most diametrically opposite opinions upon the treatment of insanity; and it seems too bad to place the patient in the way of this cross-fire.

"One of them, Dr. Nicholson, advising classification; prescribing opiates occasionally to allay the restlessness of patients who are sleepless or in an excited state; and recommending the adoption of beer or wine as part of the ordinary diet, and frequently ordering them for particular patients; the other two (Dr. Charlesworth and Dr. Elmshirst) being adverse to classification; rejecting opiates in *all* cases; and ordering beer or wine very rarely, and then only as medicines in extreme cases when stimulants or tonics are imperatively required. It is worthy of remark that there is a standing order of the Governors (who are *not* a medical body) by which the use of opiates to produce sleep, as also the use of beer and wine, as parts of the diet, are prohibited. * * * * * * * * * It appears by the 'Physician's Journal,' that Dr. Nicholson visited the hospital throughout the whole of August, and that he was succeeded by Dr. Charlesworth, 1st Sept. Dr. Nicholson on the 30th Aug. directed porter to be given daily to 16 patients and wine to one. On the 1st Sept. Dr. Charlesworth ordered 12 of these to discontinue the porter, and the one to discontinue the wine." P. 359.

Other instances are added, but we need not cite them; the inherent mischievousness of such an arrangement being apparant enough. The Commissioners also object to the absence of due classification; to indecencies which have arisen from insufficient separation of the sexes; to the noise, turbulence, and frequent casualties; the allowing troops of strangers (311 in one month) to visit the wards; to the time of the House-surgeon being occupied by attending these idlers through the wards, and various other duties foreign to his proper province, which he is prevented fulfilling by an order of the Governors, that prohibits him from prescribing for the patients; to the insufficiency of the diet, &c. &c. &c. The Governors have made a most flippant reply, ill-suited to the gravity of the subject, and the courtesy with which the original strictures were delivered. We hope the time is not distant when an establishment possessed of such fine capabilities as this asylum, will not be permitted to waste them in the reckless manner it has hitherto done. That the non-restraint system ever succeeded under such a management speaks volumes for its power of universal application.

Present System of Medical and Moral Treatment of Lunatics in the English Asylums.—This forms the last part of the Report; and is unquestionably a document of great importance, as exhibiting the practice pursued at the chief asylums of this country, obtained by means of a circular letter containing queries addressed by the Commissioners to the various Medical Superintendents. To these, answers have been returned by upwards of fifty of these gentlemen, including in the number nearly every name of eminence in this department of medical science. The substance of these replies is analytically distributed for the purpose of reference, and some of them extend to a considerable length. We can here only indicate the general tenor of their statements, referring our readers to the Report itself, as containing the only authentic account of the opinions and practice prevailing among those whose opportunities constitute our authorities upon this important subject. The queries related to four principal topics: viz. the treatment adopted in Mania; in Epilepsy complicated with Insanity; in Paralysis connected with Insanity; and in Melancholia; and, upon consulting the various replies given at length in the Appendix, we find far less discrepancy of opinion than we were prepared for; and less we think than will be found to prevail upon most other medical topics.

1. *Treatment of Mania*—(1.) *General Bleeding.*—The replies are nearly unanimous in condemnation of this, and even the few physicians who approve of its employment under peculiar circumstances, just as strongly disapprove of it as an ordinary remedy in mania. The Drs. Fox of Brislington House, make an important observation. "Previously to admission, most of our patients have been under medical treatment, and we have often had reason to suspect that the general bleeding to which they had been subjected has been detrimental, and that it has in some cases induced permanent fatuity;" and Drs. Miller and Shapter of St. Thomas's Hospital, Exeter, in like manner state that "the experience of its employment derived from those cases admitted after it had been freely practised, shows it to be evidently injurious, by breaking down the constitution, and conducing to an incontrollable mania, very apt to settle down into Dementia." Dr. A. Sutherland "considers the violent paroxysms of the acute stage as depending not on inflammation, but irritation. He thinks the arterial congestion which is found in cases p. m. the result not of inflammation but of irritation—an effort to repair the mischief sustained in some cases, and in others the effect of anæmia, which venesection would aggravate." Dr. Conolly considers it as "rarely admissible and generally dangerous in insanity." Those physicians who countenance it at all restrict its use to cases in which great plethora, threatening apoplexy, exists—never employing it as a mere means of quieting a paroxysm of excitement.

(2.) *Local Bleeding*, although condemned by some experienced practitioners, is, by the great majority, spoken well of.

(3.) *Emetics and Purgatives.*—The former of these, once so constantly employed in the treatment of mania, are now laid aside by nearly all experienced practitioners, the endeavouring to diminish excitement by small-

er doses of antimony, finding, however, favour in the eyes of several. "I am not in the habit of prescribing emetics," Dr. Sutherland observes, "for two reasons: 1, because in those cases, where there is a tendency to congestion in the capillary vessels of the head, they are known to increase it; 2, because the nerves of the stomach in insanity, as those of the intestines, are often less sensitive to impressions than in a state of health, owing to the disordered state of the functions of the brain, and it is sometimes necessary to give a large dose before the stomach will act." The beneficial effect of *Purgatives*, on the other hand, seems to be everywhere acknowledged, as indeed the frequency of the existence of constipation might have, *à priori*, led us to anticipate. Those of a strong character are generally preferred, and several practitioners speak very highly of the preferability of Croton oil.

(4.) *Anodynes.*—A prejudice long and generally prevailed against the use of this class of medicines in Insanity; but they are now looked upon by our most experienced physicians as very efficacious remedies in several forms of the disease—and especially so in cases of extreme violence and maniacal excitement. The different preparations of opium and henbane, but especially of the former, are those generally preferred: while, as to the efficacy of the Indian hemp, much difference of opinion seems to prevail, due, doubtless in some degree, to the difficulty of obtaining the substance genuine. Dr. A. Sutherland has some very interesting observations upon this class of remedies, a few of which we extract:—

"These remedies are, according to my experience, of essential service in those cases of insanity which border closely upon delirium tremens, in cases of puerperal mania, in the acute stage, and particularly in the paroxysms and sleeplessness of mania, in cases where there is great nervous irritability from poverty of blood, and in cases combined with cachexia, from starvation and other causes. They seem to be contra-indicated when there are symptoms of incomplete general paralysis and congestion of the head. Prescribed merely because the case is one of insanity, without taking into consideration physical symptoms accompanying it, or not in proper doses, or not given sufficiently often during the day as well as during the night, these remedies disappoint the practitioner: they keep up irritation, and add to the excitement instead of allaying it. I have sometimes seen a very simple case converted into a very complicated one by the excessive use of anodynes. At St. Luke's I have been in the habit of prescribing the *Acetate of Morphia* in solution with distilled water—in private practice I often combine it with distilled vinegar (a very old remedy in insanity). The hydrochlorate is combined with advantage with dilute hydrochloric acid. I have found the meconiate of morphia very serviceable in cases where the two former preparations have not agreed with the patient. *Hyosciamus* and *Conium* are also very serviceable in the treatment of insanity. I am in the habit often of prescribing the former in those cases where it is essential that the bowels should not become constipated; and as it also acts upon the kidneys and skin, it is likewise useful when we wish the increase of the secretions of these organs. Combined with potassio-tartrate of Antimony, henbane is useful also in paroxysms of furor. I have seen considerable lassitude follow the administration of *træ. hyoscy.* 3j. with ¼ grain of the former ter die. This is, of course, in some cases, not to be desired. Combined with camphor, opium allays the irritability of those suffering under mania complicated with delirium tremens, and in the incipient paralysis of the insane, tartar emetic is the remedy I place most confidence in. *Conium* is

very useful, either given alone, or in combination with hyosciamus and opium. The boasted effects of *Camphor* have not been realized to the extent, at least, which some of its advocates have insisted upon. I think, however, its effects in allaying uterine irritation cannot be doubted. The combination of hop, camphor, and henbane, is valuable in such cases. *Stramonium* is a remedy which has not succeeded in my hands, although I have tried it in large doses." P. 391.

(5). *Baths* of various kinds, especially the tepid and shower, are well reported of in almost all the answers; many physicians have derived great advantage from the simultaneous application of cold to the head. Still, baths are seldomer resorted to in this country than is desirable; and far less often and for very much shorter periods than in France.

(6.) *Diet and Stimuli.*—Medical practitioners connected with Asylums are nearly unanimous in recommending a generous diet, most of them advising the use of beer, wine, and other stimuli. As patients will oftentimes refuse food Mr. Philipps observes that they should at all times be well supplied with it, so that they may have access to it in the sudden fits and starts during which they will consent to take it. Dr. A. Sutherland frequently finds the various tonics of great use in the cases met with at St. Luke's, in some of which the insanity has indeed been induced by poverty and wretchedness.

2. *Treatment of Melancholia.*—There is, perhaps, less diversity of opinion as to the treatment of Melancholia than with respect to that of Mania. Most of the medical officers, who have given us an account of their practice in this form of mental disorder, seem to agree in directing their attention to the state of the alimentary canal, and the organs subservient to the digestive functions; and to be of opinion that, in cases of Melancholia, the primary disease is often to be sought in some derangement there seated, and that great benefit may be derived from the means which tend to correct disorders of this class. Such are the use of purgatives, tonics, and stimulants of various descriptions. There is, however, likewise a prevalent opinion, expressed or implied, that the vascular system of the brain is in some manner oppressed and disordered in Melancholia either secondarily or primarily. Many of the remedies resorted to, seem to be prescribed upon this hypothesis, and if they are really efficacious and of benefit, which may be supposed from the fact that so many judicious and experienced persons agree and persevere in their use, this must be considered as affording evidence that the hypothesis in question is well founded.

"There is a greater amount of testimony for the beneficial effect of counter-irritation acting on the head in this disorder than in Mania; and even bloodletting, both general and topical, seems to have been in some instances found more useful in Melancholia than in maniacal affections. * * * * *
All the Medical Superintendents who have entered into the nature and purport of our enquiries, are unanimous in advising, as indispensable for the cure of melancholia, a regimen calculated to promote health and vigour of body and mind, viz., much exercise in the open air, cheerful society, abstracting the thoughts as much as possible from gloomy impressions, and some advise the use of wine and other stimulant drinks. Most are of opinion that if sleep does not follow a day spent in exercise of body, it should be procured by the use of some narcotic remedy, such as opium or henbane." P. 205-210.

Dr. A. Sutherland has known prolonged courses of mercury of great avail, and believes that medical treatment is frequently prematurely abandoned. Dr. Willis of Shillingthorpe considers the preliminary action of

an emetic and drastic purge is essential; after which he has been in the habit of prescribing with great success the *tr. guiac. vol.* in *infus. cascarill.*; continuing to employ the warm-bath, exercise, and the flesh-brush, with occasional emetics, purgatives, and blisters.

3. "*Treatment of Epilepsy complicated with Insanity.*—Insane persons, subject to fits of Epilepsy, are generally supposed to be incurable; and there is reason to believe that, owing to the prevalence of such an impression, cases of Epilepsy, complicated with mental disorders, have been much neglected. We have not unfrequently seen, during our visits to Asylums, patients who had been brought from workhouses or from the cottages of their parents, where they have been many years subject to severe Epilepsy and reduced to a state bordering on fatuity, and who have been much improved in their mental and bodily condition after their admission into the Asylums. We have been informed that their paroxysms had become much diminished in frequency, and had in some instances ceased altogether, and that the mental faculties of these patients had become much less oppressed. This has been principally attributed by the medical officers to the improvement in diet, and the greater opportunities for exercise in the open air, inducing a general amelioration in the physical condition. That such instances would be more numerous than they are, if cases of Epilepsy were not neglected under the supposition of their hopeless nature, cannot be denied." P. 211.

When we consider the infrequency of cure in simple epilepsy, it is not surprising that an opinion of its incurability when complicating insanity should have become very general. Dr. A. Sutherland draws a distinction between epilepsy preceding and epilepsy succeeding insanity; and states his belief that the latter is *not* incurable, and may at least be alleviated. He prescribes, however, only the remedies ordinarily in use, and so frequently in vain, for epilepsy. Mr. Casson, of Hull, says he has never known a *chronic* case of epilepsy connected with insanity recover; and in all the patients whose brains have been examined by Sir A. Morrison at the Surrey Asylum, organic changes have been observed. Most practitioners seem to rely much on hygiene and diet as measures for mitigating the severity of the disease; but some difference of opinion prevails respecting the appropriate degree of fulness of diet allowable. "I have tried," says Dr. T. Prichard, "on a large scale every remedy proposed in the works of various authors, upon the chronic cases that have come under my care, but in no one case successfully;" and Dr. Conolly states, "I have never seen a case of epilepsy in an adult permanently cured by any medicine whatever." We are disposed to think this is the true state of the matter; and although much may be done to improve the condition of these patients, and diminish the number of attacks, that they are never cured.

4. "*Treatment of the General Paralysis of the Insane.*—The peculiar form of disease distinguished by this name was not recognised and described till within a few years, though it must always have existed. It is now well known in all the large Asylums, as one of the principal causes of mortality among the male patients. It is most frequent among persons whose constitutions have been impaired by vicious courses and intemperance, and among those who have been reduced to extreme debility by want and other depressing causes. General paralysis has been almost invariably thought to be hopeless of recovery, and its

victims usually perish within two or three years from the commencement of the disease." P. 217.

Dr. A. Sutherland, however, who seems more fortunate in the results of his practice than most of us, states he has met with three instances of recovery; and he seems chiefly to rely upon calomel and counter-irritation. This last, together with cold to the head and local bleeding, is employed by many others in the early stages of the disease. The Drs. Fox believe the disease may be sometimes arrested by the iodide of mercury, by prolonged open blistering of the scalp, and the use of the electro-galvanic apparatus.

5. *Moral Treatment.*—The Commissioners take a review of the present state of medical opinion upon this point; but having so recently adverted to the various topics it embraces, we will content ourselves with one extract:—

"The principle of treatment adverted to in these remarks is important in two different points of view. In the first place, as furnishing a great part of the resources available for the cure of mental diseases, when the circumstances are such as to hold out a hope of recovery; and, secondly, as promising materially to lessen the sufferings and increase the comfort of incurable patients. In this last respect, it is of greater moment than any medicinal remedies. As a means of cure it ought never to be lost sight of. But there is reason to apprehend that the attention of medical men has been of late years too exclusively devoted to what is termed Moral Treatment, to the neglect, in some instances, of the resources of medicine. They appear occasionally to have lost sight of the fact, that insanity never exists without a physical cause, namely, some disturbance of the functions of the brain; disorders of the mind being only the result of some temporary or permanent derangement of the organism, by means of which all mental operations are carried on; whence it seems to follow that physical agents ought to be resorted to in the first instance, as the means of restoring the healthy and natural state. From the replies, indeed, which many of the Medical Officers of Asylums have given to the questions submitted to them by us, it may be perceived that the fact to which we have just adverted, has operated upon their minds, though there appears to be some variety in the methods in which they have acted under its suggestions. The conviction with which most of them seem to have been impressed is, that the disturbed state of the brain, which is the proximate cause of insanity in its various forms, is, in most instances, the result of disorder in some other part or function of the body, or of some serious derangement of the general state of health; and that the principal resources available for the cure of the cerebral affection consist in measures calculated to remove the original disorder of the physical or bodily functions, and to restore the health of the constitution in general. Hence the general recommendation of means likely to promote vigour of the body, such as exercise in the open air, ample diet, the careful administration of stimulants and tonics, bathing, warm clothing, and healthful recreations. Experience, as we might collect from the replies which have been received, if no other means of information existed, has fully confirmed the truth of this fundamental principle. It may, indeed, be observed that, in general, the number of recoveries from Insanity is found to be in proportion to the degrees in which the curative resources above alluded to have been employed. Under the old system of keeping patients bound hand and foot, in cells often dark, loathsome and disgusting, and feeding them with coarse and unwholesome diet, the result was an accumulation of chronic cases, and a frightful aggravation of human misery. The present humane method of treating the Insane, and the

provision made at the public cost, for Pauper Lunatics, of Asylums furnished with every resource for promoting health and comfort, exhibit in a striking point of view the intelligence of the age; and while they promise to diminish the numbers of the permanently insane, cannot fail to alleviate, in a great degree, the sufferings of that most afflicted class of human beings." P. 230

THEORIE EXPERIMENTALE DE LA FORMATION DES Os. Par *P. Flourens*, Secrétaire Perpetuel de l'Academie Royale des Sciences (Institut de France), &c. Paris, 1847.

An Experimental Theory concerning the Formation of Bone. By *P. Flourens*, Perpetual Secretary of the Institute of France.

THE process of ossification is interesting to the physiologist on several accounts, independently of its bearing on the mere formation of bone. In consequence partly of the mechanical texture, partly of the chemical nature, and partly of the microscopic texture, some of the most important points connected with the first formation and subsequent growth of organic structures, have been made out by the careful investigation of the ossifying process. The activity of deposition going on in growing animals in general, may, for example, be inferred from the fact that the whole skeleton of a young pigeon becomes deeply tinged in twenty-four hours, by giving the animal one meal in which madder has been mixed: by modifying this experiment—by feeding animals with madder, by withholding that substance, and again administering it, some of the interesting phenomena connected with absorption can be advantageously studied, whilst at the same time light is thrown upon that subtle molecular action, which is incessantly proceeding in the animal economy: again, by tracing with the aid of the microscope, the various and successive changes displayed in the soft amorphous matter or cartilage, and in the organic cells imbedded in this as in a matrix, and by observing the relations of both these elementary matters to the blood-vessels, several important facts relative to the actions of cells have been determined.

In the treatise before us, among other matters, M. Flourens gives the results of his experimental enquiries respecting a subject of much interest to the practical surgeon—the process, namely, adopted by Nature in the formation of bone. He also re-publishes his experiments upon the effects of madder on the colouration of bone; and concludes with some remarks on what is here called "the great and new problem concerning the relation of forces with matter in living bodies." In the first chapter the author explains his theory on the formation of bone, as embracing the six following propositions:—"1. That bone is formed in the periosteum. 2. That it increases in thickness by superposed layers. 3. That it increases in length by layers in juxtaposition (*par couches juxta-posées.*) 4. That the medullary canal enlarges by the absorption of the internal layers of the bone. 5. That the heads of bones are successively formed and re-absorbed, in order to be again re-formed, so long as the bone

grows. 6. That the *continual mutation* of matter is the great and marvellous spring (until now unknown) of the development of the bones." P. 1.

We need not remind our readers that this theory is essentially that of Duhamel, who, in his celebrated and admirable memoirs, contended, to use his own words, that the bones increase in thickness by the superaddition of the layers of the periosteum, which, by becoming ossified, form the substance of the parietes of the medullary canal. We cannot agree with M. Flourens that these views, opposed, as it is well known, by Haller, have been rejected by almost all physiologists. On the contrary, many excellent observers, among whom it will suffice to mention Dupuytren in France, and Howship in this country, have ascertained, in a way that has been deemed by many subsequent writers perfectly satisfactory, that in the reparation of fractured bones, the periosteum does undergo the same changes as those discovered by Duhamel, although the process by which union is ultimately effected is differently described. The subject, however, required further investigation; and we are quite ready to recognise the value of M. Flourens' researches.

In order indisputably to prove the immediate agency of the periosteum in this process, the author excised in several dogs a portion of the rib, (which bone being fixed to the spine and sternum, the divided ends could not fall together), carefully leaving the periosteum. On examining the part four days afterwards, this membrane was found very much swollen and thickened; in six days the periosteum, left between the divided ends of the rib, was already converted into cartilage, and in the midst of this, "two small osseous nuclei were found perfectly distinct, circumscribed, and far distant from the two extremities of the rib;" in sixteen days the nuclei were blended with one, and in three months with both ends of the divided rib. In a subsequent chapter, an account is given of some experiments in which the extremities of the humerus, radius, &c. were removed in young dogs, and in which, the periosteum having been left, the membrane became thickened, osseous nuclei formed in it, and at length in this way the head of the bone was re-produced. From all these researches it is concluded, that the new bone is produced in the periosteum, without contact in the beginning with the old bone; that the original bone does not elongate itself; that its extremities never bud out or rejoin themselves, the new bone being always interposed between them; and, lastly, that the new bone is formed in the periosteum itself, and not in any substance or extravasation whatever foreign to that membrane.

In another set of experiments it is shown that, wherever there is periosteum, bone will be formed; for instance, if a hole be bored in a bone and a canula be introduced into it, the periosteum soon enters the canula, becomes there thickened, cartilaginous, and at last is converted into bone.—(*L. c.*, p. 11.)

In order to establish the second proposition—that the bone, namely, grows by superposed layers, the author surrounded with a ring of platina wire, various bones of dogs, rabbits, Guinea-pigs, &c. "At the end of some time the ring, which at first immediately surrounded the bone, is found surrounded by the bone, and is contained, at length, in the medullary canal;" that is to say, the wire, placed at first between the periosteum and the bare surface of the bone, becomes by degrees covered by new

layers deposited externally to it. As regards the increase in length, several experiments are related, like those which Hunter performed for showing the same fact, in which two holes having been made in the tibia of a growing rabbit, it was found, when the animal was killed some time afterwards, that the distance between the holes remained precisely the same, although the bone had become considerably lengthened. This shows, therefore, that the bone does not grow in length by the extension of its tissue, but by the addition of new layers, placed in juxtaposition, and formed by the fibro-cartilage, which separates the epiphysis from the shaft.

It has been stated above, that if a wire ring be placed around a growing bone, that at length it is found in the medullary canal. Two explanations have been given of this fact: according to Duhamel the bone is extended, and being pressed by the ring, is broken, in order to be re-united beyond the ring; according to Hunter, on the contrary, it happens that whilst the bone on the one hand acquires external layers which cover the ring, it loses on the other its internal layers which are re-absorbed. In order to determine this point, and also to obviate the objection that a metallic ring, by its pressure and resistance, might break the bone, M. Flourens employed a very small and thin plate of platina, which being isolated and free, could offer no resistance. The result, however, was just the same; the plate became covered with new layers of bone, and the original bone being absorbed, in 36 days the piece of platina was found in the medullary canal. In this manner the fourth proposition is demonstrated—the medullary canal enlarges by the absorption of the internal layers of the bone. The evidence (p. 29) by which the author wishes to prove that, so long as the bones are growing the heads are successively formed and re-absorbed, does not appear to us very satisfactory.

The first chapter concludes with the affirmation of a principle, which, although it is more susceptible, for the reasons already stated, of demonstration in the osseous tissue than elsewhere, is of universal application in animal bodies; it is that of the continual mutation of organic matter. This is proved, in the instance before us, by the experiments made with metallic rings and plates just noticed, which show that the body of the entire bone is without cessation re-absorbed and re-formed—"that the continual mutation of matter isthe great and marvellous source of the development of the bones." This continual renovation is during growth so very active, that a few weeks suffice in the dog and rabbit for the entire renewal of the shaft of such a bone as the tibia; the longest experiment of this kind lasted only 36 days.

In the succeeding chapter M. Flourens relates some very interesting experiments, illustrative of the powers of absorption and reproduction possessed by the periosteum and the medullary membrane, the ultimate object being to prove *the identity of these two membranes*. It is, however, necessary to explain that what is here called the medullary membrane, is quite distinct from the tissue which lodges the marrow. The latter membrane, it is well known, is nothing else than a portion of the common adipose tissue, and consists, like that, of vesicles in which the medullary matter is contained. The structure and exact relation of the former membrane are most important, and bear so immediately on the present question, that they require to be briefly explained. This so-called medullary

membrane is a very thin, and highly vascular cellular tissue, which immediately adheres to the osseous substance, lining firstly the large internal cavity, and from thence being prolonged into all the cells of the cancellated substance and into the Haversian canals, where it meets, and is anatomically confounded with, the processes derived from the periosteum; to this important membrane may be very appropriately applied the term suggested by Dr. Walshe, *endosteum*.

The first position sought to be established is, that "the medullary membrane (endosteum) is the organ which absorbs the internal layers of the bone." We know from the valuable researches of Troja, that if a long bone be sawn across and a stylet be introduced into the medullary tissue, so as to destroy it, necrosis ensues, and a new bone is formed around that which has died. The author desiring to operate on the entire bone, instead of a part only, modified this experiment by making a perforation in the radius of a goat, and then, by the introduction of a stylet into the medullary membrane, destroying the whole of the internal membrane.

In a short time an entirely new radius was formed around the original bone, and with it also a new medullary membrane. But, and this was the immediate object of the experiment, it was likewise found on examining the external surface of the original and now dead bone, that it was rough and, as it were, eaten into; whilst at the ends of the shaft, the absorption had proceeded so far, that portions of it were completely removed. A careful inspection showed that the agent of this removal was evidently the newly-formed medullary membrane belonging to the new case of bone, and which presented on its internal surface an unequal aspect arising from small mammary eminences with depressions between them. It was found by another experiment, that if the small rib of a rabbit were introduced into the medullary canal of the tibia of a dog, that, after a time, it became corroded and partially absorbed by the thickened medullary membrane.

The following are the author's conclusions from this set of experiments:—1. That the destruction of the medullary membrane of a bone is followed at first by the death of this bone, and afterwards by the formation of a new medullary membrane and a new bone. 2. That the new bone forms itself in the periosteum of the old bone. 3. That this same periosteum of the old bone produces the new medullary membrane, which at first holds to this periosteum, and only separates from it by the interposition of the new bone. 4. That the internal surface of the new medullary membrane, alternately excavated and mammillated, dissolves and eats away by degrees the old bone, and finishes by absorbing it." P. 41.

By other experiments, in which the periosteum was destroyed, and the bone consequently deprived of its vitality, it was found that the medullary membrane, which had been left intact, had the power of forming a new bone; this became covered by a new periosteum, and was situated in the medullary cavity of the original bone; hence the author concludes that "the medullary membrane does, like the periosteum, produce bone." By these same experiments it was further shown that the periosteum has the power of absorbing bone. Other researches prove, lastly, that the periosteum is capable of producing or forming the medullary membrane.

In considering these investigations, we need scarcely point out how intimately they are connected with the phenomena of necrosis, upon which

they throw much additional light. The formation of the osseous case around the enclosed dead bone; the separation of the latter by the absorbing powers of the newly-formed medullary membrane; and the worm-eaten and furrowed appearance of the surface of the sequestrum, are all readily comprehended by the preceding experiments, which are themselves explained by the normal relations of the endosteum and periosteum.

The author investigates in the third chapter a subject which has been a frequent object of experiment—the formation of callus. As it is now generally agreed that the theory of the celebrated Duhamel is in the main correct—that is to say, that the periosteum is the agent by which the new osseous matter is produced—it will suffice, if we merely extract the summary of M. Flourens' researches. "When a bone is fractured, the periosteum begins to swell and thicken, and to send prolongations between the ends of the broken bone; this is, for the periosteum, the first stage; in the second stage, the membrane attaches itself to these fractured extremities, and unites itself to the medullary membrane; then one or several osseous nuclei appear in the periosteum; lastly, these osseous nuclei develop themselves, extend, attach themselves to each end of the broken bone; and the fracture is re-united. Thus all takes place in the periosteum—it is the periosteum which swells; it is the periosteum which attaches itself to the extremities of the broken bone; it is the periosteum which unites itself to the medullary membrane; it is in the periosteum that the osseous nuclei arise and develop themselves; and these nuclei are the callus, the only callus, the intermediate solid which re-unites the fracture, which re-joins the ends of the broken bone." P. 61.

As it is the intention of M. Flourens speedily to publish his microscopic researches on the effect produced by madder on the intimate texture of bone, a subject which has already given in the hands of Mr. Jones and others, some very interesting results, we shall not on this occasion notice the account given in the present work, of the general effects observed when bones thus coloured are examined by the naked eye. In the interim, however, we would advise those of our readers who have access to the treatise before us, to peruse the chapters relating to the action of madder, which are illustrated by some beautifully-executed figures.

In the concluding part of this Experimental Theory, the author briefly touches upon one of the most mysterious phenomena of the animal frame —the incessant mutation of organic matter. To the common observer the solid material of the body seems to be fixed and unchangeable; but the philosopher regards it as being continually in a state of motion and renovation, a condition thus happily expressed by Buffon: "That which is, says this illustrious writer, the most constant, the most unalterable in nature, is the print or the mould of each species; that which is the most variable and the most corruptible, is the matter which composes it." The same idea is thus eloquently developed by Cuvier: "In living bodies no molecules remain in their place; all enter and go out successively; life is a continual vortex, of which the direction, all complex as it is, remains constant, as well as the kind of molecules propelled by it, but not so the individual molecules themselves; on the contrary, the actual matter of a living body will soon be there no longer, and yet it is the depository of the forces which will constrain future matter to march in the same direction

with itself. Hence the form of these bodies is more essential to them than their matter, since this changes without cessation, whilst the former persists unaltered. It is then a false idea of life to consider it as a simple bond which holds together the elements of the living body; since it is, on the contrary, a spring which moves and transports them incessantly."

Animal chemistry has in some sense, by analysing the débris of the body, given us the measure of this eternal movement. Thus Lehmann has ascertained that, by substituting violent for moderate exercise, the quantity of urea, which represents more especially the waste of the muscular organs, is increased about one-third; whilst Prout, by showing that the proportion of alkaline phosphates in the urine is augmented by mental labour, has proved that a similar law prevails in the nervous system. That the same principle applies to the bones, though in a less degree, is certain; and one of the merits of M. Flourens' researches, is the clear demonstration they afford of this fact, already known from other sources. "The mechanism of the development of the bones is the *renovation*, the *continual mutation* of all the parts which compose them. This bone which I regard, and which grows, has no longer at this moment, any of the matter it had some time since; and soon, it will have nothing of what it now possesses; and yet, in all this perpetual renewal of matter, how little does it change its form."

Having completed our brief review of these Experimental Enquiries, it only remains for us to express the gratification and instruction we have derived from them; a sentiment which we are assured will be re-echoed by all who occupy themselves with the study of physiology.

REPORT ON THE CLIMATE AND PRINCIPAL DISEASES OF THE AFRICAN STATION; Compiled from Documents in the Office of the Director-General of the Medical Department, and from other sources, in compliance with the directions of the Right Honourable the Lords Commissioners of the Admiralty. Under the immediate direction of Sir William Burnett, M.D., K.C.H., F.R.S., by *Alexander Bryson*, M.D., Surgeon R. N. 8vo. pp. 266. London, 1847.

WE beg to tender our best thanks to Sir William Burnett for a copy of this most interesting and valuable Report. Would that the resources of our Army and Navy Medical Boards were more frequently brought into requisition, under the superintendence of their enlightened chiefs! That the Report before us must prove of very great utility to all medical men who may be called upon to visit the pestiferous shores of Africa, will be best appreciated by those who have been in that unhealthy region without such a companion and guide. To the readers of this Journal it cannot but be peculiarly acceptable at the present time, as forming an excellent commentary on some of the principal topics discussed in the elaborate article on Yellow Fever in our last number. All that we propose to do, just now,

is to extract those portions of it which will best serve to illustrate the history of that malignant epidemic disease: we shall add but very few remarks of our own, deeming it better that each reader should investigate the points of relation between the present and the previous article for himself, and judge accordingly.

"The Topographical Remarks on the African Station, extending from Cape Verde on the North to Cape Negro on the South of the Equator," are very interesting, and form an appropriate introduction of the work. The details would, as a matter of course, be unsuitable here. We shall merely select two or three passages that will be found to bear on some of the observations and extracts in subsequent parts of our narrative.

It is well known that Sierra Leone is, perhaps, of all places along the coast—at least since Fernando Po has been abandoned—the most pernicious to the health of Europeans; at all events, the greatest mortality usually occurs there. But whether this be owing to indigenous or to accidental and contingent causes, it is not very easy to determine; seeing that there is a constant influx and reflux of strangers from different parts, and more especially the frequent arrival of prize-crews on board captured slavers. Then, too, we have to consider the extra-fatigues and exposure of the sailors, attendant upon refitting and watering there, &c.; not to mention the greater facilities which they have of committing all sorts of irregularities at this settlement than elsewhere.

"Remittent fevers occur throughout the whole year, but from November to April they are by no means of a formidable character, unless contracted under peculiarly aggravating circumstances, such as an intemperate course of living, with exposure and fatigue in the marshes, or from sleeping in the open air during the night.

"From July to October the nature of the place is totally changed, and no vessel, during that period, can remain more than a week or two at a time at anchor near the settlement, with safety to the health of her crew. During the former month the rains commence, and continue to fall, with short intervals of fine weather, until September, when the river, having become enormously swollen, and of a tawny colour from the admixture of soil, overflows its banks, and floods the marshy flats behind the settlement to a great distance, where, as the rains decline and the river withdraws into its proper limits, it leaves large shallow lagoons, to be evaporated by the heat of the sun and the drier winds of the succeeding months.

"During this season, therefore, Europeans, whether on shore or in vessels at anchor in the river, are apt to be assailed by remitting fever of a more virulent character than during the dry season, while the more acclimated residents suffer from intermittents generally of an irregular type." P. 5.

Dr. Bryson gives a truly graphic description of the "demon of the swamp." Let it, however, be remembered, in making use of this expression, that Freetown itself "is built upon the base and side of a rocky hill, completely denuded of bush, and incapable, at any time of the year, of retaining much moisture. The hills to the westward, as far as the sea, are of the same geological formation, and are now nearly cleared of natural bush. Eastward from the town, so far as the junction of the Bunce with the main stream, there is a narrow strip of land of a more tabular form with a few patches of natural scrub, at the distance of several miles; all within that consists of cultivated fields, a race-course, villa enclosures and gardens;—yet this part of the colony has sometimes been described as a

swamp, although it by no means deserves the name. Towards the eastern base of the hills, upon each side of the road to Kissy, the ground is dry, rocky, scant of soil, and, in many places, strewed over with small granitic boulders and fragments of plutonic rock. There are also several small rivulets, but nothing deserving the name of marsh. A great part of the mountain ridge behind the hill, upon which the town is built, and the intervening valley, have also been cleared of their natural thickets, whilst the latter, in many places, is under cultivation,—so that if marsh effluvia have anything to do with the constant succession of sporadic fevers occurring at Sierra Leone, which it is apprehended they have not, they must be swept by the wind from the upper part of the river at its junction with the Bunce, or from the banks of the latter, a distance, considering the astonishing elasticity and miscibility of the atmosphere, it is difficult to conceive that they could be borne without becoming thoroughly innocuous." Notwithstanding the very great difference in the physical condition of the country at different parts of the coast, the character of the prevailing and endemic diseases is the same in all.

Is it not truly melancholy to think that the fell ravages of fever are too often so powerfully aided, on the one hand, by the imprudent exposure of the crews in the boats, and on the other, by the wicked recklessness of the men themselves when, either by permission or otherwise, they are withdrawn from the superintendence of their officers. The practice, which has been not unfrequently adopted, of increasing the quantity of grog in such a climate as that of the African Coast, cannot be over strongly reprobated. Would that, by increasing the comforts of sailors in every respect, and by the adoption of those means that are best calculated to elevate their moral character, and to make them think that they are something more than mere hired servants of the state, the use of this—often, at least—most pernicious beverage, were lessened rather than increased!

Might not the medical officers often exert a salutary influence in convincing the men of the folly and danger of unbridled indulgence? It is not by the peremptory command of a captain, so much as by the affectionate exposition of a disinterested friend, that the good object is to be attained.

We do not feel disposed to go so far as Dr. Bryson does in his, all but, condemnation of the use of tobacco. In moderation, and always provided it be not made an apology for spirit-drinking, smoking is one of the safest exhilarants in an unwholesome climate. Chewing should unquestionably be discouraged. On the whole, coffee is by far the best beverage that can be served out to the men.

It is, perhaps, scarcely necessary to preface the following observations on the medical history of the squadron on the African station from 1823 to 1845 by stating that, of all the diseases which affect the vessels there employed, Remittent fever is by far the most pernicious and destructive. "It prevails throughout the whole line of coast, at all times and seasons of the year, as an *endemic*; and, at distant and uncertain periods, it occasionally assumes an *epidemic* form." . . . . . "In a few instances," adds Dr. Bryson, "it appears to have *acquired* contagious properties."

1823, 1824, *and* 1825.—During the month of February 1823, fever of an epidemic and malignant character began to manifest itself at Sierra

Leone, and rapidly extended to the crews of vessels in the harbour, in the river, or in the adjacent rivers and creeks, and to the different settlements along the coast. About the end of March, H.M.S. "Bann" became first affected. The subsequent history of the disease on board this ship has, it is well known, been minutely described in the admirable report of Sir William Burnett.* It will be useful to give the leading points of the narrative.

"The first case appeared on the 25th of March, after she had been at anchor off Free Town from the 11th January until that date, the crew during that time having been employed refitting the ship, and also in refitting a small prize vessel which had been converted into a tender. By the surgeon's report it also appears 'that they were much exposed to the heat of the sun's rays, and had, perhaps, indulged in irregularities, circumstances that have never yet been known to fail in producing fever at Sierra Leone, particularly when persisted in for several weeks in succession.

"On the 26th of March the master and two seamen were next attacked, but recovered.

"On the 27th the vessel sailed from Sierra Leone, and between that date and the 31st three more cases were added to the list, and other four on the 3rd of April.

"According to Captain Phillips' account, the sick list then rapidly increased, the disease beginning forward in the ship, came gradually to the after part, till nearly all the officers and men were attacked. Indeed, when it ceased at Ascension, about the 11th of May, only sixteen of the officers and ship's company had escaped. The total number was ninety-nine, of whom thirty-four died, fifteen of them before the Bann reached Ascension. The disease had also attacked that part of the crew which was detached in the tender, San Raphael, to reconnoitre the Gallinas; and after her return to Sierra Leone it raged with such fury that at one time it was determined to destroy her.'

"'The Bann, on her leaving Sierra Leone, was ordered to St. Thomas', but from the unhealthy state of the crew and the bad weather, it was deemed advisable to proceed directly to the island of Ascension. On her arrival at that place, tents were erected on shore, at the distance of nearly five hundred yards from the garrison, all intercourse with which was interdicted, and the whole of the sick, amounting in number to forty-five, were landed and placed in the tents provided for them.'

"Eighteen days after her arrival, viz., on the 11th of May, a boy (son of one of the serjeants of the garrison) was violently attacked and died, but it is neither known nor believed that he had any nearer communication with the sick of the Bann than passing daily at no great distance from the tents to feed his father's poultry.'

"About this time the fever in the Bann had nearly ceased, but it went on daily attacking some of the garrison; and it appears by the official report that twenty-eight were taken ill, of which number fifteen died and thirteen recovered. The disease finally became extinct upon the island about the 16th June." P. 37.

Sir William acknowledged his inability to account with certainty for the origin of the fever, either in the Colony or in the Bann; but, at the same time, expressed his opinion "that it was in the first instance merely

* *Official Report on the Fever which appeared on board H.M. Ship Bann on the Coast of Africa, and amongst the detachment of Royal Marines, forming the Garrison on the Island of Ascension in the year* 1823. By William Burnett, M.D. &c. 8vo. pp. 78. London, 1824.

the common endemic of the country, brought on by hard labour and exposure to the sun, not possessing, under these circumstances, any contagious properties, and continued to be so until after the middle of February; that it subsequently, by the state of the weather preventing ventilation, and from a great number of the sick being confined in a small place, became contagious; and that, though it was impossible to trace the fever in question directly from the Bann to any individual of the garrison of Ascension, yet there is just reason to believe that the disease was introduced into the island by that ship."

The "Cyrene" also was at Sierra Leone in March, 1823. Several of her crew, it is stated, contracted fever from intemperance and exposure on shore. Of eleven men, who were affected, one only died; so that it is evident that the disease was not of a malignant type. During April and May, the ship was employed off the Gold Coast. Only one case of fever occurred there, in a man who slept five nights ashore in the town of Cape Coast. The Cyrene sailed for England in December. "During the year, upwards of eighty cases of fever occurred on board, principally between March and September, seven of which proved fatal."

The "Owen Glendower" arrived on the station in the early part of 1823. On the 26th of March, when at Sierra Leone, there was not a man on the sick list. Although the men were a good deal exposed, only a few cases of fever occurred. After leaving this anchorage, the duties of the crew, while employed in the boats in the Bight of Biafra, were most severe and harassing. Upwards of seventy cases of fever of a remittent character occurred. "The disease," it is added, "was not contagious, and was, in every instance, contracted by exposure and irregularity, chiefly amongst those on detached service."

Towards the close of the year, she proceeded to Sierra Leone, where several cases of fever occurred; only one, however, proved fatal. In February, 1824, she arrived at Cape Coast, where the marines and a party of seamen were landed to garrison the castle; but, on account of the great abundance and cheapness of spirits, it was found impossible to keep the men sober. They were, therefore, more particularly as their health began to suffer, re-embarked. A large proportion of them suffered subsequently from hepatitis and colonitis;—diseases which "are now of comparatively rare occurrence on this part of the coast." There occurred also a number of cases of fever, amongst the men who had been exposed at the castle.

It is scarcely necessary to detail any particulars respecting the "Swinger," or the "Maidstone." Of the former vessel we read that, when at Bunce Island, twelve miles higher up the river than Sierra Leone, "she was secured alongside the wharf, and her holds cleared out, the crew in the meantime having opportunities of indulging immoderately in trade rum. In consequence of their imprudence, fever of a malignant character broke out and carried off eight men." It does not appear to have spread.

On board the "Victor," there occurred, between June, 1824, and March, 1825, between twenty and thirty cases of fever, of which five terminated fatally. In one of these fatal cases, "a liquid, like coffee-grounds, was found on dissection in the stomach."

After mentioning the cases of the "Athol" and the "Redwing," Dr.

Bryson makes the following general remarks on the state of health during the years 1823, 1824, and 1825 :—

"It appears that there existed a great amount of sickness, both throughout the squadron and throughout the different European settlements along the coast, than usually happens; but whether this resulted from accidental circumstances, or from some epidemic condition of the atmosphere, there is no means of determining. The probability however is, that both in some degree assisted in causing the more general prevalence of disease, and the increase in the mortality.

"In the first place, there was a greater number of soldiers, the refuse of other regiments, sent out to Sierra Leone to fill up the ranks of the African corps, many of whom were men of incorrigibly bad habits, who, in a manner, drank themselves to death in a short time after they had landed in the colony.

"There was also a greater influx of Europeans upon the Gold Coast, in consequence of the Ashantee war, in which several of the vessels of the squadron also took a part, particularly in the defence of Cape Coast Castle; and it would appear that the squadron congregated more in harbour than it has ever done since. All these causes, therefore, if they did not add to the virulence of the epidemics in 1823 and in 1825, at least tended to multiply their victims.

"The prominent features of the disease, whether in the endemic or epidemic form, appear to have been identically the same with those presented to the medical officers upon the coast in the present day, notwithstanding the great diminution of bush in some parts, and particularly around Sierra Leone. Lassitude, dull erratic pains and rigors marked the stage of invasion; heat and headache, with general pains, thirst, intolerance of noise and light, irregular pyrexial exacerbations and remissions, the stage of maturation; yellowness of skin, stupor, and somnolency, dark dry tongue, irritability of stomach, black vomit, and black dejections, the stage of decline in cases terminating in death.

"In almost every instance where the disease assumed a formidable character, its origin could be traced to one or more of the common well-known predisposing and exciting causes, namely, to undue exposure to the vicissitudes of the weather, either on shore or in boats near the shore, combined with fatigue, cold, wet, insolation, or with intemperance, and other imprudences included under the head of irregularities.

"The Bann contracted the fearful scourge, which swept off nearly one-third of her crew in little more than two months, at Sierra Leone, from a protracted exposure to the influence of that pestilential locality. The Cyrene contracted a similar disease, although less virulent, from similar causes, and in the same locality. The Owen Glendower, in the Bight of Biafra and at Sierra Leone; the Swinger, in the rivers Pongos and Bunce; the Redwing, in the rivers of Benin; and the Atholl at Bunce Island and at Sierra Leone. It however does not appear to have assumed a contagious form in any vessel but the Bann." P. 45.

1826—1830.—It would seem that, from the year 1825 to 1829, fever did not prevail as an epidemic, or with an unusual malignancy, in any of the settlements on the coast, or on board any of the vessels of our squadron. The latter year, however, was marked by the outbreak of a most fatal pestilence, primarily (it was believed) at Sierra Leone, and subsequently at Fernando Po, and in three, if not more, of the ships of war on the station. The following details will be found to possess much interest.

The "Eden" arrived at Sierra Leone in Sept. 1827, and sailed for Fernando Po in the beginning of October, with the necessary stores for the projected establishment at Clarence Cove on that island. In the course of the next month, "ulcer made its appearance on board in a

somewhat malignant form." Its occurrence was attributed, no doubt very properly, "to the crowded state of the ship, there being more than double the number of people on board that there was fitting accommodation for." Notwithstanding the vessel was several times whitewashed and otherwise purified, the disease continued (in the Spring of 1828) to prevail. It was remarked that those persons, whose ulcers became foul, were generally attacked in groups of three or four in one night, and mostly after much thunder and lightning, preceded by several days of hot, sultry, and oppressive weather. The sloughing process only attacked the lower extremities, particularly the feet and ankles, and never ascended higher than midleg.

On the 29th of October, 1827, the Eden reached her destination. It was not long before the unhealthy nature of the place became felt; several cases of fever having occurred before the end of the year. It became more general in April of the following year.

In consequence of its showing a disposition to spread in the hospital that had been established on shore, the whole of the sick were removed on board. Besides these, two cases of fever were removed from the Horatio tender, employed in cruising off the Calabar river on the opposite coast, and one came from a prize.

During the Spring quarter, ulcer was entirely subdued; but four cases of dysentery occurred. The ship, being free from disease, sailed in June for Sierra Leone, which she reached on the 6th of July, and again left on the 21st. During this fortnight it rained with very little interruption. In the course of a few days after leaving Sierra Leone, the usual fever made its appearance, and was chiefly confined to men who had recently volunteered from timber ships, and most of whom had been living on shore and committing every kind of excess. The disease was marked by great mental depression, with but little determination to the head. The remissions were in some cases very indistinct. In the majority, there was more or less of yellow tinge in the eyes and skin; this appearance was usually first observable about the seventh day.

While at sea, between the 17th of September and the 12th of October, cruising off the Bonny and occasionally anchoring near the coast, there were not any severe cases of fever; but, on returning to Fernando Po on the day last-mentioned, five were received from the shore, in a very dangerous state. On the 20th, she sailed for the island of Ascension, having embarked seven cases of fever, all of which were contracted on shore. She returned to Fernando Po on the 26th of December, and remained (it is presumed) in Clarence Cove until the beginning of April of the next year, 1829. On the first of May she arrived at Sierra Leone, all on board being then healthy. At this time, some cases of "malignant fever" occurred on shore and on board the trading vessels in the river, *on which account medical men augured an unhealthy season.* The Eden at once began to suffer,* and from the first week in May to the 11th of June, when she arrived at Fernando Po, having left Sierra Leone on the 26th of

* The first cases seem to have been two midshipmen, who were taken ill on board a prize of the Eden. Both died.

May, between 40 and 50 of the crew were attacked, and of these 25 died. The whole of the officers, with the exception of one lieutenant and the gunner, were either dead, or confined to bed. "The men were dying daily, amidst almost incessant rain and frequent tornadoes, accompanied with much thunder and lightning; the main deck was crowded with sick, and constantly wet. The moral effects of these scenes became palpable in every countenance; while, from the want of medical attendance, the surgeon and two assistant-surgeons having died, it was impossible to pay that attention to the ventilation of the ship, or even to the personal comforts of the sick, which their situation required." The sick were landed on the following day upon an isolated spot; and the ship was thoroughly cleansed, whitewashed and fumigated. But it is unnecessary to follow out the details. Suffice it to say, that "the deaths in May amounted to twenty-seven; in June, to thirty-one; in July, to thirty-two; and in August, to seven; while, out of thirty men left in hospital at Fernando Po, only nineteen were alive on the 1st of December; making the total number of deaths from fever and its sequelæ, between the 1st of May, 1829, and the 1st of December of the same year, one hundred and ten!—of whom fifty died on board the Eden, and fifty on shore at Clarence Cove. Thirteen were natives of Africa, all the others were Europeans."* The deaths must have exceeded two-thirds of the persons attacked. The disease was most indubitably malignant Yellow fever, *typus icterodes*. The symptoms of the last stage are thus given:—"The debility increased; the eyes became more yellow, bloodshot, and glassy; the skin also became of a yellow tinge, and covered with a cold perspiration, with sordes on the teeth, chapped lips, and hurried respiration, vomiting of black matter (black vomit), sometimes delirium and convulsions; at others, coma and insensibility to surrounding objects closed the scene. All the deaths occurred between the third and ninth day of the disease, but the majority on the fourth or fifth."

Respecting the supposed origin of the pestilence, and the cause of its propagation, Dr. Bryson, after alluding to "the febrific exhalations eliminated in that pestilential spot," Sierra Leone, where the Eden was when the sickness first appeared on board, says:—

"The emanations from the flats between the town and the Bunce River, and from the Bullom shore, although seven miles distant, together with the state of the weather, are therefore supposed, in the first instance, to have originated the disease, which spread with fearful rapidity and virulence over the greater part of the colony, and assumed a more than ordinary degree of malignancy amongst the shipping at anchor in front of the town; some merchant vessels in fact lost nearly all hands. From a variety of circumstances, it was considered not to have been transmissible from person to person, although it appeared to be developed in certain infected spots, and that exposure for a very short time to the exciting cause in a concentrated form, was sufficient to produce the specific effect: thus a soldier contracted fever in the East St. Vincent merchantman, although he remained only two hours on board." P. 63.

We must now look at the history of the "Sybille" frigate, which arrived

* "This number probably includes several men who were not entered on the ship's books." P. 64.

at Sierra Leone from England in May, 1827. Four cases of remittent fever occurred on board in the beginning of June: they seem to have recovered. All that is known of the vessel for the next two years is derived from a paper, that was communicated by Dr. M'Kinnal to this Journal.* From this it appears that she had been cruising in the bights of Benin and Biafra from December 1828 to the 21st of June 1829, when she arrived at Fernando Po, the health of the crew having hitherto been wonderfully good. Here they found the "Eden," which was still suffering from the ravages of the frightful epidemic with which she had been visited. The "Champion" too was there, having arrived from Sierra Leone on the 14th of the month, *i. e.* just a week before the Sybille. The colonial surgeon and his assistant, who had come from England on board the former vessel, died soon after landing at Fernando Po.

On the 22d, the day after her arrival, the Sybille received on board several men from the Eden. On the 23d, one of these was attacked with fever and immediately sent on shore; and, in the course of the evening of that day, she sailed from Clarence Cove. On the 26th, a boy fell sick, and on the 2nd of July, a marine (who had come on board from the garrison of Fernando Po) was seized.

"From that period the disease continued to show itself in different parts of the ship while at sea. It soon assumed a most malignant character, and attacked individuals of every class, age, and temperament, although the negroes were affected with it in comparatively small numbers, and in a mild degree. On the most minute investigation, the surgeon was unable to trace the disease, either from man to man, or from mess to mess; and those who attended the sick were not more affected than those who kept aloof. The sick-berth attendant, and the surgeon himself, though both unprotected by previous attacks, escaped the fever. The sailmaker, who sewed up the dead bodies in their hammocks, had a slight attack, though he had formerly had the yellow fever at Jamaica; while the boy who assisted him escaped. Every attention was paid to cleanliness and ventilation; and the dead bodies were speedily committed to the deep, together with their bedding and clothes.

"'The disease was evidently yellow fever in the greatest degree of intensity. With two exceptions it was of the continued kind; the stage of excitement short. In the worst cases, it terminated fatally between the third and sixth day, most frequently on the fifth. Death was preceded in a great number of cases by black vomit, often accompanied by a dingy or livid hue of the countenance. Yellowness of the eyes and skin was very common before death; it varied from a pale lemon colour, to a dark orange hue. An officer, who died on the eleventh day of a relapse, had previously suffered from yellow fever in the West Indies.'

"It is worthy of remark, that of the eight marines received from Fernando Po and the Eden, who took bark during eight days, two only became affected with fever.

"'The sudden cessation of the disease,' says Dr. M'Kinnal, 'on the 28th of August, when forty men were on the list, and when the power of contagion (if it existed) must have been at its height, seems to prove that atmospheric changes had great influence in the production of the disease, as well as in its extinction.'

"'On the 12th of September, the ship arrived at St. Helena without a man on the sick list; being the seventeenth day from the date of the last seizure, and the third day from the date of the last death by fever. After two days' quarantine, the officers

* Vide Medico-Chirurgical Review for October, 1830.

and men of the Sybille went on shore, and mixed with the inhabitants from that time till the 25th of October, without any accident resulting from the intercourse.'" P. 54.

The Sybille again anchored at Fernando Po in November, when the settlement was considered healthy. She touched at Princes' Island, and on the 5th and 6th of December was at anchor off Whydah. She then sailed on a cruise, and arrived at Princes' Island on the 3d of January 1830, all well on board. There she was joined by her tender, the "Black Joke," which arrived from Sierra Leone, where she had been very sickly and lost 23 of her men. Her crew, however, were now entirely recovered or convalescent.

"When the Sybille was weighing anchor on the 7th, a boy, who laboured under common tertian, came on board from the Tyne, which latter vessel was stated to be healthy. Six days afterwards, namely, on the 13th of January, yellow fever again broke out in the Sybille, while cruising off Cape Formosa. Dr. M'Kechnie, an assistant-surgeon, who had lately come from England, and who had been on board the Black Joke for a few minutes, was the first seized. The disease soon increased in the ship, and early in February it become very alarming, producing the most dreadful havoc amongst all classes on board. The number of cases during this visitation amounted to eighty-seven; of which twenty-six died, with the usual symptoms of the most malignant yellow fever."

"Afterwards, in St. Helena Roads, on the 22d of March, the fever again broke out, twenty-two cases occurred, and six died." P. 54.

After this period there does not appear to have been any malignant fever in the Sybille.

The circumstances connected with these two destructive attacks of the disease on board—the first having occurred immediately after communicating with the "Eden," which had lost her captain, surgeon, assistant-surgeon, and a great many men by fever contracted at Sierra Leone, and the second after communication with the "Black Joke"—reasonably suggest the idea of infection as the cause of the sickness. The surgeon, indeed, attributed it principally to "noxious emanations from the interior of the ship, probably caused by the decomposition of the wood from the long-continued action of heat and moisture, aided, perhaps, by an accumulation of different substances under the limber-boards of the holds." But then the existence of such an accumulation has been positively denied by officers who served in the ship, and who have asserted that no vessel could be cleaner or better ventilated. Lastly, we must not omit to mention that, although her surgeon uniformly maintained that the fever was not infectious, the officers and men thought it was highly so. The commodore, from humane motives, prohibited all Europeans, with the exception of himself and the medical officers, from visiting the sick.

We have now a few words to say respecting the Medical History of Fernando Po. That remittent fever is *endemic*, and therefore continually present there, has been too well known ever since the place was first occupied in Oct. 1827; but it was not until the end of June 1829, that it prevailed *epidemically* in the settlement. It has been seen that the Eden arrived there from Sierra Leone in a most sickly state on the 11th of this month, and, after a few days quarantine, was permitted to send her sick on shore; that the Champion arrived from the same place on the 14th, and that

several bad cases of fever had been landed from her; and that the Sybille arrived on the 21st, her crew at that time in health; her subsequent history has just been given. Now for the outbreak of the epidemic fever on the island:—

"On the 29th of June, a sergeant of marines was attacked; on the following day four other cases occurred, and it then became general; between the above date and the 31st of August, there were no less than seventy-seven persons prostrated by the disease, to thirty-nine of whom it proved fatal—a mortality that sufficiently stamps the malignancy of the disease. It however appears that the season had been unusually wet during the months of July and August, when the disease had acquired its greatest virulence." P. 68.

It deserves, however, to be stated that Fernando Po is, in all years, and especially at certain seasons, exceedingly unhealthy. As a proof of this, it may be stated that "of thirty mechanics, who arrived there in November, 1827, all had suffered; the number that died cannot be ascertained; a few are invalided, and five only remained when the Eden arrived in June, 1828;" and that "of the whole party of marines and mechanics, including officers and women, landed upon the island in the middle of June, amounting to fifty-eight, only four had escaped an attack of fever on the 31st October, 1829, and most of the others had experienced two." Altogether, Fernando Po is unquestionably one of the most pestiferous and deadly spots on the face of the wide globe. After about five years' occupation, it was at length utterly abandoned in the beginning of 1833. It should be stated that, while the epidemic of 1829 was raging, several cases occurred of invalids being received from shore on board ships in the harbour, without communicating the disease to the rest of the crew. We must not omit to mention, at the same time, that "this fever was distinctly remittent in its character, and accompanied by yellow suffusion of the skin and eyes, and by black vomit."

Dr. Bryson, after carefully considering all the circumstances of the case, makes the following significant remarks:—

"It will be observed, that both the Eden and Champion contracted the disease at this colony (Sierra Leone), and in their crowded state carried it with them to Fernando Po, where the supernumeraries for the colony and all the sick were landed in the course of a few days after their arrival. Amongst the former it continued to rage for the two succeeding months with unparalleled fury, and also assailed other individuals who had no direct communication with either vessel. It also appears that it broke out in the Hecla at Sierra Leone, or shortly after she left it, and carried off thirty-nine of her ship's company. The Sybille, as previously detailed, contracted the disease at Fernando Po in June, and in the course of a few weeks lost twenty-two men out of the sixty-nine attacked. In the Black Joke, however, in prizes, and otherwise, she lost in addition thirty-seven, making the total number of deaths fifty-nine for the year. There was not any vessel that suffered in the same proportion with the Eden; during the year, out of a crew it is supposed of not more than one hundred and sixty men, she lost altogether ninety-nine, sixty of whom died on board, and thirty-nine on shore. The loss in all the other vessels on the station during the year was, comparatively speaking, trifling, with the exception of the Sybille's tender, the Black Joke, from which there was not any returns sent into office, in consequence of her being attached to that vessel, and manned exclusively out of her ship's company; it is, however, ascertained that she also contracted the disease at Sierra Leone.

"After the disease had been extinct for a period of upwards of four months,

it reappeared in the Sybille in January, 1830, and again, in the same ship, at St. Helena in March. In these two visitations her losses amounted to thirty-two men, and then it may be said the epidemic of 1829 and 1830 finally ceased, although fever of an equally virulent character nearly unmanned the Plumper, in the November and December of the latter year, at Sierra Leone." P. 87.

From 1831 *to* 1836.—During the five years following 1830, fever in a malignant and epidemic form did not appear in any of the vessels of the squadron, the "Conflict" excepted. For twelve months after her arrival on the station, she suffered but little from the common remittent of the country; but in July, 1831, after the greater part of the crew had been on shore at Sierra Leone, and allowed to commit the greatest excesses, thirty cases of bad fever, of which eight terminated fatally on board and five were sent to the hospital, were the result of these imprudences.

"The disease appears to have been of a most malignant character. It was remittent, but varied in its symptoms in different cases; in the worst it was attended with great excitement, and, as it advanced, the skin assumed a yellow colour, interspersed with livid spots. Towards dissolution, in the fatal cases, a quantity of dark matter was vomited, while a disagreeable cadaverous smell exhaled from the body some hours before life became extinct. The dejections were also frequently dark and fetid. In one instance, in the course of half an hour after death, the whole surface of the body presented a dark blue colour, and the cuticle separated. There can be but little doubt that all these cases were what is usually termed yellow fever, in its worst form. Still, although every circumstance is very minutely detailed, contagion is not alluded to." P. 96.

The Conflict sailed in August for Ascension, where, upon being overhauled, "the hold presented, on the removal of the tanks and limberboards, a very filthy appearance, blackish mud with vegetable matter being brought into view, the effluvium from which was at first insufferable. The passages to the pump-well were found to be completely blocked up."

1837 *and* 1838.—The "Ætna" returned from Gibraltar to the African station in Nov. 1837, and anchored at Sierra Leone on the 30th, all on board being then quite well. Malignant fever was at the time committing great ravages on shore, and amongst the shipping. The Ætna only remained until the 3rd of December, for the purpose of watering, and this was effected by the Kroomen: she then proceeded to sea. The weather was still calm, wet and sultry.

"On the 10th of the month two cases of fever occurred, and on the 12th there were two more; of these three died, one on the fifth day, and two on the seventh day, of the disease, with black vomit and yellowness of the skin. There were not any fresh attacks until the 20th of December, when two others occurred, and on the 21st there were five. The disease then began to attack officers and men indiscriminately. As it was considered to be contagious, recourse was had to artificial means of ventilation, by swinging stoves and windsails, and to fumigation, by whitewashing the decks and sprinkling them with chloride of lime. In the meantime a course was shaped for the region of the trade winds, with the view of making Ascension; on getting into the S. E. trade, however, on the 15th of January, the violence of the disease did not abate, but on the contrary, it continued to attack one after another of the remaining few who had hitherto escaped, with as much virulence as it did when the ship was becalmed in the immediate neighbourhood of the land; nor did it entirely cease until the 20th

January, the day on which she anchored at the above island. The total number attacked was ninety-nine, including one Krooman and four African boys; of these twenty-five died. The total number of the ship's company, exclusive of the Africans, was ninety-eight, and of these only five escaped, two of the latter being nearly all the time on the sick list, one with intermittent fever, and the other with rheumatism.

"The disease was considered to have been contracted at Sierra Leone, and its influence was supposed to have been the greater upon the ship's company from mental depression, in consequence of their being obliged to return to the coast of Africa instead of being paid off, as they had anticipated, from a general scorbutic taint amongst them at the time, from the laborious nature of the service on which they were employed, and from incidental privations peculiar to it. The fever was distinctly of a remitting character, attended with yellow suffusion of the skin and eyes, hæmorrhage from the gums and fauces, and, in the fatal cases, with black vomit." P. 120.

No allusion is made asto whether any, and what, measures were taken at Ascension, on the arrival of the ship.

It is worthy of notice that the Ætna returned to the coast towards the end of spring, and re-commenced surveying, the convalescents having been discharged to duty, although still in a weakly state. Upon the whole, however, the crew continued tolerably healthy until she returned to England in October, 1838.

The "Bonetta" and the "Forester" were also very sickly at the beginning of this year, 1838. The former left the coast (in a healthy state we presume, for nothing is said to the contrary), having taken on board a supply of Indian corn and yams for the island of Ascension. In the second week of January, she fell in with the latter in a sickly state, several cases of bad fever having occurred on board. After receiving a prize-crew from her, the vessels separated: and the Bonetta proceeded on her voyage to Ascension, which she reached on the 20th January, having lost eight men from the disease on the passage. The report continues thus :—

"In consequence of the sickness and death of the assistant-surgeon, there is not any account of the origin and progress of the disease until another joined on the 3d of February, when the state of the sick list and ship were as follows :—The commander, master, assistant-surgeon, purser, and twenty-eight seamen and marines, were all lying upon the deck in a most helpless and melancholy state, three with black vomit, and to all appearance beyond the aid of medicine. The vessel was in a very filthy condition, the stench from the holds being almost insupportable, and totally incompatible with health. It may also be added that, on subsequently clearing her out, the corn and yams with which she was freighted were found to be in a state of decomposition.

"Shortly after her arrival, tents were erected on shore, and the whole of the crew were landed and placed in them ; the sick being separated from the healthy. This precaution was unnecessary, or at least without the desired effect, as the few remaining Europeans and three Africans were almost immediately added to the list ; making a total of thirty-nine, of whom twenty-eight recovered and returned to duty, three were invalided and sent to England, and eight died.

"The fever in this instance appears to have displayed all the usual characteristics of the common remittent or yellow fever ;—yellowness of skin, bleeding of the gums, and black vomit. It is to be regretted that there are not any means of obtaining information relative to the first appearance of the disease, or of its progress prior to her arrival at Ascension on the 30th of January, in the deplorable condition previously described. The first assistant-surgeon who took charge of her, after her

arrival, does not allude to the question of contagion; the other who succeeded him on the 16th of February states that, from the information he had received, he was led to regard the disease as decidedly contagious. The sick were admitted to pratique on the 1st of March." P. 122.

With respect to the "Forester," all that we learn is that, "in the early part of the year, and again in May, fever of a most virulent character assailed the ship's company at Sierra Leone, and nineteen fell victims, being upwards of a third part of the whole: amongst them was the assistant-surgeon. There is therefore not any history of the disease until the 20th of June, when two-thirds only of the crew were in existence, and in a state of convalescence."

Besides these vessels, the "Curlew," the "Fair Rosamond," and the "Waterwitch," suffered severely in the first half of this year, 1838. The history of the last-named ship deserves particular notice.

"The 'Waterwitch' was employed cruising on the north part of the station, the crew being healthy until she arrived at Ascension in April, when numerous cases of diarrhœa occurred, supposed to have been occasioned by the water of that island having been drunk while in a turbid state. Its progress was immediately checked by abstaining from the cause. About this time a malignant fever prevailed among the inhabitants on shore, but there was not any sickness on board the Waterwitch up to the 3d of May, when she left for Africa. On that day one case of malignant fever occurred; and again on the 13th, ten days after being at sea, there were four more added to the list, all resembling the first, but presenting more decidedly the characteristics of true yellow fever. From this time the malady continued its ravages until the 4th of June; within that period no less than sixty had been attacked, and fifteen died. The crew consisted of fifty-two Europeans and eighteen native Africans; only three of the white men escaped. Many of the Africans also suffered, but their attacks were comparatively mild, and not attended with danger.

"There was not any possibility of separating the sick from the healthy: but ventilation and cleansing were had recourse to as far as circumstances would admit, although unfortunately this was next to an impossibility, so great was the quantity of provisions and other stores taken on board at Ascension for the use of the squadron. The state of the weather, together with the locality (the Bight of Biafra), where the rains had just commenced, also tended to aggravate the disease. As the vessel approached the coast, the number of cases increased with such rapidity, that there were at one time twenty-three fresh attacks within the space of three days, leaving only five white men to do duty on deck, although from the 4th to the 6th of June there had been only three!

"The fever was characterised by remissions, redness of skin, black vomit, black urine, hæmorrhage from the nostrils, throat and mouth, and vomiting of blood. There were several recoveries after hæmorrhage from the mouth, throat and nostrils had taken place, but there were not any after black vomit had occurred." P. 128.

The "malignant fever among the inhabitants on shore," here alluded to, appears to have broken out in the latter part of March, after heavy rains on the 16th and 17th of that month, preceded by some years (?) of dry weather. "Among the people of the island there prevailed an opinion that the disease was imported by the vessels from the coast"—viz. the Ætna, the Forester, and the Bonetta. The surgeon, however, of the hospital at Ascension seems to have been of a different opinion, and to have regarded the disease as of local origin. The town was, at the period of

the invasion, in a most offensive state, in consequence of the collection of stagnant putrid water in many parts of it; but whether we can admit the sufficiency of this as the real and primary cause of the epidemic, and quite independently of the arrival of sickly vessels from the African coast, which had been more than usually unhealthy during the season, we shall leave to our readers to judge for themselves, more especially as Dr. Bryson does not think fit to express his own sentiments upon the question.

There is nothing in the medical history of the following 5 years (1839—43) that calls for particular notice. At no time, does malignant fever appear to have prevailed epidemically on board any of the squadron, or at any of the settlements.* We shall therefore pass to the year 1844, during which there was certainly more than the average amount of sickness in several of the ships on the station. In the early part of the year, the "Hydra" (which had been cruising off the coast to the South of the Line, and had also been up the Congo river) suffered not only from remittent fever, but also severely from diarrhœa; no fewer than seventy-two cases of this disease having occurred. Upon this point, Dr. Bryson makes the important remark that "this has been a frequent precursor of severe epidemic attacks of fever upon the station; it was so in the 'Eclair,' and in many other instances heretofore noticed in these remarks; there is reason therefore to suppose that it is but a different effect of the same general morbific cause. Is does not appear, however, that the diarrhœal affection has any effect in warding off an attack of fever; it is in fact more of the nature of a predisponent than of a prophylactic; while by lowering the general tone of health, it renders the constitution less capable of withstanding the shock of the disease afterwards." Towards the end of the year, fever again made its appearance on board the Hydra, and then seemed to assume two forms; "'one being mild, and the other malignant;' the former was the more general, and occurred among men who had not been out of the ship, particularly amongst the stokers; the latter, few in number, occurred in the gig's crew, who were exposed to concentrated malaria for a period of forty-eight hours in the river Sherbro."

During the last three months of the year, the "Growler" steamer had 63 cases of fever, 21 of febrile catarrh, and 11 of dysentery—"an amount of disease that clearly enough indicates the pestilential nature of the locality," Sierra Leone and the adjoining coast. Twenty fresh cases of fever occurred in the course of Jan. and Feb. 1845: three of these terminated fatally. In March, she went to the Cape de Verde islands to recruit the health of her crew, returned again to her old station, where she remained till the following August, and then returned to England.

The "Lily" and the "Penelope" do not seem to have suffered more than usual during this year, 1845; but we find that on board the "Styx," six days after leaving Fernando Po where the crew had worked hard in coaling and watering the ship, fever of a malignant type made its appearance. From the 23rd of November to the 3rd of December, thirteen cases in all occurred; and of these, five proved fatal—one on the third day, three

* It may be of importance to know that, during the interval mentioned, the men were much less engaged in the harassing and dangerons duties of boat-service than they had ever been before.

on the seventh, and one on the ninth. "During the early period of the disease, the exacerbations and remissions were remarkably distinct, there being a good day and a bad one alternately; on the good day there was nearly a total remission of febrile symptoms; but as the fever advanced, and the vital energies declined, the remissions became more irregular, the skin being sometimes dry and harsh, at other times covered with a cold clammy perspiration; the thirst was then great, the tongue become cleaner and sometimes red and shining. One of the fatal cases occurred in a Krooman; in him the disease presented a very aggravated form, with early and restless delirium."

The medical history of the "Eclair," from the time of her leaving England in Nov. 1844 to her disastrous return in September of the following year, is traced with considerable minuteness by Dr. Bryson. As a matter of course, we have no occasion to follow him in this narrative. We shall, therefore, merely mark one or two topics that will serve to complete the lengthened details which we have already given, first in the number of this Journal for July, 1846, and subsequently in that for July of the present year, when we brought the instructive report of Dr. McWilliam under the attention of the reader.

Between the 8th of March and the 3rd of April, a number of cases of Diarrhœa occurred on board. As already stated, this is a frequent precursor of the invasion of malignant fever on the coast of Africa.

The first cases of fever were observed in the men who had been employed in the boat expeditions in the Sherbro river. Up to the 15th of June, fourteen cases in all had occurred; and, of these, no fewer than nine had proved fatal; seven among those (26 or 28 in number) who had been in the boat expeditions, and two among those who had not been out of the ship. From this circumstance Dr. Bryson infers "that the disease was contracted from local causes exterior to the ship; for although two cases occurred in persons who had not been out of her, still from her close proximity to the land, the whole crew must have been more or less exposed to the same malarious emanations as the men employed in the boats, although perhaps in a less concentrated form, while, not having suffered from privation and fatigue, they were not so susceptible of the disease."

Notwithstanding the amount of sickness already experienced, it appears that the expeditions in the boats were not discontinued until the 2nd of July, when the staemer sailed for Sierra Leone, where she arrived on the 4th, "the crew, comparatively speaking, healthy, and the last remaining cases of fever advancing favourably towards convalescence." The subsequent stay there for three weeks in one of the most unhealthy months of the year; the cleaning out of the hold of the "Albert," with the irregularities and excesses then committed; the unfortunate permission granted to the men to go on shore, few returning at sunset, as ordered; the subsequent exposure of part of the men in painting and refitting the other steamer—these are circumstances that are well known, and will not fail to be taken into account in examining the history of the Eclair epidemy. Not a word is said by Dr. Bryson as to the time when the first case of "black-vomit" occurred among the fever patients; although (it will be remembered) Sir William Pym took it upon himself to fix the date with precision! We need scarcely say that he disapproves, in the strongest terms, of the delay on the part of Sir William to have the crew immediately removed

from the vessel on her arrival at the Motherbank. On this point there can be but one opinion, and that opinion is against the measures that were taken.

We do not remember having seen the following account of the fever on board, as it existed when Mr. Bernard joined the vessel at Madeira: Mr. B., in his first report, says:—

"The cases which presented during the above period were characterized by intense frontal head-ache, and a sensation of weight over the eyes, severe pain across the loins, and in some cases there were general pains; the tongue loaded with a white mucus in the centre, leaving the edges and apex of a bright red, its substance firm; thirst urgent, the skin hot and dry, bowels constipated, the pulse small and without any hardness. In the course of about six hours, vomiting of a greenish yellow fluid took place, accompanied with pain in the epigastrium, or across the chest. The vomiting then became continuous. The dejections procured by purgatives or enemata were dark and fetid. On the evening of the second day brownish flocculi might be detected in the fluid vomited, which increased on the third day, by which time a sinking of the pulse was observable, with coldness of the extremities, and sometimes low delirium, whilst at others there was a wildness of manner, and a disinclination or absolute refusal to take either food, drink, or medicine. The tongue then became of a bright red colour; in some it was quite moist, but generally dry. From this state none rallied. Such were the prominent symptoms, although the latter sometimes did not occur until the fourth or fifth day. The Kroomen, who are still on board, and have been employed in attendance on the sick, have not suffered." P. 191.

There is no mention here of any remissions, or tendency to their occurrence; and Dr. Bryson subsequently says that the fever was "of a typhoid character."* In conclusion, Dr. B. points to the close similarity between the cases of the Eclair and of the Bann, as we had previously done in our article of last year. His words are—"Both vessels contracted the disease at Sierra Leone, and apparently from the same cause or causes, and under similar circumstances. In both vessels, in the course of a few weeks it assumed an epidemic character, if it did not acquire contagious properties; the one vessel proceeded to the barren rocky island of Ascension, a few degrees to the south of the equator, where a disease of the same character made its appearance amongst the inhabitants, and committed great ravages; the other proceeded to the nearly equally barren island of Boa Vista, a few degrees to the north of the equator, where in like manner a disease a short time afterwards broke out, and raged with equal severity."

From the tabular account given by Dr. Bryson of the total loss sustained by each vessel of the squadron on the African Coast, during the year 1845, it appears that, while the Eclair lost no fewer than 74 men from fever, the greatest mortality from this cause on board any other ship did not exceed 3! The following table,—exhibiting the annual mean strength, and the number of deaths from disease, from accident, and from all causes,

---

* The following statement, which, as far as we know, has not been made known before, deserves to be noticed:

"The holds of the Eclair it was supposed had been made perfectly clean, while her crew were disembarked at Boa Vista; but there was afterwards found, when she was re-commissioned, a large collection of mud, fully three inches in depth, upon that portion of her bottom occupied by the boilers and machinery, which apparently had not been disturbed for a long time." P. 223.

between the years 1825 and 1845, both years inclusive—serves well to point out how pre-eminently sickly some seasons were above others:—

Year.	Annual Mean Force.	Deaths.		Total Deaths from all causes.
		From Disease.	From Accident.	
1825	663	41	7	48
1826	1043	57	6	63
1827	955	40	4	44
1828	958	81	3	84
1829	792	202	2	204
1830	667	72	4	76
1831	785	22	3	25
1832	512	18	3	21
1833	562	12	10	22
1834	620	18	8	26
1835	815	19	3	22
1836	965	16	4	20
1837	815	105	4	109
1838	885	115	3	118
1839	790	55	5	60
1840	855	32	3	35
1841	1070	68	17	85
1842	1330	43	29	72
1843	1267	23	4	27
1844	1715	43	6	99
1845	2540	121	7	128
	20,604	1203	135	1338

"Total mean force for twenty-one years, 20,604; ratio of mortality per 1000 of mean force, 58·4; ditto from all causes, 64·9." P. 177.

"It would thus appear," adds our author, "that the annual ratio of mortality from disease alone on the African station for a period of twenty-one years, was 54·4 per 1000 of the mean force employed. The fatal nature of the climate, however, becomes more apparent when placed in juxtaposition with the mortality on other stations, to wit:—

South America - - -	7·7
Mediterranean - - -	9·3
Home - - - - -	9·8
East Indies - - - -	15·1
West Indies - - - -	18·1
Coast of Africa - - -	54·4

"It is proper, nevertheless, to observe that nearly one-half of this proportional amount resulted from epidemic fever alone, which was confined to a few vessels of the squadron during the years 1828–9 and 30; again in 1837–8, and 39, and in the Eclair in 1845. Deducting the loss from epidemic fevers, therefore, the ratio of mortality from all other classes of disease on the station will be about 20·0 per 1000 of the mean force annually; this, however, can give no adequate idea of the permanent loss of health, which is assumed to be great. Still, from these and other data, it seems fair to deduce that if boat-service were in some degree restricted; if prize crews were not permitted to land at Sierra

Leone, and if all vessels contracting epidemic disease were to leave the station, and proceed directly to a colder climate, the ratio of mortality, and the permanent loss of health one year with another, would be reduced at least nearly one-half." P. 178.

And here we must draw our narrative to a close. With a few remarks on the general history of that disease, which, as we have seen, is every now and then apt to prove so terrible a scourge to our brave seamen in their perilous duties—would that success commensurate with the peril incurred, not to say with the sacred justice of the cause, might reasonably be expected!—off the fatal coast of Africa, we shall conclude.

From the following passage it will be seen that the views of Dr. Bryson, respecting the nature of true Yellow Fever, and also upon the important question of its communicability or power of infectious propagation, correspond very exactly with those which we endeavoured te enforce in our last number. He first alludes to the confusion that has been introduced into the nosological returns of the navy, by the adoption of different names applied to the same disease by different medical officers.

"The bilious remittent of one person was found to be the climatorial of another; the endemic of a third was the typhus icterodes of a fourth; the adjectives ardent, yellow, congestive, inflammatory, had all been used in describing the same disease. A more simple phraseology was therefore, unless under peculiar circumstances, deemed advisable. The character of these fevers, in fact, is such that the synochal of one day may become a remittent on the next, and probably ere long terminate in an intermittent; the ephemeral of little force may suddenly become one of high vascular action; or at the same time, but in a different subject, pass rapidly through the stage of excitement, and at once enter upon the *typhoid;* while that which invades with great intensity of action may frequently be of ephemeral existence only. It is therefore obvious that it is not until the fever approaches its termination that it can be brought under any one of the previous heads; consequently in a practical point of view such visionary distinctions are of little or no importance." P. 250.

He then subjoins the important remark:—

"The fevers of Africa, strictly speaking, are only divisible into two kinds; namely, into the remittent and intermittent. The former, however, may be subdivided into the endemic, epidemic, and contagious; but as either of the former, as in the Bann, may be converted into the latter by improper ventilation, the depressing passions, and physical prostration, and as it—the contagious—does not originate or even exist for any length of time except under these conditions, the subdivision is again reduced to two heads—the endemic and epidemic, both of which are remittent, and both generally, according to their persistence, attended with more or less yellowness of the skin; and occasionally in the more severe cases with black-vomit. It becomes a question if the latter be not an aggravated type of the former, in consequence of the more general prevalence of a common exciting cause. Still, from its uncertain modes of invasion at distant periods; from its apparent restriction to certain bounds; and from its greater severity, the appellative distinction, at least until the subject is better understood, remains strictly warrantable." P. 250.

Notwithstanding the doubt expressed in the last sentence, the fair and legitimate deduction from all the statements of this volume appears to us to be, that the malignant or pestilential malady, known as the true or genuine Yellow Fever, is but an aggravated and epidemic form of the ordinary and endemic fever of the coast; this having become—from causes which we but

very imperfectly understand*—more of a continued type, and being then accompanied with symptoms that are clearly indicative of a dissolved or diffluent state of the blood. That Dr. Bryson regards it as of the nature of Typhus, would appear from his using such expressions as the following in reference to the disease as it existed on board different ships:—"the typhoid stage of yellow fever;" the fever was of "the nature of *typhus icterodes;*" it was of "a typhoid character," &c. The deeply altered state of the circulating fluid is alluded to in various passages. For example, we read in one that "the blood, when drawn, is sometimes described as being very dark-coloured, and, when allowed to stand, was loose in its texture, neither showing a buffy coat, nor separating into serum and crassamentum." Many similar statements are made in other parts of the volume.

We need scarcely say that not the slightest countenance is given by anything which occurs in Dr. Bryson's Report to the doctrine of the yellow fever being a "nova pestis;" an idea originally suggested by Chisholm, and still, most strangely, believed in by Sir William Pym.

We have seen that our author recognises the occasional and contingent infectiousness of the disease, by direct effluvia from the body of the sick. No warrant, however, is to be found in his work for the belief that it was ever conveyed or transmitted by *fomites*. The only instance in which reference is made to this matter, occurs in the description of the outbreak of the pestilence at Ascension in 1838, soon after the arrival of the "Ætna," the "Forester," and the "Bonetta," as mentioned in a preceding page. It was asserted by certain of the inhabitants of the island, that the infection had been introduced by the clothes of the deceased officers having been sold to some of the people on shore. The following statement will remind the reader of Dr. McWilliam's evidence about the washerwomen of Boa Vista, as recorded in our last number:—

"Sergeant Warren most unquestionably made the most extensive purchases, and his wife took in washing; they both certainly died of the disease, but neither of them was attacked until a late period of the sickness, and both had been attending day and night in the crowded houses of their sick neighbours. Mrs. Scarisbrook was also an extensive purchaser; she was, however, the very last woman attacked, and her husband entirely escaped the disease. George Downes purchased some wearing apparel, which he took to his house, where he had a wife and three children; he has worn a thick watch coat that belonged to the late Lieutenant ——— (who died of fever) on his night watch ever since, yet neither he nor any of his family have been attacked. They probably owe their immunity simply and solely to their house being situated at a considerable distance to the windward of the squares, and at an elevation of several hundred feet. In short, many who were purchasers escaped the disease, while others who did not make any purchases were attacked." P. 133.

Had our space permitted, we should gladly have followed Dr. Bryson in his excellent remarks on the important question of the Treatment of

* However true this remark may be, as applied to the disease when prevailing *epidemically*, it is always to be remembered that, in all seasons, the ordinary coast fever will sometimes put on all the phenomena of the worst malignancy, when the men have been allowed to sleep on shore, and have been committing great excesses. These occasional cases, however, never exhibit any tendency to spread.

Yellow Fever. We very strongly recommend every tropical practitioner to study them with attention. It is quite obvious that no inconsiderable amount of mischief has, not unfrequently, been done by the rash and injudicious adoption of over-active measures—the heroic method of treatment, as it has been absurdly termed—in a true *blood-disease*, such as the African pestilence unquestionably is. Does not the following statement read a useful lesson to the ultraist practitioner? At the time when the "Eden" was suffering most severely from the fever, and her medical officer was one of the victims, "the office of surgeon (there being no assistant) was then assumed by the captain, whose extensive knowledge and long experience of the African climate and diseases, rendered him peculiarly fitted to perform its duties. His treatment of this formidable disease, it is stated, was simple, but more successful than any that had hitherto been adopted. Having witnessed the frequent and fatal result of 'energetic treatment,' he had imbibed a kind of horror of bleeding, and, at the same time, a predilection in favour of mild measures, probably from observing the greater success that attended the simple means employed by the natives and resident Europeans. The abstraction of blood did not therefore form any part of the treatment. He commenced with some brisk purgative, and after its operation, patiently waited for a remission of the symptoms, when he exhibited quinine, and continued its use, until the patient got well; omitting to give it, however, if a paroxysm of fever intervened. When diarrhœa supervened during convalescence, which was not unusual, he gave calomel until ptyalism was fully established; after which the patient generally recovered rapidly."

Dr. Bryson dwells with marked emphasis on the pernicious results that have, in many instances, flowed from the too common practice of pushing the mercurial treatment to a most extravagant length. Even when salivation has been induced, and the system has therefore been brought under the influence of the medicine, no benefit has followed.

There seems to be no difference of opinion as to the propriety, nay the necessity, of administering the bark or quinine freely, whenever there is evidence of the accession of the apyretic stage. [Does not this circumstance alone testify most strongly as to the real *nature* of the disease?] But, alas! in very many cases, there is no distinct cessation, scarcely an abatement, of the pyrexial symptoms, and all medication is utterly profitless. Hence the experienced naval surgeon will often have his mind much more intent upon precautionary and prophylactic, than upon (what are called) curative, measures; for he knows well that, while much may frequently be done, by the timely adoption of appropriate hygienic means, for the prevention and arrest of malignant fever on board a ship, too often all his professional skill is of little or no avail when the enemy has once shown itself among the crew. And here we must not omit to mention that several instances are recorded, in the Report now before us, of fever, if not arising directly from, at least being strikingly aggravated both in intensity and frequency by, causes *within* the vessel itself. The reader has only to refer to pages 224, 228, 229, and 230, to be convinced of this. It will be useful to notice a judicious caution, which Dr. Bryson gives to naval officers, touching this subject:—

"It would be well to avoid, under all ordinary circumstances, attempting to clear

out a vessel on the spot where the disease originated, more particularly if there be reason to suppose it has arisen from a foul state of the holds; for by opening and disturbing the various matters contained in them, the cause must necessarily be let loose upon the men with increased force, while the latter, in a state of fear and despondency, being aware of the danger, are rendered more obnoxious to its influence, and brought, by the nature of their duties, more immediately within its sphere of action. It will be time enough, after the entire cessation of the disease, when by a change of climate and diet the general health of the ship's company has become invigorated, and when confidence has been restored, to commence the work of expurgation in the vessel. It is by no means uncommon for an epidemic to become aggravated by opening up the holds of a ship within the tropics." P. 229.

In concluding our notice of this interesting Report, we would earnestly urge upon all young surgeons of our ships of war the great importance of their keeping an *exact* and *regular* account of the health of the crew, and of the origin, progress and decline of fevers and other constitutional diseases on board, with remarks on the influence of all causes, whether these be *ab extra* or *ab intra*, whether connected with the nature of the climate, the season of the year, the character of the men's occupations, or in their food or the state of the ship itself, &c. &c. To be of any decided utility, such observations must be continued not for a few weeks or months only, but for several years; for it is in this manner alone that we can ever reasonably hope to understand something more than we yet do of the true history of certain epidemic diseases;—which, be it remembered, are only of occasional and of unforeseen occurrence, and not of an annual or constant prevalence. We say "unforeseen," because, as yet, there are scarcely any data to enable a medical man to form even a rational conjecture on the subject. But is it so utterly inconsistent with our knowledge of the operations of nature in respect to climatorial and other atmospheric conditions, that we must never entertain the hope that a discovery may yet be made by some carefully-observing and deep-reflecting medical philosopher to enable us to anticipate, in a measure at least, the recurrence of those fearful visitations of wide-spread disease, to which the term "pestilential" is usually applied? We think not. At all events, nothing can even be expected to be done in this direction, unless there has been previously accumulated a large store of accurate facts and observations, perseveringly registered during a succession of many years. Hitherto this has been very imperfectly done; and the humiliating reflection abides with us, that we are just about as ignorant of almost everything appertaining to the history of Yellow Fever as our forefathers were.

A TREATISE ON THE PHYSICAL CAUSE OF THE DEATH OF CHRIST, AND ITS RELATION TO THE PRINCIPLES AND PRACTICE OF CHRISTIANITY. By *William Stroud*, M.D. Pp. 496. Hamilton and Adams: London, 1847.

THIS is a remarkable work—remarkable alike for the subject of which it treats, and for the admirable manner in which it is written. It displays a great deal of professional research, and, at the same time, no ordinary scholarship in biblical as well as in classical literature. Everywhere it breathes a spirit of pure and elevated religious feeling; the main object and desire of the writer being evidently the simple discovery of Truth, and its practical application to the welfare of himself and others. He has sought to do good rather than to acquire fame, to improve and edify the hearts of his readers even more than to enlighten and inform their understandings. His volume is evidently the fruit of long and patient study, and of deep and solemn meditation, such as the importance of its theme demanded. Dr. Stroud has read much, but he has thought more. While he has, with most praiseworthy diligence, perused the writings of very numerous authors on most of the topics connected with his argument, he has always kept his mind unfettered by any mere human authority; and thus he never fails to exercise an independent judgment of his own. His chief study has been the Holy Scriptures themselves; and to them, and to them alone, he appeals as the source of all divine instruction. So much for the general tone and character of the present work. And here it may be necessary to guard the reader from supposing that it is more curious than instructive, more likely to attract from the singularity of its views than convince by the soundness of its reasonings, or edify by the piety of its reflections. The title of the book may possibly lead some readers to imagine that it deals in speculations that are, to say the least of them, profitless. But this is not the case. It has been no mere curiosity of learning that prompted the author to engage in the enquiry, no idle exercise of his mind, no love of notoriety or distinction. Dr. Stroud has had a much higher and more worthy motive to stimulate him at first, and to direct him throughout his labours. And has he not found his reward? We feel confident that he has. His labour has not been in vain. He has anxiously sought for the truth; and a striking development of it has been manifested to him. The result of his researches has been, if not the discovery, at least the happy elucidation of certain points in the sacred narrative of our Lord's sufferings which were, before, imperfectly understood, and the right appreciation of which serves to throw very considerable light upon various portions of Holy Writ. This was the chief recompense he sought for: and, trifling though it may seem in the eyes of the multitude, it has a power of inward enjoyment and self-satisfaction they are little aware of.

The chief object of the work is stated by Dr. Stroud himself to be—

"To demonstrate an important physical fact connected with the death of Christ, and to point out its relation to the principles and practice of Christianity; but, al-

though the subjects discussed and the conclusions deduced from them are, it is hoped, of no inconsiderable value in a devotional point of view, the treatise itself is rather argumentative than sentimental, and more concerned with the foundation of evangelical religion than with its superstructure. The fact is not indeed now announced for the first time, having been more or less correctly anticipated by several pious and excellent writers during the last century; but, as in matters of such solemn import conjecture and probability are not a sufficient ground for conviction, the author has laboured to supply a demonstration of the fact, which he trusts will be found both new and satisfactory. He has accordingly been careful not to assume anything which is not generally acknowledged; and has supported every point of the argument with proofs and evidences so combined, as apparently to leave no other alternative than that which is here maintained. Should the attempt have been successful, it will furnish a fresh proof of the value of inductive reasoning; which, like a sounding line let down into the ocean of time, has thus, from the depth of eighteen hundred years, brought up to the surface a pearl of great price." *Preface*, p. vi.

As the argument and its illustrations are almost entirely of a Physiological nature, and as medical men must therefore be better able to estimate their value and their force than others, we can have no hesitation in inviting the attention of our readers to the consideration of a volume that, in our judgment, is so creditable to its author, not less as an enlightened physician than as a sincere Christian. The reasoning indeed is mainly physiological, but its application is essentially spiritual; the data on which it rests are drawn from medical science, but the great lesson which the whole is designed to teach is to impress the mind with a deeper and a more devout sense of that divine love that moved the Saviour to endure the mysterious agonies of the garden and of the cross, in working out the Redemption of the human race. As a matter of course, it is the medical argument that will most exclusively engage our attention. And here we would remark that, whatever opinion may be formed of the reasonings advanced in the volume before us, and whether its conclusions be adopted or not, no one, we feel assured, will rise from its perusal without having the utmost esteem for its pious and talented author. In his hands, Medicine becomes the handmaid of Religion; the discoveries of physical Science are consecrated by being laid on the altar of heavenly Truth. And is not the example worthy of imitation? Are medical men always to shrink from even so much as making any allusion to the blessed pages of the Bible? Is every branch of learning to be studiously examined and discussed, and may not a word be ever said of one great end and object of all instruction, viz. the elevation of the heart, the spiritualising of the affection? Might it not be well if our thoughts and feelings were, at times, withdrawn from the incessant attention that is paid in the present age to the merely material portion of our professional pursuits, and occasionally directed to the contemplation of subjects which interest us, in common with our fellow-creatures, as sojourners in a world where everything proclaims that we are but as links of a mighty chain whose ends are hid in impenetrable darkness, and that the great object of our lives is for purposes, the fulfilment of which reaches on to another state of existence?

No set of men, it must be obvious, have so many advantages, derivable from ir education and from the nature of their engagements, in following out tudy of many questions appertaining to the moral happiness and welfare

of mankind, as the members of the medical profession. To them, more frequently than to others, it is given to observe the mutual relationship between mind and matter, between spirit and body; and to witness the effects of this relationship either for good or for evil, for happiness or for misery to the individual. They have peculiar facilities of watching, if not of explaining, the reciprocal actings and reactings of the corporeal and psychical portions of our compound nature; for their aid is as often invoked to "minister to a mind diseased," as to relieve the sufferings of the body, and they are frequently called upon to soothe "the troubles of the brain," though they cannot "cleanse the foul bosom of that perilous stuff which weighs upon the heart."

May the following remarks be made profitable to some readers by awakening their attention, more than perhaps it has yet been, to a class of subjects that well deserve to be thought upon and pondered by all! The first part of our observations is intended to illustrate the effects of certain strong passions on the bodily frame; in the second, we shall point out the application which Dr. Stroud has made of this enquiry, to explain what he terms "the physical cause" of the death of the Redeemer.

The influence of intense mental emotions, whether of an exciting or of a depressing nature, on the actions of the Heart and blood-vessels and on the vital fluid that permeates through them, has been recognised by medical writers in all ages. A few authorities will suffice.

"It is observed by Baron Haller, the father of modern physiology, that excessive grief occasions palpitation, and sometimes sudden death; that the corporeal effects of anger and terror are nearly alike, including increased strength, and violent motions both in the heart and throughout the body, and producing *bloody sweats*, and other kinds of hæmorrhage.*—'Anger'—says Senac—'has in certain cases torn the fibres of the heart, and even opened the ventricles. It is not therefore extraordinary that it should be followed by palpitation, and accordingly, various physicians have observed such a result. . . . . But fear and terror are not less powerful causes, especially when they seize suddenly. In that case the nerves act with violence on the heart, and derange the order of its movements. The blood is at the same time propelled in these passions by a general shock, or commotion of all parts of the body: it therefore necessarily accumulates in the two trunks of the venæ cavæ, rushes into the auricles, and overcharges them, as well as the ventricles. Here then are two causes, one the consequence of the other, which, as is proved by numerous examples, produce palpitation. Dilatations are, as we have already stated, frequent results of fits of passion. Grief and sadness do not act so suddenly, nor with equal force; but as we have said, these secret and silent passions induce similar disorder.'†—'If any one'—remarks Corvisart—'can seriously deny, or even doubt the fatal physical influence of the passions on the heart, let it suffice him to know that a fit of anger may produce rupture of the heart, and cause sudden death. . . . . Complete rupture of the heart has rarely been observed in the sound state of this organ: some examples may however be cited of this lesion, in consequence of a violent effort, a fit of anger, an epileptic paroxysm, &c. . . . . But of all the causes capable of producing organic diseases in general, and more especially those of the heart, the most powerful beyond dispute are mental affections. . . . . No mental affection can indeed be experienced without the movement of the heart being either augmented, accelerated, retarded, weakened, or

* "Haller, Element. Physiog. Corp. Human., vol. v., pp. 50, 583, 586, 587."
† "Senac, Traité du Cœur, vol. ii., p. 515."

disturbed, without its force in fact being increased, enfeebled, or almost annihilated. Pleasure, pain, fear, anger, in short all the powerful passions, cause the heart to palpitate, to beat more or less frequently, strongly, slowly, or regularly, or to suspend its action momentarily, sometimes even mortally."* P. 78.

The effects of Terror on the organs of circulation, &c., are thus described by Dr. Alex. Chrichton :† "The heart is thrown into greater and more violent action than usual, but the arterial system, so far from corresponding with it in a general sense, is either rendered torpid at its extremities, or else is affected with a spasm; a sudden paleness spreads itself over the countenance, the lips lose the coral tint, and the whole body of the man seems to shrink into a smaller compass, a tremor agitates his whole frame, and he feels as if he had suffered a great diminution of strength. . . . . It happens now and then, when the whole play of the mental faculties is as it were destroyed by the impression of the dreadful object, and no possibility of escape appears, that, volition being then without a stimulus, a person drops down on the earth, as if suddenly bereft of all his animal powers."

The same writer has well depicted the effects of Grief and Sorrow :—

"The general corporeal effect of all modifications of grief and sorrow is a torpor in every irritable part, especially in the circulating and absorbent system: hence the paleness of the countenance, the coldness of the extremities, the contraction and shrinking of the skin and general surface of the body, the smallness and slowness of the pulse, the want of appetite, the deficiency of muscular force, and the sense of general languor which overspreads the whole frame. As the action of the extreme branches of the arterial system is greatly diminished, the heart, and aorta and its larger vessels, and the whole system of the pulmonary artery become loaded and distended with blood. The painful sense of fulness which this occasions gives rise to a common expression, which is in some degree descriptive of what really exists. In sorrow the heart is said to be *full*, and in deep sorrow it is often said to be *like to burst*. A sense of oppression and anxiety, a laborious and slow respiration, and the remarkable phenomena of sobbing and sighing, naturally arise from this state of torpor and retarded circulation." P. 82.

It is rather the suddenness and violence, than the nature or kind, of the mental emotion that is apt to produce the consequences now described. Death has been known to be produced by excess of unexpected joy, as well as by overpowering grief or terror. We read that Sophocles died from elation, at his triumph in a contest of honour. Aulus Gellius tells us of a man, who suddenly expired on hearing of his three sons being crowned as victors on the same day; and Livy mentions the case of an aged matron who, having been in the depth of distress from the tidings of her son's being slain in battle, died in his arms from the excess of joy at his safe return. In these and such-like cases, the death is, as our author observes, probably owing to some sudden lesion, functional or organic, of the heart. Zimmerman says that Philip V. died suddenly on being told that the Spaniards had been defeated; on opening his body the heart was found ruptured.‡

---

* "Corvisart, Sur les Maladies du Cœur, &c.; Discours Preliminaire, p. xli. pp. 259, 369, 370."

† On Mental Derangement. Vol. II., pp. 119—121, &c.

‡ On Experience in Physic. Vol. II., p. 268.

But, before we examine this subject more minutely, let us turn our attention to that rare, certainly, but nevertheless perfectly well authenticated, effect of strong mental emotion upon the secretion of the skin; we allude to the remarkable phenomenon of *bloody sweat*. The following instances are adduced by Dr. Stroud, in the way of illustration:—

"The eminent French historian De Thou mentions the case of—'an Italian officer who commanded at Monte-Maro, a fortress of Piedmont, during the warfare in 1552, between Henry II. of France and the Emperor Charles V. This officer having been treacherously seized by order of the hostile general, and threatened with public execution unless he surrendered the place, was so agitated at the prospect of an ignominious death, that he sweated blood from every part of his body.'—The same writer relates a similar occurrence in the person of a young Florentine at Rome, unjustly put to death by order of Pope Sixtus V. in the beginning of his reign, and concludes the narrative as follows: 'When the youth was led forth to execution, he excited the commiseration of many, and through excess of grief, was observed to shed bloody tears, and to discharge blood instead of sweat from his whole body; a circumstance which many regarded as a certain proof that nature condemned the severity of a sentence so cruelly hastened, and invoked vengeance against the magistrate himself, as therein guilty of murder.' Amongst several other examples given in the Ephemerides, of bloody tears and bloody sweat occasioned by extreme fear, more especially the fear of death, may be mentioned that of—'a young boy who, having taken part in a crime for which two of his elder brothers were hanged, was exposed to public view under the gallows on which they were executed, and was thereupon observed to sweat blood from his whole body.'—In his Commentaries on the four Gospels, Maldonato refers to—'a robust and healthy man at Paris who, on hearing the sentence of death passed on him, was covered with a bloody sweat.'—Zacchias mentions a young man who was similarly affected on being condemned to the flames. Schenck cites from a martyrology the case of—'a nun who fell into the hands of soldiers; and, on seeing herself encompassed with swords and daggers threatening instant death, was so terrified and agitated, that she discharged blood from every part of her body, and died of hæmorrhage in the sight of her assailants;'*—and Tissot reports from a respectable journal that of—'a sailor who was so alarmed by a storm, that through fear he fell down, and his face sweated blood, which during the whole continuance of the storm returned like ordinary sweat, as fast as it was wiped away,'"† P. 88.

Haller, in more than one passage of his physiological writings, has alluded to the exudation of a bloody fluid from the cutaneous vessels. "Mental emotions," says he in one place, "have been known to produce extraordinary changes in the secretions, so that blood and bile have been forced out from the vessels of the skin."‡ Perhaps one of the most striking instances on record of the oozing of a sanguineous fluid from the surface

---

* "Ephemerid. Acad. Natur. Curios. Ann. 2, p. 34;—Dec. ii. Ann. 10, p. 354;—Dec. iii. Ann. 1, Append. pp. 124, 125;—Ann. 7 and 8, Append. p. 124;—Ibid. edit. 2da. vol. i. p. 84:—vol. viii. p. 184:—Thuanus, Hist. sui Temp. vol. i. p. 373; vol. iv. p. 300; Joannes Maldonatus, Comment. in quatuor Evangelist. p. 601;—Paulus Zacchias, Quæstiones Medico-legales, lib. iii. p. 154;—Joannes Schenck à Grafenberg, Observ. Medic. &c. lib. iii. p. 458."

† Tissot, Traité des Nerfs, &c. pp. 279, 280.

‡ Elementa Physiologiæ Corp. Hum. vol. v., p. 50.—Primæ Lineæ Physiologiæ, p. 126.

occurred in the person of Charles IX. of France, who died in the 25th year of his age. We read in De Mezeray, the historian, that, "after the vigour of his youth and the energy of his courage had long struggled against his disease, he was at length reduced by it to his bed at the castle of Vincennes, about the 8th of May (1574). During the last two weeks of his life, his constitution made strange efforts. He was affected with spasms and convulsions of extreme violence. He tossed and agitated himself continually, and his blood gushed from all the outlets of his body, *even from the pores of his skin;* so that, on one occasion, he was found bathed in a bloody sweat." Voltaire, in alluding to this narrative, adds:—"This malady, of which there are some examples, is the result either of excessive fear, furious rage, or of a violent and melancholic temperament."* Besides this, and other cases already mentioned, Dr. Stroud quotes the following one, related in the French "Transactions Medicales" from a former number of this Journal. It stands thus:—

"A young woman, aged twenty-one years, irregular in menstruation, and of indolent habits, and obstinate temper, had been much irritated by some reflections made by her parents, on account of her abjuring the Protestant religion. She left her paternal roof, and after wandering about for some time, took up her residence in an hospital. She was then suffering violent attacks of hysteria, attended with general convulsions, and exquisite sensibility in the pubic and hypogastric regions. After paroxysms of hysteria, which sometimes lasted twenty-four or thirty-six hours, this female fell into a kind of ecstacy, in which she lay with her eyes fixed, sensibility and motion suspended. Sometimes she muttered a prayer, but the most remarkable phenomenon was an exudation of blood from the cheeks and the epigastrium in the form of perspiration. The blood exuded in drops, and tinged the linen. The cutaneous surface appeared injected in those parts whence the blood escaped, being red, and showing a network of arborescent vessels. This bloody perspiration took place whenever the hysteric paroxysm lasted a considerable time. This state continued for three months, and ultimately gave way, it is said, to local bleeding, ... together with strong revulsive measures."† P. 382.

This case will probably call to the recollection of many of our readers the strange stories about certain "estaticas," who were, a few years ago, (and perhaps still are) exhibited in some nunnery in Italy to the edification of the credulous. We gave a short account of the phenomena alleged to be witnessed in these deluded young women, in our number for October 1845. It is worthy of remark that they were all subject to violent convulsive attacks, and were evidently labouring, at the same time, under the influence of intense mental excitement. Their malady was doubtless one of the most distressing forms of Hysterical disease, just as in the instance of the girl whose case has been related above.

We now proceed to show, as already intimated, that intense mental emotion, more especially when it is of an exciting nature, may suddenly induce a mortal lesion of the Heart, and that this lesion has been, in some cases at least, an actual rupture of this vital organ. The following extracts

* De Mezeray, Histoire de France, vol. iii.—Voltaire, Œuvres Completes, vol. xviii.

† Medico-Chirurgical Review for Oct. 1831, p. 496.

from the writings of two distinguished writers of our own day, may be appropriately quoted upon this point:—

"'It is generally in the left ventricle,' remarks Dr. Hope, 'that the rupture [of the heart] takes place; a circumstance which at first appears remarkable, since this ventricle is the stronger, but for the same reason it contracts more energetically, and . . . it is only strong muscles which undergo rupture from the energy of their own contraction. Hence rupture of the auricles is much more rare than that of the ventricles. The exciting causes of rupture are generally considerable efforts, paroxysms of passion, external violence, as falls, &c. . . . . Rupture of the heart or great vessels into the pericardium is not always immediately fatal, as a solid coagulum or a fibrinous concretion has in several instances been known to arrest the hemorrhage for a few hours. Of ten cases mentioned by Bayle, eight died instantaneously, one in about two hours, and another in fourteen. Amongst the causes of rupture of the heart, Dr. Copland enumerates,—'violent mental emotions especially anger, fright, terror, unexpected disappointment, distressing intelligence abruptly communicated, anxiety, &c., sudden and violent muscular efforts, and laborious or prolonged physical exertions of any kind, particularly in constrained positions. . . . . In some cases,'—he observes,—'inexpressible anxiety and pain have been felt in the præcordia and epigastrium, with cold extremities, and cramps, shortly before dissolution. In the majority rupture has produced instant death, but in some this has not been the case. . . . . In most of the cases in which the rupture is preceded by violent pain, M. Ollivier thinks that it is produced gradually from the successive laceration of several layers or fasciculi of muscular fibres, and that the pericardium becomes only gradually distended by the effused blood. Where the laceration and aperture are at once large, a copious effusion instantly occurs, fills the pericardium, and abolishes the contractions of the organ."* P. 90.

It must be confessed, indeed, that there are not many instances on record in which sudden death from mental emotion has been *proved*—by dissection, we mean—to have been owing to Rupture of the Heart. In many such cases, no *post-mortem* examination has been made. Nevertheless, it may be reasonably conjectured that this mortal lesion is not so extremely rare as has been generally supposed. Dr. Stroud thinks it highly probable that the death of Palmer, about the close of the last century, while enacting the part of the Stranger, in the play of that name, on the boards of the Liverpool theatre, was from this cause. The circumstances were these. For some time previously, he had been labouring under the most profound grief, in consequence of the death of his wife and a favourite son, and he had struggled to bear up against it, although he felt that his mental sufferings would very shortly bring him to the grave. "On the morning of the day, he was much dejected; but exerted himself with great effect in the first and second acts of the play. In the third act, he showed evident marks of depression; and in the fourth, when about to reply to the question of Baron Steinfort relative to his children, he appeared unusually agitated. He endeavoured to proceed, but his feelings overcame him. The hand of death arrested his progress, and he fell on his back, heaved a convulsive sigh, and instantly expired without a groan. . . . . Dr. Mitchell and Dr. Corry gave it as their opinion,

* "Dr. Hope, On the Diseases of the Heart and great Vessels, pp. 198, 199;—Dr. Copland, Dict. of Practical Medicine, Part v., p. 224."

that he certainly died of a *broken heart*, in consequence of the family afflictions which he had recently experienced."

The expression here used, it is very reasonably imagined, may have been literally, as well as metaphorically, true. We shall presently adduce several examples in corroboration of this idea. Meanwhile, it may not be uninteresting to remark, *en passant*, that the language of every nation has recognised, so to speak, its strict physiological truth. Hence, as well observed by our author, "although the term—*broken heart*—is not always used literally, but often in a figurative sense to denote intense or perhaps mortal sorrow, it was no doubt originally derived from the actual fact, either accidentally observed, or sagaciously conjectured by poets and moralists, habitually engaged in the study of the human passions and of their influence on the bodily frame." Shakespeare, with that intuitive penetration of genius which was so pre-eminent in him beyond all other writers, has repeatedly alluded to this cause of sudden death from agony of mind. An instance or two must suffice:

> "Give sorrow words: the grief that does not speak
> Whispers the o'erfraught heart, and bids it break;

and by a bold poetical license, Marc Antony is made to represent the death of Julius Cæsar as occasioned, not by the daggers of his conspirators, but by his anguish of soul on seeing his friend Brutus in their number:

> "Ingratitude, more strong than traitor's arms,
> Quite vanquished him: then burst his mighty heart,
> And, in his mantle muffling up his face,
> Even at the base of Pompey's statue,
> Which all the while ran blood, great Cæsar fell."

For other passages of a similar bearing, the curious reader must consult Dr. Stroud's work: proceed we to more matter-of-fact details.

Although actual Rupture of the Heart is unquestionably a rare occurrence from mental emotion or from any cause whatsoever,* it is not a difficult thing to prove, from the concurrent testimony of numerous writers, the direct influence of strong passion in inducing less serious lesions of this vital organ. Harvey relates the case of a man who, under the long-continued working of smothered indignation and revenge, fell into a scorbutic and hæmorrhagic state, attended with extreme oppression and pain of the chest, owing to an immense enlargement of the heart and principal arteries, which was entirely the result (it was believed) of mental emotion. "Had this emotion," Dr. Stroud remarks, "been more intense, it is easy to conceive that, instead of a slight oozing of blood from the cutaneous vessels, and a mere enlargement of the central organs of circulation, the result would have been bloody sweat and rupture of the heart."

Dionis has related an interesting case of an officer affected with serious disease of the heart, attributed by the patient himself to the strong efforts

---

* It has been asserted by some writers that Rupture of the Heart never takes place in a normal state of the organ; but this statement has been denied by Portal and Rostan, in whose writings are recorded numerous observations which prove the contrary, and which serve to show that rupture of the left ventricle may take place without any coexisting alteration of its muscular tissue.

which, several years before, he had used to suppress a violent fit of anger. On dissection, the right auricle was found as big as the head of a new-born child, and contained nearly a pint and a half of blood, the greater part of which was coagulated. "In like manner, Tissot quotes from Viridet the case of a merchant, who, in consequence of violent grief, was seized with constriction and severe pain of heart, terminating in death. On inspection of the body,—'the heart was found twice as large as it should have been, and the whole of its left cavity filled with blood strongly coagulated.' —In another merchant, aged sixty-two years, who suddenly died of grief, Bonet states that the heart and lungs were found greatly distended with blood which, in the right ventricle, was almost entirely coagulated. Of the separation of the blood in some of these cases into its constituents, the same author gives two examples.—'A paralytic orphan girl, seventeen years of age, suddenly died of suffocation without any obvious cause. On dissecting the body, I found the heart of twice the usual size, its auricles very large, and like the veins and arteries, much distended with water and black clotted blood..... In a soldier who suddenly died after long-continued grief, whilst all the other viscera were healthy, the pericardium was found to contain not only water, but also much coagulated blood.'"*

In reference to this last case, Morgagni has remarked:—"Although you will see it repeated in a note that the heart was loaded both with *blood and water*, it is by no means necessary that you should believe this water to have been any other than the serum of the blood separated from the coagulated part, as not unfrequently happens to a considerable amount."

This remark of Morgagni leads us, by a natural transition, to the consideration of a topic connected with rupture of the heart, which will be found to have a most important bearing on the general argument of our present enquiry. It deserves, therefore, to be examined at some length.

The separation of the blood into its solid and fluent constituent parts seldom or never takes place completely while it remains within the heart and the vessels, unless, indeed, their structure or conformation be seriously altered, as in cases of aneurismatic enlargement, &c. It is commonly either wholly liquid or wholly solid, or a portion of liquid blood is intermixed with pale or dark-coloured coagula; but its distinct or complete separation into serum and crassamentum—or, to use popular language, into *blood* and *water*—is scarcely ever met with. The attention of anatomists has hitherto been little directed to this subject. Two gentlemen, however, distinguished alike for the extent of their enquiries and the accuracy of their observations, have very fortunately, within the last few years, published some remarks upon it; we allude to Dr. John Davy and Mr. Paget. The results of their labours may be briefly stated to be as follow. Of 164 cases in which the state of the blood was noted by the former of these gentlemen, it was found "coagulated and containing fibrinous concretions, in 105;—coagulated and broken up, as if by the contractions of the heart,

* Harvæus, Opera, pp. 127, 128;—Dionis, Anatomy of Human Bodies, pp. 270, 445–451;—Tissot, Traité des Nerfs, &c. p. 361;—Bonetus, Sepulchretum, vol. i. pp. 585, 887, 899.

feebly continued for some time after apparent death, in 17;—liquid, in 14;—in the state of soft coagulum, or merely grumous and without fibrinous concretions, in 12;—partly liquid and partly coagulated, in 9;—and wholly or nearly deficient in the heart, in 6. In one instance only there were fibrinous concretions without cruor, that is, without liquid blood or bloody serum. Dr. D. next gives a tabular account of 35 cases of post-mortem examinations, made in the General Hospital at Fort-Pitt, Chatham, from January to Sept. 1838. The various conditions in which the blood was found are still more minutely described in these cases, than in the preceding ones; but in none of either set does clear serum appear to have been discovered, except in one solitary instance, and under very peculiar circumstances; namely, in No. 16, a case of phthisis, wherein, says the author,—'a mass of fibrine in the right ventricle contained a collection of transparent serum. The mass was firmest externally. There was some crassamentum, fibrinous concretions, and a good deal of cruor in the right cavities of the heart. Two hours after examination, the cruor was found jellied. After twenty-four hours the coagulum had contracted, and serum had separated. When broken up and agitated, some air was given off.'"*

The testimony of Mr. Paget is still more precise:—

"In all cases it must be remembered that the coagulation which takes place in the body is much slower than that which ensues in blood drawn from it, either during life or after death; so that a quantity of uncoloured fibrine is found in the heart and uppermost vessels of the dead body in many cases, in which it is most probable that, had the blood been drawn during life, it would not have presented a buffy coat. In the majority of cases, the blood does not coagulate in the body for the first four hours after its rest has commenced. In many it remains fluid for six, eight or more hours, and yet coagulates within a few minutes of its being let out of the vessels. But, as this greater slowness of coagulation is common to all, it is not material in a comparison of the blood of the dead with that of the living."† P. 404.

Mr. Paget, in a letter addressed to our author subsequently to the publication of these remarks, observes:—"I have never found clear serum, such as I could suppose to be separated from the blood in its coagulation, collecting in any part of the body after death," *i. e.* as long as the blood remained within its natural receptacles. The following passage from Dr. Carpenter's excellent work on Human Physiology contains an apposite illustration of this point:—

"Instances occasionally present themselves in which the blood does not coagulate after death, and in most of these there has been some sudden and violent shock to the nervous system, which has destroyed the vitality of solids and fluids alike. This is generally the case in men and animals killed by lightning, or by strong electric shocks, and in those poisoned by prussic acid, or whose life has been destroyed by a blow on the epigastrium. It has also been observed in some instances of rupture of the heart, or of a large aneurism near it, and a very interesting phenomenon then not unfrequently presents itself; the coagulation of the blood which has been effused into the pericardium (the effusion having taken

* Researches Physiological and anatomical. Vol ii., pp. 110-213.
† London Medical Gazette, for 1840. Vol. i. p 618.

place during the last moments of life,) whilst that in the vessels has remained fluid."* P. 150.

The various statements now made amply warrant the conclusion that the separation of the blood into serum and crassamentum seldom if ever takes place completely, as long as it remains in its natural receptacles, these being at the time in a normal state.

Dr. Stroud suspects, and with reason, that, in several cases of Rupture of the Heart on record, this separation had really taken place, although it is not specified in the account given of the appearances found on dissection. After alluding to three cases, related by Abercrombie in the first volume of the *Transactions of the Medico-Chirurgical Society of Edinburgh*, he quotes one that is recorded by Dr. Thurnam in the *Medical Gazette* for 1838. This was "'a case of rupture of the heart from external violence, but without any penetrating wound. The pericardium contained several ounces of serum and coagulated blood. There was a considerable rupture of the right auricle, and a smaller one at the apex of the heart.' The same gentleman mentions an instance 'of spontaneous rupture of the right auricle and ventricle, attended with great and general softening. The pericardium was filled with liquid blood,'—coagulation having apparently been prevented by the feebleness of the heart's action, which is usually attended with a corresponding condition of the blood."

Dr. Elliotson† has related the case of a female, who died suddenly with severe pain in the region of the heart. The pericardium was found distended with clear serum, and a very large coagulum: there was a rupture of the aorta near its commencement. In the case related by Dr. Fischer, and which was occasioned, it is said, by the slow operation of continued grief, it is stated in the account of the dissection:—"On puncturing the pericardium, which had the appearance of being distended by a substance of a dark blue color, a quantity of reddish fluid escaped, and afterwards florid blood to the amount of two or three pounds. The membrane was then slit up, and the heart seen surrounded by a coagulum more than three pounds in weight. This having been cleared away, a rupture was discovered in the aortic [left] ventricle, which extended upwards from the apex, about an inch and a half on the external surface. The internal wound was found but about half-an-inch in length, and its lips [were] at least as wide again asunder as those of the external breach."‡

Dr. Townsend§ of New York mentions the case of a young female, who died suddenly in consequence, it is believed, of anguish of mind. "On dissection, the sac of the pericardium was found filled with about ten

---

* The contrast between the state of the blood within the cavities of the heart, and of that which was extravasated (into the pleura?), is well marked in one of Morgagni's cases. The quantity of limpid serum was so great that, on first opening the chest, it was believed to be a case of dropsy. But, on further examination, a large quantity of coagulated blood was found *infra aquam*, and the real disease was discovered to be an aneurism of the aorta, which had given way.

† Lumleyan Lectures on Diseases of the Heart, p. 30.

‡ London Medical Repository, vol. xi. pp. 422–427; and vol. xii. pp. 164–168.

§ On the Influence of the Passions in the production and Modification of Disease, pp. 51–56.

ounces of coagulated blood, and two of serum. The heart, on all sides covered by it, was of the ordinary volume, but much loaded with fat. At the summit of the aortic [left] ventricle was discovered the breach, from which the effused blood had issued. It was irregularly lacerated, and measured about half-an-inch in diameter."

Dr. Williams, of Southampton, communicated to our author the particulars of a somewhat similar case :—

"R. W., a labourer, aged fifty-six years, had generally enjoyed good health, but for ten years had suffered great despondency of mind, owing to the unfaithfulness of his wife. About six months before his death he was troubled with severe cough, which came on in paroxysms, generally at night and early in the morning, and after a fit of this kind was found one morning dead. A post-mortem examination took place in the presence of Mr. Boulton, surgeon, of Leamington. On opening the chest, the bag of the pericardium appeared much distended with fluid, and was of a dark blue color. On cutting into it, a pint at least of transparent serum issued out, leaving the crassamentum firmly attached to the anterior surface of the heart. On further examination to ascertain the source of this hæmorrhage, we found the left ventricle, from the origin of the aorta downwards to within an inch of the apex, ruptured. The heart appeared in no way disorganised, there was no softness of its walls, the internal membrane was healthy, and so were the valves of each cavity." P. 100.

Reference may also be made to a case recorded by Mr. Adams,* of a man who died with severe cardiac symptoms, after great agony of body and mind. "The pericardium was found distended, and emitted when divided a quantity of serous fluid; but the heart was entirely concealed by an envelope of coagulated blood in three distinct layers, owing to rupture of the left ventricle close to the septum, and nearer the apex than the base of the heart."

In a case of rupture of the right auricle of the heart from violence, recorded by Ludwig, in his *Adversaria*, it is stated:—"The pericardium was so distended by a large quantity of transparent serum and coagulated blood as to push the lungs upwards. The yellowish serum contained in its cavity exceeded half-a-pound." The Commentaries of the Academy of Bologna, for 1757, contain an account of a man who died suddenly. In addition to other lesions observed in the body, a small rupture was found in the left ventricle of the heart, and the pericardium was so distended as to occupy a third part of the cavity of the chest. On opening it, a large quantity of serum was discharged, and two pounds of clotted blood were seen adhering to the bottom.

Besides the cases now quoted, Dr. Stroud refers to, and briefly describes, several others; viz: one related by Mr. Watson in the London Medical Repository for 1814; two related in the London Medical and Physical Journal for May 1822 and April 1826; one in Wheeler's Manchester Chronicle for Nov. 22, 1834; one in the Medico-Chirurgical Review for 1836, (the case of the late Sir David Barry); one in the Edin. Medical and Surgical Journal for January 1843; one in the Dublin Medical Transactions for 1830; and two very interesting cases by Dr. France, in a recent number of the Guy's Hospital Reports. In all these instances, the extra-

* Journal of Morbid Anatomy, Ophthalmic Medicine, &c. Art. v.

vasated fluid is stated to have been distinctly separated into its crassamentum and serum, *blood* and *water*.

Before dismissing the pathological subjects that have been engaging our attention, we may state that it has been asserted that a quantity of blood, or bloody fluid, has been found within the pericardium, even when there was no discoverable rupture of the heart or large blood-vessels. Two such cases are related in the German Ephemerides.

"Both the subjects were robust soldiers who died of excessive joy, in whose bodies no morbid condition was afterwards found, except a large quantity of clotted blood in the pericardium, by which the action of the heart had been suppressed. The latter author ascribes the effect to sudden distension of the exhalants opening on the inner surface of the membrane. This would correspond to the manner in which bloody sweat is produced; but, as the exhalants of the pericardium are very inferior both in size and activity to those of the skin, it is more probable that in such cases the effusion is due either to rupture of some of the nutrient vessels of the heart itself, termed its coronary vessels, or to hæmorrhage from without, penetrating by a minute or circuitous passage into its capsule. Such at least is the opinion of Morgagni, Zecchinelli, and other anatomists. Of blood thus finding its way into the pericardium by a small aperture, which, without great attention, might easily escape notice, the former gives several examples; and in the Ephemerides, Dr. Daniel Fischer mentions the case of a soldier who died suddenly after eating a hearty dinner, and in whose body the only morbid appearance discovered on inspection was,—'the pericardium filled and distended with very fluid and florid blood. The membrane having been divided longitudinally, in order to trace more exactly the source of the hæmorrhage, this was found at the base of the heart, where a branch of the coronary artery had ruptured, and from which blood was still actually flowing.'"* P. 92.

Before proceeding to show the bearing of the facts and reasonings hitherto adduced upon the main subject of our author's argument, we shall here introduce, for a reason that will be readily understood, a few remarks upon that mode of capital punishment that was resorted to in the case of our Lord.

It would seem that Crucifixion was practised among many nations from the remotest antiquity. The earliest instance on record is probably that of the chief baker of Pharaoh: for, although we read in our Bibles that he was *hanged*, Josephus expressly says that he was *crucified;* and we know that these two words are often exchangeable in the Scriptures, the one for the other. Most persons imagine that crucifixion was a Jewish mode of putting to death. Not so. The Jews only crucified the dead bodies of those who had been stoned for blasphemy: hence it was that the "nailing to a tree" was deemed by them as so peculiarly "accursed." Moreover the Mosaic law enjoined that the body, so exposed, should be taken down before the sunset of the day on which the criminal had been slain. It is always to be remembered that Christ was condemned by a Roman tribunal, and was put to death by Roman law, although the guilty instigators

* "Ephemerid. Acad. Natur. Curios. Dec. iii. Ann. 9 and 10,—p. 293;—Ibid. Edit. 2da. vol. v. pp. 141, 142;—Matt. Van. Geuns, De Morte Corporea, &c. p. 591;—Zecchinelli, Sulla Angina del Petto, &c. vol. i. pp. 95, 96;—Thurnam, in Lond. Med. Gazette, 1838, pp. 813–817;—Curling, ibid. pp. 894, 895;—Fitzpatrick, in Lond. Med. Repository, vol. xvii. pp. 295–298.

of the act were those of his own nation. The Jewish Rulers were the accusers, Pilate was the judge, and the soldiers were the executioners. Now crucifixion was in common use among both the Greeks and the Romans, more particularly in the case of their slaves when convicted of a capital offence. It was consequently regarded as the most ignominious and disgraceful of all punishments. Everything was thus combined to render the death of the Saviour accursed in the sight of the people. He suffered as the vilest of malefactors according to the law of the heathen masters of Judea, for alleged *sedition* against the authority of Cæsar; while, at the same time, his being "nailed to the tree" represented and fulfilled, in the eyes of the Jews, the divine malediction that was always associated in their minds with the peculiar punishment for *blasphemy*—the pretext, it will be remembered, on which they sought His life.

Crucifixion appears to have continued in force among the Romans until the time of Constantine, the first Christian Emperor, who abolished it throughout his dominions. "He would not suffer the instrument of our salvation," says Crevier,* "to be dishonoured by any use, not only profane, but capable of making men look upon it with horror. He thought it indecent and irreligious that the cross should be employed for the punishment of the vilest offenders, whilst he himself erected it as a trophy, and esteemed it the noblest ornament of his diadem and military standards."

That death by crucifixion was usually protracted and lingering is not only abundantly testified by history, but is strictly in accordance with what might be anticipated from the very nature of the punishment. It will serve to give the reader a more correct notion of it, if we insert the following particulars respecting the construction of the Cross, as used in ancient times:—

"The cross consisted of a strong upright post, sharpened at the lower end by which it was fixed in the ground, having a short bar or stake projecting from its middle, and a longer transverse beam firmly joined near its top. As the middle bar, although an important appendage, has been almost universally overlooked by modern authors, it will be proper here to insert the account given of it by some of the early fathers of the church, and founded on personal observation. 'The structure of the cross,' says Irenæus, 'has five ends or summits, two in length, two in breadth, and one in the middle, on which the crucified person rests.' Justin Martyr, in like manner, speaks of—'that end projecting from the middle [of the upright post] like a horn, on which crucified persons are seated;' and the language of Tertullian, who wrote a little later, exactly corresponds: 'A part, and indeed a principal part, of the cross is any post which is fixed in an upright position; but to us the entire cross is imputed, including its transverse beam, and the projecting bar which serves as a seat.'† The criminal condemned to this dreadful mode of death, having first been scourged, was compelled to carry the cross on his shoulders to the place of execution, a circumstance which implies that the scourging was not excessively severe, and that the dimensions of the gibbet did not in general much exceed those of the human body. On arriving at the spot, he was stripped of his clothes, and after receiving a cup

---

* History of the Roman Emperors, vol. x. p. 132.

† "Irenæus, Opera, p. 166;—Justinus Martyr, Cum Tryphone Judæo Dialogus, pp. 271, 272;—Tertullianus, Ad Nationes, p. 49; Adversus Judæos, p. 195."

of wine, sometimes medicated with a view to impart firmness or alleviate pain, was speedily nailed to the cross, either before or after its erection. In either case he was made to sit astride on the middle bar; and his limbs having been extended and bound with cords, were finally secured by large iron spikes driven through their extremities, the hands to the transverse beam, and the feet to the upright post." P. 36.

With respect to the degree and usual duration of the sufferings inflicted by this horrible punishment, Dr. Stroud remarks:—

"The bodily sufferings attending this punishment were doubtless great, but, either through ignorance or design, have been much exaggerated. The insertion of the cross into its hole or socket, when the criminal was previously attached to it, did not necessarily produce the violent concussion which has been supposed; and as the body rested on a bar, it did not bear with its own weight on the perforated extremities. At all events, there have been many examples of persons enduring these sufferings with the utmost fortitude, and almost without a complaint, until relieved from them by death. A fact of importance to be known, but which has not been sufficiently regarded, is that crucifixion was a very lingering punishment, and proved fatal not so much by loss of blood, since the wounds in the hands and feet did not lacerate any large vessel, and were nearly closed by the nails which produced them, as by the slow process of nervous irritation and exhaustion. This would of course be liable to variety, depending on differences of age, sex, constitution, and other circumstances; but for persons to live two or more days on the cross was a common occurrence, and there are even instances of some who, having been taken down in time and carefully treated, recovered and survived. In many cases death was partly induced by hunger and thirst, the vicissitudes of heat and cold, or the attacks of ravenous birds and beasts; and in others was designedly accelerated by burning, stoning, suffocation, breaking the bones, or piercing the vital organs."* P. 38.

Numerous instances might be adduced from the writings of Martyrologists in proof of the slowness of death from crucifixion—according to Origen and other early fathers, the sufferer usually survived about two days;—but we prefer to quote the following accounts from two works of recent date:—

"'The capital punishments inflicted in Soudan'—observes Captain Clapperton, writing in 1824,—'are beheading, impaling and crucifixion; the first being reserved for Mahometans, and the other two practised on Pagans. I was told, as a matter of curiosity, that wretches on the cross generally linger three days before death puts an end to their sufferings.'—When describing the punishments used in Madagascar, the Rev. Mr. Ellis remarks,—'In a few cases of great enormity a sort of crucifixion has been resorted to; and in addition to this, burning or roasting at a slow fire, kept at some distance from the sufferer, has completed the horrors of this miserable death. . . . . In the year 1825 a man was condemned to crucifixion who had murdered a female for the sake of stealing her child. He carried the child for sale to the public market, where the infant was recognised, and the murderer detected. He bore his punishment in the most hardened manner, avenging himself by all the violence he was capable of exercising upon those who dragged him to the place of execution. Not a single groan escaped him during the period he was nailed to the wood, nor while the cross was fixed upright in the earth. The wooden

* "Claudius Salmasius, De Cruce, &c., pp. 229–340, &c.;—Justus Lipsius, De Cruce, pp. 98–109, &c.;—Dr. Adam Clarke, The New Testament, with Commentary, &c.; Comment on Matt. chap. 29, v. 35.

frame used in the place of a cross resembles a gallows. To this the malefactor is nailed whilst it remains flat upon the earth, after which it is lifted up with its miserable burden, and fixed in two holes made in the ground for the purpose. Here the sufferer is kept until he dies of cold, hunger, or agony. Some criminals after being nailed to the frame, have remained for hours for the gaze of the multitude. A fire has oftentimes been placed to windward of them, by which they and the cross have been consumed together.'" P. 42.

Mr. Slade also, in his record of Travels in Turkey, Greece, &c., gives the following horrible account of the execution at Constantinople of a captain of banditti, a few years ago :—

"As a preparatory exercise, he was suspended by his arms for twelve hours. . . . . The following day a hook was thrust into his side, by which he was suspended to a tree, and there hung enduring the agony of thirst till the third evening, when death closed the scene; but before that about an hour the birds, already considering him their own, had alighted on his brow to peck his eyes. During this frightful period he uttered no unmanly complaints, only repeated several times,—'Had I known that I was to suffer this infernal death, I would never have done what I have. From the moment I led the klephte's life I had death before my eyes, and was prepared to meet it, but I expected to die as my predecessors, by decapitation.'" P. 43.

From what has now been stated, it may be very reasonably inferred that the death of Jesus, within six hours after He was nailed to the "accursed tree," was not, as is frequently believed, the result of extreme bodily torture, and therefore, as our author observes, that, "although the ordinary sufferings of crucifixion contributed to his death, they were not its immediate cause." It is certain that the bystanders, and others engaged in the dreadful act, were surprised at the suddenness of the Saviour's death; for we find that Pilate scarcely believed that such could be the case, when Joseph applied to him to take the body down from the cross.*

Admitting, then, that there was something unusual and extraordinary in the quickness with which the sufferings of our Lord came to a close, we now proceed to enquire, with all due reverence, whether any probable explanation can be offered to account for it. And here we must first remark that there is nothing in any of the sacred narratives, to make us believe that the human nature of the sufferer was brought to the verge of exhaustion by the bodily and mental sufferings—however great these may have been—which He had undergone, during the eighteen hours that elapsed from his agony in the garden up to the moment that preceded his dissolution. Commentators have repeatedly dwelt upon the circumstances of his crying *with a loud voice*, 'My God, my God, why hast thou forsaken me!' of his then saying 'I thirst,' and receiving the vinegar that was offered to Him; and, last of all, of his again crying in a loud tone, "It is finished; Father, into thy hands I commend my spirit,"—as being utterly irreconcileable with the supposition that life was at its last ebb, from the extinction of vital energy. These were evidences and signs—to use the language of an intelligent and pious writer—that "his life was whole in Him and nature strong. The voice of dying men is one of the first things that fails.

* St. Mark, ch. xv., v. 44.

With a panting breath and faltering tongue, a few broken words are hardly spoken, and more hardly heard; but Christ, just before He expired, spoke like one in his full strength, to show that his life was not forced from Him, but was freely delivered by Him into his Father's hands, as his own act and deed."*

One of two explanations must therefore be adopted to account for the suddenness of his dissolution; either that the Saviour, seeing that all things were now fulfilled, spontaneously, and by an act of His own divine will, yielded up His life; or that (not, we need scarcely say, independently of, but only in accordance with, His permission) some mortal lesion of a vital organ of his human frame suddenly supervened, and was the immediate and, so to speak, the physical cause of his death. Unquestionably the first of these opinions has been that which has generally been received by biblical commentators, and also, we should think, by most simple readers of the sacred narrative. It has this advantage, that it seems, at least upon first thought, to be most consonant with various expressions used by Christ himself in reference to his death: such as in that remarkable passage in the Gospel of St. John, where He says, "I lay down my life that I may take it again. No man taketh it from me, but I lay it down of myself." Moreover, it has been argued that the words αφηκε (and παραδωκε) το πνευμα, used by St. Matthew and St. John, rendered in our version, "yielded, and gave up the ghost," clearly imply a voluntary dismission of His spirit; but then, be it remembered, that the other two evangelists merely say that He expired, εξεπνευσεν.

Dr. Stroud—while he recognises, as every sincere Christian must do, in all its plentitude and force, the sovereign power of the Divine sufferer to yield or to retain his life, and only after the most studious and devotional investigation of all the scriptural statements, whether in the way of narrative, of prophecy, or of doctrine in reference to the death of Christ—adopts the second of the opinions alluded to above. The positive declarations that He was slain by his enemies, that He died the death of the cross, that He "became obedient unto death," that the Jewish rulers were "his betrayers and murderers," that they "slew the Prince of Life whom God raised from the dead," &c., intimate, he thinks, that the death was the result and consequence of the crucifixion, and not of any supernatural agency. Some learned divines have come to the same conclusion. Thus Bishop Pearson in his Exposition of the Creed, observes:—"Should we imagine Christ to anticipate the time of death, and to subtract his soul from future torments necessary to cause an expiration, we might rationally say that the Jews and Gentiles were guilty of his death, but we could not properly say they slew Him. Guilty they must be, because they inflicted those torments which in time death must necessarily follow; but slay him actually they did not, if his death proceeded from any other cause, and not from the wounds which they inflicted."

And our author has the following very appropriate observations on the same subject:—

"That it was in the power of Christ to avoid such a death, had He chosen to renounce the object of his mission, is evident amongst other reasons from his miraculous overthrow of the hostile band in the garden of Gethsemane; from his question to

* M. Henry's Exposition on the New Testament.

Peter,—'Thinkest thou that I cannot even now request my Father, and he would send to my aid more than twelve legions of angels? [but] how then would the Scriptures be fulfilled, [which declare] that thus it must be?'—and from his remark to Pilate,—'Thou wouldst not have had any authority at all against me, had it not been given thee from above.'—In all the scriptural allusions to this subject, the death intimated, although voluntary, is moreover represented, not as self-inflicted, but as penal and vicarious. In the very passage which has been thus misinterpreted, the death encountered by the good shepherd for the safety of his flock is ascribed to the wolf from whom the hireling flees." P. 59.

As a matter of course, it would be wholly out of place here to enter more at length upon the very interesting, but mysterious, question alluded to. It is not susceptible of perfect solution, and must therefore be left to each individual enquirer for calm reflection in his own mind.

We are now prepared to follow our author in his application of the facts and reasonings adduced, in the early part of the article, to the illustration (if so it may be called) of the sufferings of our blessed Lord. And first of his *bloody sweat*. It will be remembered that, on the night before his crucifixion, after celebrating the Passover with his disciples, and instituting that rite which was thenceforth to be commemorative in all ages of his death, and having commended himself, his followers, and his cause, in solemn prayer, to the Father, He went forth with his disciples to the Mount of Olives, and thence, across the brook Cedron, to the garden of Gethsemane. We then read that, when he arrived at this place, He began to be seized with consternation and anguish (ηρξατο εκθαμβεισθαι και αδημονειν,) so that his soul was "exceeding sorrowful, even unto death;"—that he then withdrew about a stone's-cast from the three favoured disciples whom He had taken with Him, and fell upon his face, and prayed most fervently that, if it were possible, that hour might pass from Him—that, returning to them, He found them asleep, and, after exhorting them to watchfulness, withdrew, and prayed a second time that, if it might be, the cup of his bitter affliction might be removed from Him—that, again returning to them, and again finding them asleep, He went away and prayed a third time, using the same words, "Father, if this cup may not pass from me, except I drink it, Thy will be done;"—and that then, an angel having appeared from heaven to strengthen him, He fell into an agony,* and prayed

* The import of the word 'agony' will be better understood from the following remark of Castello, in his Lexicon Medicum, 1746, on its Greek etymon. "Agonia, ἀγωνια, *angorem*, significat, et verbum ἀγωνιᾶν, juxta Galen, lib. 2, *de symptom. caus.* cap. 5, *ad finem*, affectum animi compositum ex ira et timore; illa quidem sanguinem et spiritum foras agente et fundente, hoc vero utrumque ad vitæ principium et interiora, cum refrigeratione eorum quæ in summo corpore sunt, reducente et contrahente. Derivatur ab ἀγών, *certamen*, lucta. Unde et ἀγωνια certamen quandoque significat. . . . . Ad summam, *Agonia* significat in genere colluctationem diversorum affectuum animi inter se contrariorum."

In the case of our Lord, it most probably implies a violent struggle of conflicting emotions—of overwhelming grief at the sense of divine abandonment on the one hand, and of intense desire of working out, even at the cost of his own life, the salvation of the human race, on the other. It is emphatically called by the prophet, the "travail of his soul."

with greater intensity, and "his sweat was as it were great drops of blood falling down to the ground," or, as Dr. Stroud renders the words, "his sweat became as it were clots of blood dropping to the ground, (εγενετο δε ὁ ἱδρως αυτου ωσει θρομβοι αιματος καταβαινοντες επι την γην).* Truly, never was sorrow like unto His sorrow!

Such is the simple and impressive account of that awful hour, given in the writings of the Evangelists. The following passage will now enable the reader to judge of the manner in which our author comments upon it:—

"The intense grief and consternation which the Saviour experienced at the commencement of his sufferings in the garden, and under the shock of which he fell prostrate to the earth, might possibly have destroyed him by simple exhaustion, but would never have produced the bloody sweat reported by St. Luke; who, independently of his guidance by the Holy Spirit, was, as a physician, peculiarly well qualified to notice and record such an occurrence. He therefore ascribes this sweat to a cause by which it is fully and solely explained, namely, the communication of supernatural strength;—'There appeared to him an angel from heaven, strengthening him.'—It was then that,—'falling into an agony, [Christ] prayed most earnestly, and his sweat became as it were clots of blood dropping to the ground;' implying that he was no longer prostrate as at first, but on his knees. Attempts have been made to explain away the strong terms used by the evangelist, but they certainly denote a sweat mixed with blood in a half-coagulated state, so profuse as to fall from the head and neck (the parts liable to be uncovered, and from which sweat of any kind is most readily furnished,) in thick and heavy drops to the ground. Unless St. Luke meant to convey this meaning, his employment of such expressions is unaccountable. The fact is well stated by M'Lean.—[Christ] 'is said to be *in an agony*. An agony is a conflict of nature in the extremity of distress. The Lord was now bruising him, and putting him to grief. So great was the agony and conflict of his soul, that it produced the most wonderful effect upon his body; for we are told that—'his sweat was as it were great drops of blood falling down to the ground.' A common sweat in the open air, and exposed to the cold damp of night, when those within doors require a fire of coals to warm them, must have been the effect of very great fear and agony. What then must his agony have been, which induced a bloody sweat, and so copious as to fall down in great drops to the ground?'—It was then that, as intimated by the Apostle Paul,—'he offered prayers and supplications, [accompanied] with tears and loud cries, to him who was able to save him from death, and was heard on account of his pious fear;'—in other words, these peculiar and overwhelming sufferings were by divine interposition suddenly terminated, leaving him with restored strength, ready to undergo the trials which next awaited him." P. 116.

It is unnecessary to do more than merely allude to the perfect calmness and meek composure with which He endured the indignities and sufferings of the mock trial, the buffetings and scourgings of the Roman soldiers, the blasphemous reproaches and revilings of his own countrymen, the labour of carrying the cross, the preparations for execution, and, lastly,

---

* The Greek word θρομβος is usually interpreted by the Latin one, *grumus*. Schleusner, in reference to it, says:—"In scriptis medicorum Græcorum admodum frequenter vox θρόμβος de gutta spissi et coagulati sanguinis, et de sanguine coagulato in universum usurpatur. v. c. Dioscorid. I, c. 102; Hesych. θρόμβος: αιμα παχυ, πεπηγος, ὡς βουνοι. Confer. Faesii Œcon. Hippocr. p. 167."

Dr. Stroud remarks that "the force of the term ὡσει, frequently used by St. Luke in a similar sense, evidently is that Christ's sweat on this occasion consisted of clotted blood, not pure, but mixed with the usual watery liquid."

the pains and torture of crucifixion itself. Not a word of complaint or impatience escaped his lips; "He was brought as a lamb to the slaughter, and as a sheep before her shearers is dumb, so He opened not his mouth." For the first three hours that He hung upon the cross all was silent and still on his part, save when He prayed for forgiveness to his murderers, when He gave the penitent thief a divine assurance of pardon and acceptance, and when He committed his widowed mother to the care of the beloved disciple. It was not until the sixth hour (noon) when the sun became obscured, and blackness overspread the earth, in attestation, as it were, of the hiding of his Father's countenance, and of the consequent renewal of that dark and overwhelming agony of mind which He had experienced on the preceding night, and which would then apparently have proved fatal, had not supernatural aid been granted, and the duration of the fearful conflict been limited to one hour.

"On both occasions," says our author, "these sufferings were distinguished from all others, by beginning and ending abruptly, as well as by their peculiar circumstances and effects. On both occasions, the gloom which oppressed the Redeemer's soul was by divine appointment accompanied by external darkness, as its appropriate sign and illustration. When He was in the garden the preceding evening, it appears from astronomical calculation that the paschal full moon underwent a natural eclipse; on which account, perhaps, the numerous party which went forth to seize him were provided with lanterns and torches. Twelve hours later on the same day, according to the Jewish mode of reckoning, a preternatural darkness overspread the whole land from the sixth to the ninth hour." P. 120.

During this fearful interval, no intercourse took place between the suffering victim on the cross and the bystanders around; and a solemn pause in the evangelical narrative concurs with other circumstances to intimate that He was again enduring the peculiar sufferings of Gethsemane. It was then that, to use the language of inspiration, He "trod the wine press" of his Father's wrath, "alone," unstrengthened by heavenly aid, rejected and despised by the very world that He came to save, and exposed, at the same time, to the sharpest assaults of all the powers of darkness. At length, his human nature gave way under the anguish of the conflict, and the cord of life was snapped at the moment of his intensest suffering. Everything thus seems to indicate that there was a sudden rent of the agonised tabernacle of life; and what more likely, we would reverentially ask, than that "the pitcher was broken at the fountain, and that the wheel was broken at the cistern?" But, without indulging this thought any further, let us again follow the sacred narrative. It was about three o'clock in the afternoon, the hour of sacrifice and just three hours before the commencement of the Jewish Sabbath, that the Saviour expired. Now, as we have already observed, the Mosaic law required that, before the Sabbath began, the crucified person should be dispatched and removed. The Jews therefore applied to Pilate for permission to do this, and it was accordingly granted. It was then probably between four and five o'clock when the Roman soldiers came and brake the legs of the two malefactors who had suffered along with Jesus.

"On finding Him already dead, they abstained from offering this needless violence to his corpse; but, as if to make sure, one of them with a spear pierced

pierced his side, whence, says the beloved disciple, an eye-witness of the transaction, —'immediately there came forth blood and water,'—and with peculiar solemnity remarks that the whole took place under the superintendence of divine providence, in fulfilment of two ancient prophecies concerning Christ, one of which declared that none of his bones should be broken, and the other, that the guilty people of Israel should look on him whom they had pierced." P. 123.

To account for the flow of "the blood and water" from the spear-wound, many conjectures have at different times been proposed. The ancient commentators generally "had recourse to their favourite expedient of miraculous interposition, designed, as they imagined, to convey important symbolical instruction." By several of the modern ones, it has been "ascribed to serous effusion either into the pericardial or pleural sacs, naturally produced by that extreme debility which they suppose to have attended the Saviour's death." With respect to the first of these explanations, it would be altogether inexpedient here to examine or discuss it. As to the second, we shall now briefly state the reasons which Dr. Stroud considers to be conclusive against its adoption.

It will be remembered that the wound was on the left side, just in the region of the heart: and indeed it was with the view of insuring the death of the sufferer that the Roman soldier thrust the spear into his side. He knew nothing of the ancient prophecy that the Jews should "look upon Him whom they had pierced;" it was merely in the fulfilment of his stern duty to see that the criminals were put to death; else his own life might be made a forfeit for his negligence.

"The Roman practice of despatching in some instances crucified persons by breaking their legs, stabbing them with swords or spears, &c., is well known, and, as above noticed, has been fully described by Salmasius, Lipsius, Bosius, and others. When the soldier, therefore, pierced the side of Christ, he did nothing more than what was usual, and, having such an object in view, would naturally inflict a decisive wound, that is, a stab to the heart. This opinion has accordingly been adopted by a great number of theological writers, many of whom are cited by Thomas Bartholinus, a Danish physician, who, however, in an express treatise on the subject follows the guidance of his father Caspar, and objects to this opinion for no better reason than that, when speaking of the wound, and of the scar which remained after Christ's resurrection, the evangelist John mentions the side only, and not the heart. As a faithful witness of the transaction, John of course relates only what he saw, but leaves his readers to draw a rational inference from the facts described, which can be none other than that here stated."*

But as it has been very generally admitted that the wound penetrated to the heart, and was designed to prove fatal had life still continued, we shall pass on at once to consider what is the most probable cause of the flow of "blood and water," which followed the thrust of the spear. The opinion of Bartholinus, that the latter issued from the bag of the pleura, has been partially at least adopted by several writers. Thus the Rev. Mr. Hewlett, in his notes upon the Gospels, expresses himself to this effect:—

---

*"Thomas Bartholinus, De latere Christi aperto, &c., pp. 17-22, 45, &c.;—Idem, Epistola ad Hieron. Bardium, pp. 565–570.—Acts, chap. 12, v. 18, 19."

"'Medical writers afford numerous instances of a large effusion of bloody lymph into the cavities of the pleura, from diseases of the lungs, and in cases of violent death with long struggling.....A skilful and learned physician informed the editor that in cases of violent and painful death there is usually an effusion of lymph, or of lymph mixed with blood, into the cavities of the chest and abdomen. It is, however, reasonable to acquiesce with those who are of opinion that the evangelist here intended to express more than a pathological fact.'—The physician meant in this passage was no doubt the late Dr. Willan, who—in his 'History of the Ministry of Jesus Christ,' —makes a similar remark, equally indicative of doubt and uncertainty.—'We have instances of watery effusion into the cavities of the pleura to a considerable amount, in cases of violent death with long struggling.....The phenomena here mentioned by the evangelist is generally looked upon as miraculous.'"* P. 135.

Some have supposed that the blood came from the heart and the water from the pericardium, both being pierced by the thrust of the spear. The elder Gruner thus comments upon the subject:—

"It was doubtless the left side that was pierced by the soldier's spear. According to the testimony of John, immediately after the infliction of this wound there flowed out blood and water. Such an effusion could scarcely have taken place, except on the left side, under which, besides the lung, lies the pericardium full of water when a person dies after extreme anxiety, as likewise the heart, connected with the arch of the aorta. The lung slightly wounded might have yielded a little blood, but certainly not water. That conjecture is, therefore, the most probable, and the most in accordance with forensic medicine, which derives the blood from the [left] ventricle of the heart, and the water from the pericardium." P. 136.

In a special treatise on the cross and crucifixion, Kipping draws the same conclusion, with the exception of regarding the water poured out on this occasion as naturally contained in the pericardium. Bishop Watson too, in his Apology for the Bible, has adopted the same view. But then, as Dr. Stroud very justly remarks, the quantity of serum usually found in the pericardium after death is so small that, "in a case like that under consideration, it would have peen absolutely imperceptible." Haller states that a small quantity, not exceeding a few drachms, has frequently been found in the pericardium of executed persons; and John and Charles Bell deny the occurrence altogether, except under very peculiar circumstances. "If," these eminent anatomists observe, "a person have laboured under a continued weakness, or have been long diseased, if a person have lain long on his death-bed, if the body have been long kept after death, there is both a condensation of the natural halitus in all the parts of the body, and an exudation of thin lymph from every vessel, there is water found in every cavity from the ventricles of the brain to the cavity of the ankle-joint, and so in the pericardium amongst the rest. But, if you open a living animal, as a dog, or if you open suddenly the body of suicides, or if you have brought to the dissecting room the body of a criminal who has just been hanged, there is not in the pericardium one single particle of water to be found."

In the natural state, the quantity of the pericardial fluid is certainly very

---

* Hewlet's Bible, &c.; Notes on John. chap. 19, v. 34, and Acts, chap. 1, v. 18; Dr. Willan, History of the Ministry of Jesus Christ, &c. p. 195.

small; but after some forms of violent death, more especially when this has been attended with obstructed respiration, there is often rather more; and then it is either pure or tinged with blood. "An effusion of the latter kind is said to have been noticed in stags killed after a hard chase; and in some rare instances of sudden death, occasioned by strong mental emotion, the pericardium has been found distended with blood, owing probably, as Morgagni suspected, to organic disease, and the rupture of vessels; but for the statement of the Grüners that, after death accompanied with anxiety, the pericardium is full of water, there is no evidence."

Besides, if such an effusion take place slowly and gradually, the fatal result will be not sudden or unexpected, but lingering, and preceded by symptoms of oppression and suffocation. But this pathological objection has little weight, compared with that derived from the simple narrative of the sacred text; for is it not obvious that the discharge of blood and water must have been considerable, and the distinction between the two well marked, to have been clearly observed and so emphatically mentioned by St. John? Dr. Stroud remarks upon this point:—

"Bloody serum, whether originally effused in that state, or resulting from subsequent mixture, would not have presented this character; for it would neither have issued rapidly, nor in sufficient quantity, nor would its distinction from ordinary blood have been so striking as to have attracted the attention of an uninformed and somewhat distant spectator. Moreover, unless blood has been previously extravasated, little or none can by any kind of wound be extracted from a dead body, except by the action of gravity, the heart being usually empty, or, if otherwise, devoid of power to expel its contents. This important fact, overlooked by most other writers, was perceived and acknowledged by the Grüners, who nevertheless failed to discover the true explanation, and were induced to adopt the inadmissible opinion, that Christ was not actually dead when pierced by the soldier's spear, but merely in a faint and languid condition, which allowed the heart to act feebly, and, on being wounded, to pour forth its blood, preceded by the water, which they suppose had previously collected in the pericardium." P. 141.

The conclusion which our author deduces from the whole enquiry, of which we have given an abstract in the preceding pages, is given in the following words:—

"It may, therefore, with certainty be affirmed, that between the agony of mind which the Saviour endured in the garden of Gethsemane, and the profuse sweat mixed with clotted blood which so rapidly followed it, violent palpitation of the heart must necessarily have intervened; this being the only known condition which could have been at once the effect of the former occurrence, and the cause of the latter. In like manner, when on the cross this agony was renewed, and by the addition of bodily suffering, was increased to the utmost intensity, no other known condition could have formed the connecting link between that mental anguish and His sudden death,—preceded by loud exclamations, and followed by an effusion of blood and water from His side when afterwards pierced with a spear—than the aggravation even to rupture of the same violent action of the heart, of which the previous palpitation and bloody sweat were but a lower degree and a natural prelude. If, whilst every other explanation hitherto offered has been shown to be untenable, the cause now assigned for the death of Christ—namely, Rupture of the Heart from Agony of Mind—has been proved to be the result of an actual power in nature, fully adequate to the effect, really present without counteraction, minutely agreeing with all the facts of the case, and necessarily implied by them, this cause must, according to the principles of inductive reasoning, be regarded as demonstrated."

It is not without interest to learn that some of the commentators, ancient as well as modern, on the Gospels, had adopted views which, with the exception of certain extravagant expressions and errors of detail in their exposition, will be found to be in strict accordance with our author's ideas. Want of space, however, utterly precludes our giving any quotations.

But what is of much greater importance than any merely human corroborative testimony is the circumstance of there appearing to be a distinct allusion, in several prophecies in the Scriptures of the Old Testament, to the more remarkable of the physical effects of the Saviour's sufferings, including the bloody sweat, the intense thirst, and finally the mortal rupture of the heart. In the 22nd Psalm it is said,—"I am poured out like water; my heart is like wax; it is melted in the midst of my bowels; my tongue cleaveth to my jaws;" and, in the 69th, we read that "reproach hath broken my heart." There is a still more striking intimation of the cause of death in that memorable prediction in the 53rd ch. of Isaiah, where we are told that "He poured out his soul (or, as Michaelis and some other distinguished Hebraists render it, *His life's blood*) unto death." When too we remember that, in all the types in the patriarchal and Mosaic dispensation prefigurative of the death of the Messiah, the shedding of blood was a necessary accompaniment of expiatory sufferings, and when we think of the symbols of the bread broken and the wine poured out in the rite of the last supper, not to mention the rending of the veil of the temple, the cleaving of the rocks, and the opening of the graves at the very moment of the mortal struggle, does not Dr. Stroud's explanation gain more and more upon our convictions? and do we not feel that he is justified in saying that, "the peculiar cause of the death of Christ, which by a regular induction from the evangelical narrative has been ascertained as a fact, remarkably illustrates the entire series of types and prophecies relating to that solemn event, which could not indeed in any other manner have been completely fulfilled? These in turn, by their minute and perfect correspondence with the circumstances, afford, if that were necessary, an additional confirmation of the fact itself; and the whole transaction demonstrates with irresistible evidence the special interposition and superintendence of the Deity."

And now we must take leave of our author, but not without again expressing our high admiration of his character and attainments, and saying at the same time that, in drawing to a close the present series of this Journal—and with it the Medico-Chirurgical Review itself as a separate and independent publication—it gives us very sincere gratification to have it in our power to recommend such a volume as the present to the thoughtful perusal of all our readers.

MEMOIRE SUR LA ROUGEOLE DES ADULTES. Par le Docteur *Michel Levy*,

An Essay on Measles in the Adult. By *M. Levy*, M.D., Chief Physician and Senier Professor of the Hospital of Instruction at Metz; late Professor at the Val-de-Grâce, &c. Octavo, pp. 48. Paris, 1847.

THIS *brochure* from the pen of Professor Levy, details the particulars of an epidemic of Measles in the Adult, which he has recently had the opportunity of witnessing at the Clinical Hospital at Metz, over which he so ably presides. The conclusions he has arrived at upon several interesting circumstances connected with the disease are well worthy the notice of our readers.

"The measles," he observes, "though manifesting itself less frequently among adults than children, is far from sparing these former to the extent supposed by several pathologists who have drawn the elements of their statistics exclusively from hospitals. In the most recent analytical work on medicine, the Compendium of MM. Monneret and Fleury, we find the statement—'After fifteen years the measles become rare.' This does not agree with the clinical observations at the military hospitals, which receive a large proportion of patients between the ages of eighteen and thirty. In these we are constantly meeting with cases of measles. In the east, south, and north of France, in Corsica, and the Morea, wherever, indeed, the vicissitudes of our career have taken us, we have seen young soldiers seized with measles, and frequently the affection attains in our garrisons the magnitude of an epidemic. During the siege of Antwerp, the ambulance at Boom, the surgical service of which was under my direction, received a certain number of cases of measles; and ten years' experience at Val-de-Grâce enabled me to observe these cases occurring periodically, and almost always proportionally to the extent the disease prevailed in the town. In 1837, there were sixty cases alone in the service under my charge, the disease prevailing at the same time epidemically at Paris, Versailles, and other places. The foundation of the present work is constituted by the facts I have observed in the hospital at Metz during thirteen months, from the 1st Jan. 1846 to 28th Feb. 1847. It is first necessary to make the remark that the distinction which is judiciously drawn between the diseases of civil life and those of garrisons, is not applicable in respect to our subject. During the period embraced in our observation, the progress of measles has been precisely the same in these two groups of the population. Absent or sporadic in the town it has been equally wanting in the hospital, or was only seen there now and then: the attacks becoming multiplied in the city, it soon became epidemic in the garrison. The parallel was constant."

The number of cases observed during these thirteen months amounted to 120, three patients only were less than eighteen years of age, the oldest being thirty-four.

Form of the Eruption.—Generally this was confluent, the face being almost always vultuous. In ten cases the confluence was excessive. The papular form (boutonneuse) was observed in four cases. In four others the eruption was anormal, being scarcely observable on the body, while the limbs were covered with large numbers of bright red spots. Two patients had a violaceous suffusion of the face, and a discrete eruption over the body.

Progress of the Eruption.—In ninety-nine out of the 120 cases this went through its regular stages. Delitescence took place in twenty-one—ten times on the second, and sixteen times on the third day. In seven cases cold seemed to be the cause of the sudden disappearance of the eruption, and in three the delitescene coincided with intense diarrhœa; but diarrhœa manifested itself in many other cases without deranging the course of the eruption. As to the consequences of the suppression of the eruption there were none remarked in fourteen patients, who were as promptly cured as if the eruption had pursued its usual course. Two were seized with diarrhœa and one with vomiting, but soon recovered. In another, varicella supervened, and in another signs of phthisis. In six, severe bronchitis followed; and two died subsequently to the retrocession, one having succumbed to an assemblage of thoracic complications, and the other to severe capillary bronchitis and acute dysentery supervening upon the administration of tartar-emetic. Of these pulmonary complications M. Levy thus speaks:—

"Dyspnœa, oppression, intense cough, bright coloration of the face, and even anxiety, are not certain indications of an inflammatory complication of the small bronchi or the pulmonary parenchyma. We have seen these symptoms carried to a high degree in measles with slight catarrh as fugacious as the exanthem; and then they disappeared as if by enchantment when the eruption had entirely come out. They are only the expression of a general labouring of the organism, resolving itself in the eruption. When they subsist after the complete evolution of this, they announce the co-existence, not of an ordinary bronchitis, but of a suffocative catarrh (capillary bronchitis), or of an inflammation (almost always lobular) of the lung itself."

Concomitants.—(1.) As a precursory symptom *Epistaxis* was of frequent occurrence, and in eleven patients appeared during the course of the eruption, and generally on several occasions. (2.) *Vomiting*, which is seldomer a precursory symptom than in small-pox, was observed in seven after the appearance of the rash. (3.) The precursory *Diarrhœa* did not derange the course of the eruption in any of the cases in which it appeared, and probably may be considered as a mere effect of the morbillous hyperæmia of the intestinal mucous membrane, just as weeping is of the ocular, or coryza of the pituitary. It is to be distinguished from that which is induced by a real complication, as from enteritis, &c., from which it may be distinguished by its coincidence with the other precursory symptoms, its mucous character, the comparative infrequency of the stools—these seldom exceeding six or eight in the twenty-four hours—the absence of abdominal pain, and its non-perturbatory power over the cutaneous eruption. In thirty patients this diarrhœa appeared. It was not interfered with, and it is to be remarked that the subjects of it suffered less from the morbillous bronchitis. (4.) *Constipation* was observed only in three patients. (5.) With four exceptions, *all* the patients offered the symptoms of *bronchial catarrh.*

"These symptoms are those of *bronchitis*, but is the modification which measles impresses upon the air-passages identical with phlegmasia of this membrane? If it is true that in this pyrexia the amount of fibrine continues stationary, or even in severe cases falls below the normal mean, we may, from this alone, conclude that the bronchial lesion, which nineteen out of twenty times accompanies measles, is not inflammatory, or at all events cannot be classed with the pure phlegmasiæ of the respiratory organs. The above-named symptoms

do not absolutely characterise inflammation of the bronchi; for we observe them whenever these become the seat of active or passive congestion, as after the first week of typhoid fever, and in the advanced lesions of the heart. A simple hyperæmia of the bronchial mucous membrane, analogous to that which, directed on the reticular texture of the skin, determines the production of the spots of measles, may give rise to symptoms of bronchitis. Later, in a certain number of cases, inflammation does supervene, but it participates in the special nature of the congestion which has prepared the way for it. In this way we explain the rubeolic catarrh, to which we may add coryza, angina, ophthalmia, and diarrhœa.

"If the patient is surrounded by a temperature and the care his case requires, this follows the phases of the exanthem and does not survive it. If, however, he is imprudently exposed to the cold, the severity of the weather renders it difficult to protect him from this, or if he is predisposed to diseases of he respiratory organs, then it is by no means uncommon to find this morbillous hyperæmia of the bronchi pass into an inflammatory type, and retard convalescence, however regular the course of the eruption may have been. The measly eruption, too, may become propagated throughout the whole extent of the bronchial tree, and from the very commencement of the rubeola symptoms of a deep-seated bronchitis may coincide with those of the exanthem. Nine of our patients offered striking examples of this. The case now becomes serious. The frequency of the circulation increases in an inverse degree to the amount of respiratory surface which remains disposable in the lungs. Wherever it is invaded by the exanthem, the bronchial surface re-acts less vigorously on the air, or may become totally unfit for this function. The more narrow the field for hæmatosis is rendered the more must the blood be hastened in its course to bring it a greater number of times in contact with the air. Hence violaceous colour of the face, especially of the mucous orifices; rapidity of pulse (120 to 140), and of inspiratory movements (48 to 60); rhonchus audible at a distance; vibratory and sub-crepitant râles heard over the entire chest; obstinate, harsh, paroxysmal cough, followed by the laborious expectoration of small quantities of tenacious and viscous matters, or of a white, foamy froth; an ever increasing anxiety; and at last delirium. This is the spectacle such patients present, and, without the promptest aid, they rapidly fall into a state of asphyxia."

Complications.—(1.) The co-occurrence of several simultaneous eruptions was only observed towards the end of the epidemic. Eight patients then furnished examples of this. In three, varicella was commingled with the rubeola. In one, rubeola, varioloid, and miliaria were observed simultaneously: and, in another, with variolous pustules on the face, there was a scarlatina suffusion on the trunk, and spots of measles on the neck and limbs. In two other cases the measles was complicated with urticaria, and in another a purpura, which required 23 days for its cure, appeared. (2.) The most common complications were affections of the chest, in the form of bronchi-pneumonia, of which six examples are cited, one of these proving fatal, in consequence of the patient's neglecting its treatment, the eruption pursuing its normal course. (3.) Diseases of the digestive organs have rarely offered complications. These complications have also been equally rare in the various instances of measles in the adult which have on former occasions come under M. Levy's notice; while MM. Rilliet and Barthez noted the occurrence of gastro-enteric phlegmasia in 46 out of 167 cases of measles occurring in children. He agrees with MM. Blache and Guersant, that this difference is rather due to the cachectic condition of the children, who formed the object of those researches, than

to the mere effect of age—a condition resulting from a long residence within the walls of a hospital, and usually leading to an entero-colitis or pneumonia.

Consecutive Diseases.—"The question of diseases consecutive to measles is of a great importance in practice; but science is far from being in possession of a sufficiency of materials for its solution. The difficulties it gives rise to depend upon the complexity of the etiological elements. We are constantly exposed to the deduction from the simple succession of two facts of a relation of causality which does not exist between them. Has the so-called consecutive disease closely followed the measles? It may depend upon the constitution of the subject, and upon the influences which modified it prior to the attack. If an interval has elapsed between the one and the other, who can say whether the new affection, whatever relationship it may seem to have with the measles, is not due to causes which necessarily escape the physician? However this may be, we were well placed to follow the ulterior phases of the health of our measles patients. On the one hand our observations extend over a period of thirteen months, and on the other hand scarcely any of the patients we have treated for the disease have quitted the garrison, but remained under the eyes of our colleagues in the different regiments, whose attention having been drawn to them by the memoranda we had placed on their tickets of dismissal, they were subjected to an attentive superintendence, and in the case of their diseases requiring their return to the hospital, they were again consigned to our clinical wards."

In this manner 16 patients who had been attacked with measles came again under the author's care, and of whose cases he presents an analysis. The results, to which we can alone refer, are that eight of these cases presented examples of non-tubercular and eight of tubercular affections; the former consisting of bronchitis, pneumonia, or pleurisy, or two of these combined.

"The most general fact which results from the tables is, that all the diseases supervening after or springing up with measles have fallen upon the respiratory organs. Must we hence conclude that this disease impresses on these a greater aptitude to phlegmasiæ? Clinical observation replies affirmatively. Bronchial hyperæmia is one of the determinations of rubeola upon the internal tegument. Inflammation of the bronchi frequently spreads even to their capillary divisions, and lobular pneumonia is added in a great number of cases. These are facts acquired by science, and which the observation of our predecessors had long since established, with less anatomical precision, and according to different etiological views, which however detract nothing from the proper signification of facts. '*Infantes*,' says Sydenham, in 1670, '*præsertim huic malo (spirandi difficultati et tussiculæ importunius urgenti) sunt obnoxii quod morbillis jam facessentibus se ostendit, unde in peripneumoniam conjiciuntur, quæ plures jugulat, quam aut variolæ ipsæ, aut symptoma quodcumque ad eum spectans morbum.*'

"Is the tuberculization which we have noticed in one half these cases at all connected with this inflammatory habitude of the respiratory organs, with the progressive anæmia of the patients who have suffered from rubeola, and who do not become re-established, or with any specific and not yet defined influence belonging to this eruptive fever? These problems will never obtain a rigorous solution. It is impossible to say whether these eight examples possess only the value of a simple coincidence with measles, or whether this has played a more active and direct part in the production of the tubercle. If it were established that the frequency of bronchial and pulmonary inflammation suffices to lead to its secretion, what we have already said of the power of measles to excite inflammation would furnish a reply to the question. Tubercle shows itself sufficiently

frequently in children who have had measles; for, according to both Rilliet and Barthez, there is about one example of this in every eleven cases. M. Andral believes that measles accelerates the softening of pre-existing tubercle, an opinion which the facts we have collected do not positively confirm. Without carrying this discussion further, as it can lead to no certain proof, by reason of the insufficiency of clinical observations, we cannot admit with M. Rufz, 'that there does not exist in science a more hazardous proposition than the pretended influence of measles in the development of tubercle.' So many judicious observers of all epochs (Hoffman, Stoll, Frank, Rayer, Guersant, &c.) have seen phthisis succeed to rubeola, that the relation which they have believed themselves able to perceive between these two affections, though deprived of the sanction of a positive demonstration, cannot be altogether destitute of truth. The facts above reported incline us to this opinion. Whether in the case of adults or children, the practitioner should examine with the greatest solicitude the state of the respiratory organs, both during the course, and after the disappearance of the disease."

Antagonism.—Measles seems to exert a certain salutary action against some of the chronic diseases of the skin; obstinate eczema, impetigo, &c., disappearing after the induction of congestion of the cutaneous surface by the new disease. MM. Blache and Guersant have also seen certain neuroses, as chorea and pertussis, cured under the influence of measles. M. Levy mentions two or three facts, showing the occasional effect of the disease in inducing the suppression of long-standing gonorrhœa.

Mortality.—Of the 120 patients six died; a mortality far less than that attendant upon the disease in infancy. According to MM. Rilliet and Barthez, observing the disease however in a very bad class of subjects, uncomplicated measles is cured in five out of six cases, while, in the complicated form, one-half the cases fall victims. But these statements cannot be generalised, for in 1837 M. levy had 16 deaths in only 60 adults.

"Still, the general result of our observations leads us to recognise that the measles causes a smaller proportion of deaths among adults than among children; and this difference, which we are unable to express in figures, but in our opinion is certain, is explained by the following circumstances. A greater resistance of the organism: a less liability to lobular pneumonia: a greater facility of clinical examination, and certainty in treatment. For the rest, the rubeola of adults, like that of children, follows the course of each epidemic. The prognosis depends essentially on that unknown *quid divinum* which measures out to epidemics their proportion and sphere of energy. Meteorological conditions of themselves furnish no element for such appreciation. The serious epidemic we witnessed in 1837 coincided with a mild temperature, while in that at Metz, during December, January, and February, the thermometer was almost always between 0° C. and 10° C. It is important to distinguish the mortality as arising from the rubeola itself from that dependent upon the consequences of the affection. Not one of the deaths occurred during the eruption nor immediately after its delitescence. Between the period of admission and that of death a delay of from 12 to 38 days occurred. The danger proceeds not then from the exanthem itself, but from the complications and consequences. 'If,' says Stoll, (Aphorism 578) 'more die in the small-pox than from its consequences, more die from the consequences of the measles than from the disease itself, and perhaps as many in this as in the other.'"

Treatment.—This is much the same as in the measles of children. In the precursory stage, hygienic means alone are called for, and no attempt

Toynbee. These gentlemen have drawn up an elaborate report upon the subject: we now proceed to give a summary of its chief contents.

From their examinations and experiments, there can be no doubt that the Ledoyen fluid is an excellent corrector of foul and noisome stenches. So far, then, it may become a most useful application in a variety of circumstances, which need scarcely be mentioned. The trials that were made with it for the purpose of counteracting the offensive odour of fæculent and other discharges in the wards of hospitals, &c., were altogether most satisfactory. To nightmen too, and others engaged in the offensive, and often not unperilous, task of emptying old cesspools, opening foul drains, &c., the free use of the fluid will be found of the very greatest service. So far the report is altogether favourable to the discovery. Let us now see what other properties it possesses.

Although powerfully *anti-bromic** (to use an epithet suggested by Dr. Macdonnell, who wisely discriminates between the properties of "stench-destroying" and of "disinfecting"), the Ledoyen fluid does not possess, it would appear, any very notable *antiseptic* properties.

"The result of experiments," say the reporters, "induces us to doubt whether this fluid be more efficacious in checking the process of decomposition than other methods already known and in common use. As to large and solid parts, such as a whole limb, it is certain that they cannot by mere immersion in this fluid be kept in a state fit for scientific purposes. Membranous organs, as the intestine, may be somewhat better preserved by this fluid, and the same may be true of their sections of solid organs, as of the liver; but discolouration occurs in all these cases, and putrefaction is but imperfectly prevented. It is the opinion of a person thoroughly versed in these matters, and who assisted us in making these examinations, that a strong solution of nitrate of potash mixed with a very small portion of corrosive sublimate (bichloride of mercury), a preparation long and extensively used, is in all respects as effective as the fluid of Mr. Ledoyen.

"We are therefore of opinion that whilst this fluid may be regarded as a useful addition to the preparations already known for preserving parts for dissection, it has no peculiar efficacy in the preservation of the dead body. Still, however, from the result of experiments hereafter to be stated, we have no doubt that this fluid might be usefully employed in the dissecting-room, to disinfect the atmosphere and even for the removal of fœtor from the exposed surfaces of parts under dissection." P. 7.

Having thus ascertained the qualities of the fluid as an antibromic and antiseptic, we have next to examine whether it be entitled to the appellation of a "disinfectant," in the sense of a corrigent or antidote to the operation of infectious or disease-transmitting effluvia.

Colonel Calvert, indeed, takes this point for granted; for he does not hesitate to assure Lord Morpeth that "it disinfects sailors suffering from fever on board of vessels; it will also disinfect ships at sea and under quarantine. It disinfects patients suffering with infectious disorders and wounds, also dead bodies, so that they may be kept nearly a month." The jumbling together here of conditions, which have scarcely any analogy with one another, can only be excused on the ground of the Colonel's knowing very little of what he was writing about. As to the preservative

* Αντι, *against*, and βρωμος, *fœtor*.

I. Copy of Reports and Communications on M. Ledoyen's Disinfecting Fluid, with Supplement. Ordered by the House of Commons to be printed, 1st July, 1847.

II. Copy of Reports on Sir William Burnett's Disinfecting Fluid. Ordered by the House of Commons to be printed, 20th July, 1847.

In noticing these two Parliamentry papers, we must protest *in limine* against the very objectionable misapplication of the term "disinfecting," as employed in both. A vast deal of error and confusion has been introduced into medical reasoning by the vague and unsettled use of the words infect, infection, disinfecting, and so forth. Every one, we presume, will admit that they should all have a correlative signification, and that this signification should be definite and unfluctuating. In recent numbers of this Journal, we have sought to attain this important end by limiting the term "infection" to denote that power or quality of certain maladies to propagate themselves by effluvia given off from the bodies of the sick, and becoming diffused through the surrounding atmosphere. It must be quite obvious that, unless some such definitive restriction be adopted, it will be utterly impossible to argue with any degree of intelligible precision, far less to determine with scientific accuracy many of the questions connected with the important subjects of the origin and dissemination of febrile diseases, as well as of the best means of checking their growth or arresting their progress. We are well aware that many medical writers, and those too of high repute, have used the words in question in a larger and more comprehensive sense than that which we think wise. Still, without almost any exception, they have very generally been employed in reference to the production and presence of morbific effluvia. It is for this reason that we object so strongly to the employment of the term "disinfecting" in the sense of "stench-destroying," or even of "putrefaction-arresting;" and we therefore cannot but regret that Sir William Burnett should have given the sanction of his high authority to the perpetuation of such a misnomer, —calculated, as it is, to convey an erroneous impression to the public. It is not at all to be wondered at that M. Ledoyen, and the great trumpeter of his nostrum, Colonel Calvert, should have fallen into the blunder; if *blunder* it can be called in their case, seeing that they claim for their fluid the property not only of destroying all noisome smells, but even of "disinfecting patients suffering with infectious disorders." We shall presently see what evidence or proof can be adduced for this assertion. When M. Ledoyen, or rather Col. Calvert in his name, applied to the British Government for assistance in order that (to use his own modest words) "this country may, by publishing it here in Great Britain, spread the beneficent works of the Creator, who has given by his wisdom to man this important discovery to prevent disease," Lord Morpeth very wisely committed the investigation of the said "extraordinary discovery" (for so it is termed) to Dr. Southwood Smith and to Messrs. Grainger and

Toynbee. These gentlemen have drawn up an elaborate report upon the subject: we now proceed to give a summary of its chief contents.

From their examinations and experiments, there can be no doubt that the Ledoyen fluid is an excellent corrector of foul and noisome stenches. So far, then, it may become a most useful application in a variety of circumstances, which need scarcely be mentioned. The trials that were made with it for the purpose of counteracting the offensive odour of fæculent and other discharges in the wards of hospitals, &c., were altogether most satisfactory. To nightmen too, and others engaged in the offensive, and often not unperilous, task of emptying old cesspools, opening foul drains, &c., the free use of the fluid will be found of the very greatest service. So far the report is altogether favourable to the discovery. Let us now see what other properties it possesses.

Although powerfully *anti-bromic** (to use an epithet suggested by Dr. Macdonnell, who wisely discriminates between the properties of "stench-destroying" and of "disinfecting"), the Ledoyen fluid does not possess, it would appear, any very notable *antiseptic* properties.

"The result of experiments," say the reporters, "induces us to doubt whether this fluid be more efficacious in checking the process of decomposition than other methods already known and in common use. As to large and solid parts, such as a whole limb, it is certain that they cannot by mere immersion in this fluid be kept in a state fit for scientific purposes. Membranous organs, as the intestine, may be somewhat better preserved by this fluid, and the same may be true of their sections of solid organs, as of the liver; but discolouration occurs in all these cases, and putrefaction is but imperfectly prevented. It is the opinion of a person thoroughly versed in these matters, and who assisted us in making these examinations, that a strong solution of nitrate of potash mixed with a very small portion of corrosive sublimate (bichloride of mercury), a preparation long and extensively used, is in all respects as effective as the fluid of Mr. Ledoyen.

"We are therefore of opinion that whilst this fluid may be regarded as a useful addition to the preparations already known for preserving parts for dissection, it has no peculiar efficacy in the preservation of the dead body. Still, however, from the result of experiments hereafter to be stated, we have no doubt that this fluid might be usefully employed in the dissecting-room, to disinfect the atmosphere and even for the removal of fœtor from the exposed surfaces of parts under dissection." P. 7.

Having thus ascertained the qualities of the fluid as an antibromic and antiseptic, we have next to examine whether it be entitled to the appellation of a "disinfectant," in the sense of a corrigent or antidote to the operation of infectious or disease-transmitting effluvia.

Colonel Calvert, indeed, takes this point for granted; for he does not hesitate to assure Lord Morpeth that "it disinfects sailors suffering from fever on board of vessels; it will also disinfect ships at sea and under quarantine. It disinfects patients suffering with infectious disorders and wounds, also dead bodies, so that they may be kept nearly a month." The jumbling together here of conditions, which have scarcely any analogy with one another, can only be excused on the ground of the Colonel's knowing very little of what he was writing about. As to the preservative

* Αντι, *against*, and βρωμος, *fœtor*.

effects of the fluid, we have already seen how far the evidence of the reporters warrants his very confident statements. But what say these gentlemen on the much more important allegation as to its alleged power of counteracting the agency of infection? At page 11 we read thus:—

"In severe and malignant fevers, especially when several cases of this character occur at the same time, and when the wards of the hospital are crowded, as always happen when fever becomes epidemic, the nurses invariably suffer, and some of them are sure to perish. In our opinion, the use of this fluid will be a great protection to this class of persons under such circumstances, while, by keeping the air of the wards fresh and wholesome, it will be alike protection to the physicians and other officers, as well as to the friends of the patients who come to visit them; and at the same time it must operate favourably on the general course of the fever itself.

"There is another important application of this fluid in fever, and in all other diseases in which the excretions of the body become vitiated. It rarely happens at the fever hospitals that a laundress who retains her situation a few months, when fever is at all prevalent, escapes an attack of fever. In bad cases of fever, the evacuations are passed in bed, involuntarily and unconsciously; and all the excretions of the body with which the bed-clothes become saturated are much more noxious than in health. The effluvia which arise from these bed-clothes, when they are washed, are sure, sooner or later, to produce fever in those who inhale them. By immersing the bed-clothes and the body-linen in this fluid, it is probable that many lives might be saved, and that the constitution in still more instances might be prevented from receiving a shock which is never recovered, even when death is not the immediate result of the attack. It is obvious that the same must be true in other diseases besides fever; in fact, wherever the secretions and excretions become vitiated." P. 11.

It will be observed that the opinion here expressed is given only as a probable conjecture, and not as the result of any satisfactory experiments. The correction of fœtid effluvia is too readily assumed as a ground for believing that there is, at the same time, a necessary neutralisation or destruction of infectious miasms. But this is the very fact we want to discover. The reporters seem to have taken for granted that, if the one be the case, the other must be so likewise. At least, their language fairly bears this interpretation; for, in the very next sentence after that just quoted, it is remarked:—

"With reference to the power of this fluid to restore *purity to contaminated air*, it is important to advert to the common practice of burning pastiles and other matters, to remove offensive odours from sitting and other rooms. The effect of this practice is merely to overpower an offensive by an agreeable odour; the cause of the offensive smell still remains in full operation, only our senses are prevented from appreciating it; but when, on the contrary, the fœtor of a room is removed by this disinfecting fluid, the gas upon which the fœtor depends is decomposed, and therefore the air of the room is purified by the removal of the very cause that contaminated it." P. 11.

The expression which we have marked in Italics, occurring in the course of observations upon the spread of "severe and malignant fever," must naturally be regarded by most medical readers as implying an allusion not merely to a fœtid or offensive, but to a positively infected or miasm-charged, condition of the atmosphere. Moreover, that Dr. Smith (for we may reasonably suppose that he was the writer of this part of the Report) is constantly associating in his mind the co-existence of foul smells

with the generation and diffusion of infectious fevers is pretty obvious, from the circumstance of his making the remark, immediately after alluding to the promptly-fatal effects of Sulphuretted Hydrogen in a concentrated state upon human beings, that "we trace its remoter consequences in the fevers and choleras that follow." Yet, as in very contradiction of this mutual association, we find that Dr. Leeson—to whom the Reporters had applied for his opinion as to the virtues of M. Ledoyen's fluid—in his tabular arrangement of putrescent animal and vegetable effluvia, has classified "typhoid miasmata" among those which are *dangerous but inodorous;* and he (Dr. L.) adds, in reference to the fluid:—

"In regard to its efficacy as a *disinfecting agent*, the general result of these experiments establish the conclusion that the efficacy of this process is confined to the removal of the unpleasant odours due to sulphuretted hydrogen and hydro-sulphate of ammonia. As the sulphuretted hydrogen is the most abundant and most offensive of the various products of animal and vegetable decomposition, it is evident that, although this process cannot remove the whole of the offensive odours, it is still well suited to effect a very important and extensive amelioration of the nuisance arising therefrom." P. 13.

We may here state that the fluid in question is a solution of the Nitrate of Lead. Besides the advantage of being inodorous itself, it possesses this not inconsiderable one, viz., that the nitric acid combines with the ammonia of the putrescent matters, thus preventing its dissipation, and forming at the same time a salt which is known to be highly serviceable for agricultural purposes. Whether the metallic sulphuret, that is formed at the same time, may prove at all hurtful when the soil is dressed with manure that has been previously treated with the fluid, remains to be found out. The anti-bromic properties of the preparation are entirely owing to its power of fixing and neutralising sulphuretted hydrogen gas and hydro-sulphuret of ammonia. That it has any power in destroying other sorts of stenches appears to be doubtful. It is to be regretted that the Reporters did not think of comparing it with other preparations which have been used for the same purpose, before going so far as to recommend to the Government, "that, as it renders the removal of night-soil practicable without creating a nuisance, it ought, in our opinion, to be made a matter of police regulation that no privy or cess pool should be emptied without the previous use of a sufficient quantity of *it* to destroy all offensive smell." After the word "it," would it not have been only fair to insert, "or of any other equally effectual counteragent?" In making this remark, we allude more particularly to Sir William Burnett's (improperly designated) "disinfecting fluid," which has been much longer before the public than that which Colonel Calvert attempts to thrust upon us with such intemperate zeal. As strong and very conclusive evidences of the exceedingly useful properties of the Burnett fluid, we shall select two or three of the certificates which are adduced in its favour, in the Parliamentary Report just published, so that the reader will be able to form a tolerably fair estimate of the comparative advantages of the two preparations:

"Royal Naval Hospital, at Haslar,
"Sir: 12th of July, 1847.

"In compliance with your directions to us to report on the use of Sir William Burnett's fluid *as a disinfectant, or as to the removal of noxious smells*, we have

to inform you that it has been used in this hospital in the close-stools of patients affected with dysentery, in the water closets and cesspools, and also in the wards, when the air was tainted by purulent expectoration or discharge from sores, with the effect of immediately removing the disagreeable odours. It has also been used in the surgery with good effect, in removing the smell of putrefying animal substances, the odours of dead bodies under inspection, and when employed as a dressing to ulcers, it removes the disagreeable smell of purulent matter, and in the proportion of one part of the clear solution to eighteen of water, it preserves subjects of natural history from putrefaction, and in a fit state for anatomical inspection, after more than a year has elapsed, or as long as our trials of it have lasted. We have had no contagious or epidemic diseases in the hospital, by which its powers of arresting infection might be tested; but it has been used, much diluted, for sponging the skin of patients affected by fever, with evident benefit, and the immediate removal of the odour of perspiration, and as *it is itself inodorous*, it is in no way offensive to the patients.

We have, &c.

(signed) *John Richardson*,
Medical Inspector.

(signed) *J. Anderson*, Medical Inspector.
James Allan, Deputy Inspector.
Alexander M'Kechnie, M.D. Surgeon.
Alexander Stuart, Assistant Surgeon."

Captain Superintendent,
Sir W. E. Parry.

The fluid has been most extensively used for the correction of the noxious effects of foul bilge-water, and with the most satisfactory results, as evidenced by the reports of various naval surgeons, captains, and shipbuilders. One will suffice. Mr. Chapman, the surgeon of H. M. S. "Porcupine," says :—

"I have to inform you that your solution has been applied to the holds of the 'Porcupine,' (in which vessel I have the honour to serve,) and although a very short time has elapsed since its application, the disgusting effluvium which previously existed is now entirely removed; indeed, its effects appear to be instantaneous, for, on the morning after applying the solution, not the slightest fœtor existed; and as a further proof of its perfect success, plate, gold lace, and other metallic substances, now retain their colour and brilliancy, which before could never be kept from tarnishing—evidently showing the corrosive principles are also removed; but, above all, the health of the ship's company has already improved, and their comfort has been much enhanced by the removal of the deleterious and highly offensive effluvia wich were emitted previous to the application." P. 9.

Mr. Bowman testifies to "its value as a preservative of animal structures prepared by the anatomists. When used in a proper degree of dilution (about one part to fifty of water,) its success is complete, and it appears to me to preserve the colour and texture of the parts very admirably. It has the further very important advantage of not acting on the steel instruments, being, in this respect, equal to alcohol." Equally conclusive is the evidence of Dr. Sharpey, and other gentlemen.

Professor Quain has used the fluid as an application to sloughy tumours, &c., and he is of opinion that "it will supplant the Chloride of Lime and Soda altogether in the removal of fœtid odour." The only evidence, by the bye, adduced in favour of the Ledoyen fluid for this purpose, is a somewhat unsatisfactory communication from Mr. Travers, junior, relative to one case of "fœtid and ill-conditioned sores" on the leg, in which it was applied at St. Thomas's hospital.

It would seem, therefore, that Sir W. Burnett's preparation is vastly superior to M. Ledoyen's as an *antiseptic;* while, at the same time, it is equally potent as an *antibromic*. Let us now see whether there is any evidence of its (the former) being a disinfectant,—in other words, possessed of any power to arrest or modify the spread of infectious diseases. What thinks the reader of the following communication from Mr. Varling, the surgeon of H.M.S. "Vengeance," to Sir W. Burnett?

"Having used the chloride of zinc rather extensively on board Her Majesty's Ship 'Vengeance,' whilst employed in the conveyance of troops, I think proper to report to you the result thereof. We carried the 1st battalion of the 42nd regiment, consisting of about 700 men, women, and children, from Malta to Bermuda. Measles had prevailed epidemically in the regiment previously to their embarkation, but we received none on board labouring under the disease; yet, after being ten days at sea, several cases occurred simultaneously among the soldiers, and, on the 1st of April, having been then a month at sea, the disease appeared among our own people, ten cases occurring on that day, and from that day to the 15th of the month, when we arrived at Bermuda, fresh cases were almost of daily occurrence, either among our own people or the troops. On getting rid of the troops, which we did at Bermuda, my attention was of course specially directed to every means whereby the contagion could be destroyed. Cleanliness and ventilation were duly attended to, and every part of the ship where the sick had been, after being cleaned and aired, was sponged well over with the solution of chloride of zinc several times. Than the result, nothing could be bettter; the disease totally ceased, no fresh case occurring after. On our passage from Halifax, with the 60th regiment on board, the weather was so bad, and the ship working so much, that it was quite impossible to open any of the lower-deck ports, on which deck the whole of the people lived, troops as well as our own people, for eight days; the air throughout the deck was exceedingly vitiated with every mixture of noxious smell, but the free use of the chloride of zinc tended, in a most surprising manner, to do away with the bad smell; so much so, that the surgeon of the regiment came to me to get some to use in the part of the ship where the ladies of the officers were. The effect of the chloride of zinc is most obvious in correcting all bad and offensive effluvia; and from the sudden and surprising manner in which the measles disappeared after its use, it is not, I think, too much to say, that it must have been very instrumental in decomposing the miasm, or state of atmosphere in the ship, which tended to the generation of the disesse." P. 12.

Does the evidence warrant Mr. Varling's conclusion? We think not. Perhaps the following statement from Dr. Cronin, as to the effects of the solution when freely used in the fever hospitals at Cork, may be deemed a little, but only a little, more satisfactory:—

"I commenced," says Dr. C., "its general and exclusive use on Sunday the 20th June, 1847, chloride of lime having been previously in use, and have no hesitation in distinctly stating, that I found it a powerful agent in speedily removing all noxious, unwholesome, and offensive smells, arising both from the external and internal excretions of a number of persons occupying the same room, and which will arise, no matter how well soever ventilated such apartments may be.

"This effect of the use of chloride of zinc was not only observable in all the wards of both hospitals, but in the yards, necessaries and cesspools.

"That the purification of the air should have much influence in modifying the character, and in mitigating the severity of a disease (by many, very many, supposed to be contagious,)is an axiom very extensively, if not generally, admitted.

"How far such influence has been exercised by the use of the chloride of zinc

on the cases of disease under treatment, during the time before specified, I leave to you and others to draw your and their own conclusions from the facts which I am now about to state.

"In the month ending June 15th, eighty-three persons of both sexes were treated in hospital; of those, ten died. Whereas, from June 15th to this day, July 15th, there were admitted 162; of those four died (three males and one female), ninety-four were discharged cured, and sixty-four remain under treatment.

"Of those who died in this latter month, one was a very old man, a long confirmed drunkard; one a boy of cachectic constitution from infancy, and one a woman with pneumonic complication.

"You have seen how many (almost all) of those cases were of a severe typhoid character." P. 12.

Dr. Lindsay, Deputy-Inspector of Hospitals, in transmitting Dr. Cronin's letter, remarks:—

"The patients admitted into the Cove Hospital are mostly all labouring under fever of a typhoid form, many of them with the characteristic eruption at the beginning, and petechiæ appearing during the progress of the disease, accompanied by great debility, feeble pulse, and all the other symptoms of bad typhus.

"The rate of mortality, however, has been exceedingly low since the use of the chloride of zinc was commenced, and if any conclusion can be drawn from a three weeks' trial of it as a disinfecting agent, it must be highly in its favour.

"I herewith enclose a report from Dr. John J. Cronin, the present attendant physician of the hospital, and as I have myself been in almost daily observation of its use amongst his patients, I can add my testimony to his." P. 13.

We have already seen what the reporters on M. Ledoyen's fluid have stated on this point. Their report, it deserves to be noticed, is dated 29th March of the present year. On the 27th of April, they addressed the following letter to Col. Calvert:—

"There is nothing in Mr. Ledoyen's disinfecting fluid that can arrest the progress of fever, or influence the connexion which the history of the human family shows is universal and indissoluble between pestilence and famine. But though this preparation can produce no effect on the primary, it may have some influence on the secondary causes of fever, that is on those causes which increase its intensity, and favour its spread when once generated.

"Among the most powerful of the secondary causes are the excretions of the patients, whether those from the bowels, the lungs, or the skin. The disinfecting fluid will instantly remove all putrescent smell from uterine (alvine?) discharges, leaving only the odour of recent fæces, and that in a less offensive degree, and at the same time it will render those discharges very much less capable of giving off their volatile and diffusible exhalations. It is also probable that it may be so used as to keep the air of the hospital ward and of the private sick chamber in a state of purity by destroying the offensive odours arising from the exhalations of the breath and skin.

"It is therefore our opinion, that it would be useful to make this preparation known to the physicians and surgeons of Ireland, and also to private families; for if it be properly and assiduously used, it may have a beneficial effect upon the sick themselves, by keeping the air around them pure, and thereby materially facilitating their recovery, and it will unquestionably be a great protection to the attendants on the sick. But we must beg leave to give a distinct caution against this preparation being spoken of as a remedy in fever, for if it be so considered, it must lead to disappointment, and it will probably bring doubt on the properties which it really possesses, and which are capable of being applied in various ways to the public advantage." P. 35.

Dr. Leeson also, at the same time, having been requested by Col. Calvert to give his opinion as to what might be expected from the use of the fluid "in arresting the progress of infection and fever," expresses himself to the following effect:—

"Until much more is known with regard to the true exciting cause of fever, or rather, the immediate vapour exhalation or gas by which such infection is conveyed, it would be presumptuous in any one to say whether such cause would or would not be affected by your liquid.

"This, then, is an important object of experiment.

"It is well known, that wherever a fever-exciting atmosphere does exist, there also abound the very gases which your liquid is capable of destroying; but whether such gases are merely concomitant, or whether they are themselves wholly or partly the exciting cause, is at present a mystery: this, at least, is evident, that such concomitants are not harmless, although (as shown in the paper already referred to) the general opinion seems to incline to the belief that there are other exhalations which are the true miasmata on which fever is dependent." P. 36.

On the 26th of May, we find Dr. Smith writing to Col. Calvert respecting the use of the fluid in the Fever Hospital here, in these terms:—"It is nearly full, and we find the use of the fluid in the present crowded state of the wards extremely beneficial." He had previously remarked in the same letter:—"that there should be no death in a ward in which six deaths occurred daily before the disinfecting fluid was used, is an amount of success, if it should continue, which was not to be expected, and the continuance of which is not to be looked for." This certainly leaves a very favourable impression as to the "disinfecting" properties of the fluid on the mind of an unprofessional reader at least; we cannot, therefore, be surprised that Colonel Calvert pounces upon the Doctor's letter with peculiar avidity:—"it must excite," says he, with his usual *fanfaronade*, "every feeling of humanity to get this fluid into immediate general use, *as it will save the lives of thousands who are at this time afflicted with fever and dysentery.*"

A month subsequently to the date of this propitious letter, viz., on the 28th of June, appeared another letter from Dr. Smith, which, not only as it contains an account of his further experience, but also as it enunciates opinions of very questionable accuracy, deserves particular notice: It is addressed to Lord Morpeth.

"My Lord, 28th June, 1847.

"One of the constant and distinguishing characters of a severe epidemic is that it attacks the attendants on the sick. The fever which is at present prevailing to such a deplorable extent in almost every part of the United Kingdom exhibits this character in an unusual degree. From the accounts daily received from the larger towns in England, but particularly from those of Ireland and Scotland, it is certain that in great numbers of instances fever is communicated not only to clergymen and relieving officers who visit the sick in their own wretched homes and poisonous localities, but also to the nurses and medical men in attendance even on private families; while it is far more prevalent and mortal among nurses, medical students, and the surgeons and physicians of hospitals and unions. Now this part of the calamity at least might be spared. Whatever difficulties your Lordship may have encountered in obtaining the necessary powers to make even any commencement of a system of prevention, by the removal of the causes of fever, you have in your own hands, and have had for some months past, the

sure and certain means of preventing the extension of fever to the immediate attendants on the sick. An agent has been discovered (M. Ledoyen's Disinfecting Fluid) capable of entirely destroying the noxious gases arising from decomposing animal and vegetable substances. The properties and powers of this fluid, after having been examined by a series of careful and exact experiments, performed partly under your Lordship's own observation, have been further tested in the crowded and poisonous fever-wards of the hospitals and unions of Manchester, Liverpool, and Dublin. All classes of witnesses, from the nurses and wardsmen to the highest medical authorities, without a single exception, have corroborated (from what they have themselves seen) the correctness of the conclusions deduced from the original experiments, and given in detail in a report presented to your Lordship on the 29th of March, 1847.

"When used in a sick chamber, or in hospital and union wards, this disinfecting agent decomposes and destroys the poisonous matters given off from the breath and skin, and from all the discharges of the body, and thus maintains the air surrounding the patients in a state of perpetual purity. It therefore effects more than ventilation; for while ventilation merely dilutes the poisonous matters diffused in the air, by the introduction of fresh currents of pure air, this agent destroys the very *sources* of impurity.

"No instructed person will suppose that this fluid can exercise, as a remedial agent, any influence on the state of the fever itself, or on the diseased processes so often set up in it; yet the effect produced indirectly by it (merely by maintaining the purity of the surrounding air), in improving the condition of the patient, is sometimes most striking and permanent. It is a further property and advantage of this fluid, that it creates no disagreeable odour of its own (as is the case with other disinfecting agents), but, on the contrary, produces a peculiar sensation of freshness.

"I have been unable to afford my patients in the Fever Hospital the full benefit of this important discovery, on account of my inability to procure the fluid in sufficient quantities for daily and regular use. I have regretted this the more, because a bad form of erysipelas, proving fatal in several instances, has spread extensively through the wards, and I am satisfied this might have been checked by the free use of this fluid.

"I have also been anxious to procure enough of the fluid to immerse in it the body-linen and the bed-clothes of the patients; for we have scarcely ever had in the Fever Hospital a laundress who has not sooner or later been attacked by fever; but, from what has been stated, it is obvious that all these classes of persons, nurses, laundresses and medical men, who are always in imminent danger, and who so often suffer, might perform their arduous duties with perfect security. I therefore respectfully but earnestly beg of your Lordship no longer to withhold from the public, more especially in the present condition of the country, the knowledge of a preventive and remedial agent, the general employment of which (irrespective of other uses to which it is applicable) will undoubtedly contribute towards saving the lives of many valuable persons." P. 24.

That any instructed medical man should entertain so high an opinion of this, or of any other, alleged disinfecting agent as unwaveringly to assert that "it decomposes and destroys the poisonous matters given off from the breath and skin," and also that he has in his own hands *the sure and certain means of preventing the extension of fever to the immediate attendants on the sick*, does certainly surprise us not a little; and we must withhold our belief in the accuracy of such assertions, until much more satisfactory evidence be adduced than what has yet been brought before the public. Would it not have been well to have given the *ipsissima verba* of the medical officers of the fever hospitals alluded to, touching the very important point in question? And is it not rather surprising that Dr. Smith, on the

very day that he addressed Lord Morpeth in the terms we have just read, wrote to Colonel Calvert, respecting his own personal experience of the fluid, in language that is much less energetic and unqualified.

"By the free use," says he, "of the disinfecting fluid in the bed-pans; by the suspension of cloths saturated with it round the beds of the patients, and by the abundant diffusion of its vapour in the atmosphere of the wards, I am satisfied that the poisonous exhalations constantly emanating from the bodies of the patients *would have been* decomposed and destroyed much more rapidly and completely than could have been effected by any amount of ventilation alone." P. 26.

And straightway he adds this not unimportant statement respecting the very salutary effects of simple ventilation on fever patients:—

"By the construction and arrangement of the windows we have it in our power to introduce into the wards of the fever hospital any quantity of fresh air we desire; and the soothing and healing influence of this air on the patients is most striking; for when brought into the hospital in a state of violent delirium, with a parched and black tongue, they often become perfectly calm, and the tongue gets moist and begins to clean at the edges in a few hours after they have breathed the comparatively cool and pure air of the spacious and well-ventilated fever ward; but surely we may hope to effect still more, when, in addition to the inestimable advantage of ventilation, we obtain the means of destroying at their very sources the impurities which it is the object of ventilation to remove." P. 26.

The rest of the letter about the doctor and his family eating the peas and potatoes, which had been raised in his garden that was well manured with *Ledoyenised* night-soil, without suffering from any symptoms indicative of lead or of any other poison, need not detain us.

In place of the frivolous communications from the resident medical officer of the Fever Hospital at Battle Bridge, how comes it that Dr. South's colleague has not been applied to, to give his opinion of the virtues of the marvellous fluid? A few lines from Dr. Tweedie would surely have had greater effect, with the profession at least, than the correspondence between Mr. Sankey and Colonel Calvert. By-the-bye, we accidentally learn from Mr. Sankey a strange and certainly very reprehensible state of things that occasionally exists at the fever hospital, viz. that dead bodies have sometimes to be kept eight or ten days in the dead-house, "owing to the clumsy working of the registry of deaths."

The evidence of the resident medical officers of the Liverpool Fever Hospital merely goes to prove the stench-destroying properties of the fluid; but these gentlemen say not a word as to its efficacy as a "disinfectant." The same thing may be said of the testimony of the medical officers of the Infirmary and other hospitals at Liverpool. We now pass over to Dublin, where the Colonel seems, according to his own report, to have achieved his greatest triumphs. His modest statement is as follows:—

"On our arrival (in Dublin), I had the honour to present Mr. Labouchere's letters to Mr. Redington and Sir P. Crampton, one from Sir W. Somerville to Mr. Carmichael, and your Lordship's kind letters to Mr. Macdonnell and Mr. Roe: I was most kindly received, and immediately assisted by all to put to the test the fluid in the fever and dysentery wards of different hospitals, and a most dreadful dysentery ward was selected by us to show what could be effected: it

was in fact, the condemned ward of the North Union Workhouse, Dublin, where poor wretches were sent to die, and they usually had five or six deaths a week; effluvia and the stench was dreadful; the men in a wretched and helpless state; the *doctors refused going into it;* in the presence of at least a dozen physicians and surgeons, we entered; we undertook the care of the ward and patients; we purified it in a very short time; we recovered the sick, and we did not lose a single patient during three weeks; this will be certified to your Lordship by certificates from the physicians, &c. of the house, and others who witnessed, to their astonishment, this severe test: we have had other severe tests, for all of which I hold certificates from physicians, &c."* P. 36.

We beg, before proceeding, to say that we do not believe the assertion contained in the words we have italicised. And now, how is the above statement borne out by the medical certificates? Sir P. Crampton testifies to the *antibromic* properties of the fluid, and to them alone. Sir H. Marsh and Dr. Cusack certify to its effects "in quickly neutralising and finally destroying the noxious effluvia arising from urinary and fæcal deposits." Dr. Carmichael, while recognising its value in destroying fœtid odours, says:—"As to its power of disinfecting contaminated places, I could not positively vouch, without an extended system of experiments fairly conducted; but I think there is a strong presumption that it does possess this power, from the fact that, while it removes the most offensive odours, it leaves none in their place."

Dr. Kirkpatrick, also, physician to the Workhouse where the wonderful effects were produced, after mentioning certain facts showing the efficacy of the fluid in "decomposing, or in some unknown manner removing, noisome odours, and without producing any peculiar smell of its own," observes:—"The patients experienced great relief from the improvement in the air; and it is strange, although several deaths had occurred in this ward during the preceding week, yet, during the time of his visit here, no death happened amongst the patients first seen by Colonel Calvert;" and on the very following day, when writing to the Colonel, Dr. K. adds:—"The extraordinary effect it possesses in the decomposition of foul odours has been proved beyond dispute, and that to it may also belong the greater value of destroying that pernicious condition of the atmosphere upon which disease depends is neither impossible nor improbable."

There is another communication from Dr. Kirkpatrick, which for his sake, we trust, was never intended for publication; as it is anything but creditable either to his judgment or good taste. It contains, we may observe, nothing that is worthy of notice. Not so the letters of Drs. Carmichael and Macdonnell; who, in alluding to the experiments made at the Union Workhouse, fully recognise the value of the fluid in destroying offensive smells, but with proper caution add:—"we wish to be understood as pronouncing no opinion respecting the disinfecting powers of the liquor;

---

* It subsequently oozes out, by the Marquis of Downshire's note, that the fluid is not entitled to the exclusive merit of the cure in the Colonel's cases. The Marquis writing to him says—"be so kind as to send some by as early a period as you conveniently can, and write your own directions for its use, and your own *change of diet*, which appears to have so wonderfully brought about the sick under your charge in Dublin and Drogheda."

we have had no opportunity of ascertaining its powers in this respect, the determining of which would require a series of experiments, carefully conducted, and on a large scale."

Should any of our readers wish to learn more respecting the evidence of our Irish brethren about the Ledoyen fluid, they cannot do better than consult the capital article in the last number of our Dublin contemporary, from the pen of the editor. It exposes, with a not-unjust severity, the system of bold and braggart assertion which Colonel Calvert has thought fit to employ, in puffing off the nostrum which he has taken under his immediate protection.

Of the two preparations, which have now been brought before the attention of our readers, we are strongly inclined to believe that Sir W. Burnett's will be found to be most generally and extensively useful. Future experiments, however, can alone decide the point. One thing is certain, viz., that it is much more potently antiseptic than its rival. And this is just what might, *à priori*, be expected; for we know that Chlorine and many of its compounds have a very marked influence on all sorts of decaying matter, as well as upon most, if not all, offensive odours; whereas the action of the Nitrate of Lead appears to be limited to the neutralisation of ammoniacal and hydro-sulphuric gases. Some of our readers may perhaps remember that the late Professor Daniel was led, by the results of some experiments, to suppose that the waters of the estuaries and western seacoasts of Africa contained a large amount of the last-named gas—generated, it was presumed, by the action of decaying organic matter upon the sulphates contained in the sea-water—and to throw out the hint that the existence of this deleterious gas in the atmosphere might be connected with the production of that pernicious miasm which infects the regions alluded to, and proves so destructive to human life. The accuracy of the above statement has, however, not been confirmed, we believe, by subsequent researches, while the probability of the suggestion is at once set aside by what we know of the development of similar morbific malaria in other parts of the world. No observations, as far as we know, go to prove that there is a fixed connexion between the production of any form of fever, and the mere presence of any chemical gas in the atmosphere. The bearing of this remark upon the alleged "disinfecting" properties of the Ledoyen and Burnett fluids, and more especially of the former, will be at once obvious. One remark more, and we have done. Let it never be forgotten that the possession of an efficient antibromic may lead to the very serious evil, in certain circumstances, of getting rid of a temporary nuisance, while the removal of the radical mischief is overlooked or neglected. Dr. Smith and Messrs. Grainger and Toynbee have very properly dwelt, with marked emphasis, upon this point in their report; for, after all, cleanliness and free ventilation are the best sweeteners in the world.

TRAITE THEORIQUE ET PRATIQUE D'AUSCULTATION OBSTETRICALE. Par *J. A. H. Depaul*, D.M., &c. &c. Octavo, pp. 400. Paris, 1847. Labé.

THE subject of Obstetrical Auscultation has not attracted that notice and attention in this country which its importance demands. With the exception of Dr. Evory Kennedy, whose admirable volume contributed so much to diffuse a knowledge of it at the time of its publication, 1833, our obstetrical physicians have done but little to advance this most interesting branch of professional research, and, in this respect, have been greatly behind their brethren in France and Germany. The volume before us contains the results of our author's observations commenced ten years ago in the wards of the Maternity Hospital at Paris, under the guidance, we believe, of Professor Dubois, and steadily persevered in ever since both in hospital and in private practice. The author writes as one who is thoroughly acquainted with his subject, and his work is therefore likely to become, we should think, a manual of information on the subject of which it treats.

It is divided into two parts, the Historical and the Didactic. The first, occupying a third of the volume, gives an excellent account of the various treatises and papers which have been published both in France and elsewhere on the subject since the appearance of the admirable memoir of M. Kergaradec—unquestionably the true founder of obstetrical auscultation, although M. Mayor of Geneva had anticipated him in the discovery of one of its principal phenomena—in 1822 down to the present time, with an analytical summary of the contents of the most valuable of these writings. To the intelligent reader this account will prove acceptable; as it is not less instructive than interesting to trace the successive steps by which any branch of useful knowledge has reached to the status in which it now exists. We must, however, at once pass to the contents of the second or didactic part, which treats of the sounds that are discoverable in the abdomen during pregnancy, and the presence of which is more or less peculiar to, and characteristic of, this condition. There are, however, one or two prefatory observations that should be first briefly alluded to.

Dr. Depaul admits that the auscultation of pregnancy is attended with much more difficulty than that of thoracic diseases, and that skill, however great, in the latter department of diagnostic enquiry will not suffice to ensure speedy success in the former. He has known more than one instance of a diligent student not succeeding in distinctly making out the existence either of the uterine or of the fœtal sound, until after several months' repeated trials. M. Dubois, now one of the best obstetrical auscultators in Paris, confesses that he had to serve a long *apprenticeship* (that is the word he uses himself) to the pursuit, before he acquired anything like confidence in the results of his observations. It is undeniable, however, that, notwithstanding the difficulties, it only requires persevering diligence, and the opportunity of a sufficiently ample experience, to enable any one to overcome them all, and to gain that readiness in discover-

ing the presence or absence of those important signs of the pregnant state, which the sense of hearing can alone discover. Dr. Depaul suggests that there should be a regular class for obstetrical auscultation in every good school of medicine; nor do we see how the available and practical knowledge of it can ever be obtained save in this way. Certain it is, that no one will ever be able to instruct himself by the merely occasional opportunities afforded by private practice.

Besides the most perfect silence in the chamber, there are two or three other circumstances which the auscultator will find well to attend to. The rectum and bladder ought, if possible, to be empty at the time of the examination; for, independently of the sounds which may arise from the contents of these viscera, it is obvious that the distension of the abdomen, so produced, will prevent that ready pliancy and yielding of its parietes to the pressure of the stethoscope which it is often necessary to make, especially in the early months of pregnancy. Dr. Depaul prefers the recumbent to any other position for the purpose of auscultatory examination of the uterus. The thighs ought to be somewhat bent, in order to relax as much as possible the abdominal muscles; in this way only, can the contour of the uterine globe be distinctly traced, and the stethoscope be applied to its lateral regions. The height of the bed, on which the woman lies, is a matter of some consequence to the physician; for, if it be too low, he will soon find that the fatigue of stooping will prevent him from continuing his examination so long as he might otherwise wish; not to mention the headache and confusion of hearing which are apt to be produced thereby. Nothing should intervene between the instrument and the abdomen of the patient, but a soft handkerchief or chemise if this be thin and quite smooth; but in obscure cases it is better to apply the instrument directly to the uncovered skin. The corset, or any other tight article of clothing, must have been previously removed.

Dr. Depaul, it should be here remarked, gives a decided preference to mediate over immediate auscultation in obstetrical examinations. The following passage contains his reasons for this purpose:—

"In the first place, immediate auscultation cannot be employed, with any prospect of advantage, in the early months of pregnancy, when the uterus is scarcely above the level of the pelvic entrance. The same will be the case in a somewhat more advanced period, when, although the organ is higher up, there are interposed between it and the abdominal parietes folds of intestine or other intervening substance. The naked ear cannot be employed without bringing a large extent of the abdomen in contact with the side of the face. From this circumstance result, first, the friction-sounds inseparable from so extensive a contact; secondly, (and this remark had not escaped the penetration of Laennec,) in order sufficiently to depress the parts that separate the ear from the uterus, it is necessary to employ a much greater degree of force, the consequence of which is that the physician thereby increases the sounds arising from the contraction of his own muscles; and thirdly, even when the pregnancy is far advanced, and especially when there is a tendency to the anterior obliquity of the uterus, there are certain points of it which cannot be conveniently explored, those, for example, adjoining the inguinal regions. . . . . The great advantages of auscultation with the stethoscope are, that the sounds are thereby rendered more distinct and readily perceptible; that we can more easily determine their limits, as well as distinguish the one from the other; and, lastly, that we can with much greater accuracy appreciate their different shades or degrees

in point of intensity or force. This is not because the instrument adds aught to their intensity, but merely because it enables the physician to come within a shorter distance of their origin."

In all cases in which our author has succeeded in detecting the auscultatory signs of pregnancy in the very early months of pregnancy, it was with the aid of the stethoscope; examination with the naked ear having proved quite ineffectual. He then adds:—

"The pressure which is made with the ear necessarily bears upon a large surface, and this has many inconveniences. With the stethoscope, on the contrary, we act upon a limited point, and avoid all friction-sound. In the great majority of cases, moreover, we can, without effort or danger, remove or push aside the stratum of *liquor amnii* interposed between the fœtus and the uterine parietes. This proposition will be received with doubt by those who are in the habit of believing that the uterus is quite distended with fluid during pregnancy. But this opinion is not correct. When the different evolutions which take place during pregnancy succeed one another with regularity, the uterus remains yielding and compressible at all the stages, and this too usually to a considerable degree. Very near the full period, one or two litres of liquid might still be added to the liquor amnii, before complete distension was produced. In several instances, where women have died near their full period but without being delivered, and where the ovum was in all its integrity, I have ascertained the truth of the assertion now made."

We may observe that the stethoscope which Dr. Depaul always uses is one invented by himself, and which is now, he says, very generally used in Paris for obstetrical auscultation. A figure of it is given in his work. After these preliminary remarks, we now proceed to lay before our readers the pith and marrow of our author's very minute and accurate description of the different auscultatory signs of pregnancy; and this we shall do most correctly and briefly by first giving the conclusions, in which he has himself compressed and embodied the contents of each chapter, and then selecting a few passages from the main text to illustrate some of these conclusions at rather greater length. And first, of that sound which has its seat in the uterus itself, and with which the fœtus has nothing directly to do:—

1. The denomination of *uterine souffle* is preferable to any other term that has been proposed.

2. The uterine souffle does not resemble other blowing sounds that are perceptible along the trajet of arteries. Like them, indeed, it is isochronous with the contractions of the left ventricle; but it has characters peculiar to itself.

2. It varies exceedingly in respect of tone and persistence, and also as to the point of the uterus where it exists, &c. &c.

4. It has been demonstrated by incontestable facts that it may be perceived in the middle of the 11th, and even at the end of the 10th, week of pregnancy. In general, however, it cannot be discovered till somewhat later.

5. Its intensity goes on increasing until the end of the 7th month; after this time it makes but little progress. Allowance must be made for individual differences.

6. It is produced in the arteries of the uterus, and may be heard over every point of the organ that is accessible to the ear or to the stethoscope.

7. It is in the peculiar arrangement of the arterial system of the uterus, and in the modifications to which it is liable from the active movements of the fœtus, that we find the best explanation of its production, its irregularities, intermissions, changes in point of situation, &c.

8. Many physicians have erroneously regarded it as a sure and certain sign of pregnancy. Taken by itself, it has not much more value than the other rational signs of this state; but, in connexion with some others, it gives an importance to them, while at the same time it acquires a very great value itself.

9. It cannot in the present day be denied that a sound, in every respect similar to the uterine souffle, may exist when the enlargement of the uterus is owing to a cause altogether different from pregnancy.

10. The death of the fœtus does not appreciably modify it; *à fortiori*, it does not cease upon this event. We cannot, therefore, have recourse to it with advantage, if our object be to ascertain whether the child lives or not.

11. Neither is it modified by the diseases which may attack the fœtus during the course of intra-uterine life.

12. If it be true that a direct relation generally subsists between the size of the placenta and that of the child, we are not warranted in believing, as some writers would have us do, that we can judge of the strength and development of the latter by the intensity, or by any other character, of the uterine souffle.

13. As it is clearly established that there is no necessary relation between the uterine souffle and the placenta, we can readily understand that the morbid conditions of the latter are not discoverable by any modifications of the former.

14. For the same reason, this sound is incapable of giving us any precise notions as to the exact spot where the placenta is attached; and, if chance has sometimes seemed to warrant the idea in question, innumerable facts to the contrary have abundantly proved its general fallacy.

15. To pretend that, by the aid of this sign, we can form any rational conjecture as to the shape of the placenta, is an opinion altogether contradicted by experience, but which was, in some measure, the forced consequence of the erroneous doctrine that the uterine souffle was necessarily connected with the circulation of the placenta.

16. Neither can the presence of double pregnancies be ever made out by the existence of more souffles than one. Experience proves that two and even three distinct souffles may be met with, when the pregnancy is single; and anatomy, moreover, has shown that, in the majority of cases of twins, there is only one placentary mass.

17. The uterine souffle is of no utility in determining the situation of the child in the womb.

18. Lastly, it follows from all that has been now stated, that this sign is of very limited value in respect of its applications to practice.

With regard to the first proposition, Dr. Depaul remarks that, being convinced that the sound in question is not limited to the seat of the placenta, and that it does not necessarily arise from the circulation through its vessels, he objects to the appellation of *placentary souffle* which has been given to it by many writers, and prefers that of *uterine souffle* first proposed by M.

Dubois. To apply the term *pulsation* to the sound is surely very inaccurate; seeing that there is a total absence of any impulse or beat accompanying it. It is a something to be heard, not to be felt. It has usually the character of a soft intermitting puff; at other times it resembles the whistling of the wind through an ill-closed window, only that it is *saccadé* and not continued; while in other cases it is of a graver note, and is not unlike the vibrations of a bass string, or the cooing murmur of a dove. But whatever be the character of the sound, it is invariably isochronous with the pulse of the mother, and never with that of the fœtus.

There is no point of the uterine globe accessible to the stethoscope, in which this sound *may* not *be* heard. It is, however, over the lateral regions of the womb that it is usually most readily perceptible; but Dr. Depaul is not prepared to say whether it be more frequently present on the right, or on the left, side. Hohl says on the former; Naegele on the latter, admitting, however, that he has found it on both sides at the same time, and occasionally over every part of the uterus. Out of 295 cases in which pregnancy exceeded the fifth month, our author heard a distinct souffle on each side of the uterus, at a little distance from the crural arch in 182. It was discoverable on one side only in 27, and over the fundus of the organ in 43. In 18, the stethoscope could not be placed over any point of the uterus without the sound being readily met with; and, in 12, it was audible at three distinct points, viz., the fundus, and the parts immediately above the crural arch on each side. When the pregnancy was less advanced, Dr. Depaul has given the following results of his experience. In 4 cases where pregnancy had not exceeded the end of the third month, the souffle was heard in the median line above the pubis. In 13 cases, in which pregnancy had advanced another month, it occupied the same region; the stethoscope, it should be stated, required to be pushed somewhat forcibly towards the pelvis to discover it. In 3 cases, in which gestation was about the same period, it was heard on each side. Lastly, in 16 out of the 27 cases, in which pregnancy had reached the end of the fifth month, the sound was perceptible over every accessible part of the uterus; but it was nevertheless easy to determine that it proceeded from the lateral regions.

By the fourth proposition, the reader will perceive that Dr. Depaul confirms the accuracy of Dr. E. Kennedy's assertion, that the uterine souffle *may be* heard before the end of the third month, a period considerably earlier than that which has been fixed by most writers. The usual belief is, that it is not discoverable before the end of the fourth month; and, as a general remark, this is doubtless quite true.

M. Bouillaud and one or two other writers have, it is well known, attributed the blowing sound of pregnancy to the pressure of the enlarged womb upon the iliac arteries, and the consequent partial obstruction to the easy flow of blood through them. On this point our author thus comments:—

"Although I entirely repudiate the doctrine in question, I am ready to admit the possibility of the production of a blowing sound from the cause mentioned, having met with several examples of the kind in my own practice. In these cases, the phenomenon exists also on one of the sides of the uterus; I have never met with it on more than one side in any single case; and, in general, it is upon that which corresponds with the lateral inclination of the organ, when it does exist. It is, like the genuine uterine souffle, completely isochronous with the maternal pulse, and,

like it, follows all its changes; but it presents a constant character which will readily serve to distinguish the one from the other. We have now no longer a souffle without pulsation; on the contrary, what is present is a pulsation accompanied by a more or less intense souffle; but I have never met with this last so extended as to render the perception of the first at all difficult. Moreover, while the position of the woman, and consequently that of the womb, has no influence upon the genuine uterine souffle, the other, on the contrary, is modified and may even cease altogether."

Dr. D. alludes, also, to the occasional transmission of blowing sounds which originate from the heart itself of the mother; but no attentive auscultator can ever confound these with the souffle of pregnancy; for the force or distinctness of the former always increases as we recede from the uterine region towards the cardiac, while that of the latter is diametrically opposite in this respect.

As an illustration of the tenth proposition, we may adduce the following case.

Dr. Depaul was summoned to attend a young lady, pregnant with her first child; she considered herself to be at the full period. Upon making an examination, however, Dr. D. was surprised to find the uterus so little developed; for its fundus scarcely reached beyond the umbilicus. He therefore suspected either that his patient had made some fault in her reckoning, or else that the fœtus had been dead for a length of time. The fresh details now mentioned by the patient seemed to confirm the latter idea. On repeated auscultation, no fœtal tictac was ever discoverable; but a remarkably strong blowing sound, becoming sometimes of a sibilant character, might be heard over the side of the uterus. Dr. D. communicated his opinion to the husband; and the event proved how completely accurate it was. On the discharge of the liquor amnii, which was sanguinolent and fœtid, the fœtus was quickly expelled. It was in a semiputrid state, and seemed to have been dead for two or three months. The placenta was the seat of old and serious changes.

This case is but one out of a very large number of similar ones, which have occurred in our anthor's experience.

The only other remark that we have to make respecting the uterine souffle is, that it becomes much less distinct during the presence of a labour pain, and that it will often disappear altogether when the contractions of the uterus are strong, more especially if the *liquor amnii* has already been discharged. Our next enquiry is about the still more important sign, afforded by the transmission of the sounds of the fœtal heart through the uterine and abdominal parietes. The conclusions, which our author has drawn from all his elaborate researches upon this highly interesting point, are these:

1. The discovery of this auscultatory phenomenon preceded that of the uterine souffle, and we owe it to M. Mayor.*

* While the claim of the eminent surgeon of Geneva to the merit of this discovery cannot be denied, it is nothing but fair to M. Kergaradec to state that he appears to have been entirely ignorant of it when, in December, 1821, he read before the Academy of Medicine his important memoir, announcing the existence of a fœtal as well as of a uterine sound in the latter months of pregnancy.

2. Although there has never been any dispute as to its seat, and as therefore the name given to it is not of much consequence, it seems preferable to adopt that of the double cardiac pulsation (*doubles battements du cœur de l'enfant*).

3. Resembling in some degree the tictac of a watch, it consists of two pulsations that are quite distinct, and, in general, without the admixture of any blowing sound or murmur.

4. Nevertheless, a blowing or a rubbing sound may accompany it; but such an occurrence is rare, nor does it at all indicate the state of health of the child.

5. The double fœtal pulsation may often be heard earlier than is usually supposed. It is frequently discoverable by the end of the fourth month, and occasionally at three months and a half, or even earlier. I have adduced facts which incontestably prove that it may be heard at the end of the 12th, and even of the 11th week of pregnancy. (This is however of rare occurrence.)

6. The absence of the fœtal pulsations is an exceptional occurrence in the three last months* of gestation, unless indeed the fœtus had ceased to live. They were absent, or could not be heard, in 8 only out of 906 women who had advanced to this period.

7. The point of the uterus where they are best heard varies with the period of pregnancy, and still more according to the situation of the child.

8. The spot, which corresponds to the heart, is that which transmits them most forcibly; but, starting from this spot as from a centre, they may be perceived over a greater or less extent; and sometimes they exist, with a variable intensity, over every part of the uterus that is accessible to the stethoscope.

9. In the normal condition, their frequency always exceeds that of the maternal circulation.

10. This frequency is nearly the same at different stages of gestation as it is therefore an error to state that it goes on diminishing in proportion the pregnancy advances to its completion.

11. The uterine contractions have an obvious, but usually only a very transitory, influence upon the fœtal pulsations.

12. Their force becomes greater as pregnancy advances; but a good deal in this respect depends upon the circumstances of each case.

13. Moral emotions experienced by the mother have no direct influence on the life of the fœtus.

14. Disturbances of the maternal circulation act only consecutively on the fœtal circulation.

15. In my opinion, it is impossible to confound the double pulsations of the fœtus with any other sound which may be heard in the abdomen of a pregnant woman.

16. Their presence certifies not only the existence of pregnancy, but also the life of the fœtus.

---

* In several passages of our author's work it is stated that, in the latter half, or four months and a half, of pregnancy, the absence (or inability to detect the presence) of the fœtal tictac is quite the exception.

17. They may be discovered at a period of pregnancy, when all the other modifications or phenomena lead to but a mere probability.

18. Their absence, ascertained upon several occasions by an experienced observer, will never mislead; and, in every case, this circumstance has a value above that of all the other signs which are usually regarded as indicative of the death of the fœtus.

19. Two double pulsations, distinct and not isochronous, warrant with certainty the diagnosis of a twin-pregnancy.

20. If three such pulsations, each having a peculiar rhythm, were present, it might be possible to recognise a triple pregnancy.

21. The discovery of a fœtal circulation, when at the same time it is known that the uterus cannot contain the product of conception, would suggest the existence of an extra-uterine pregnancy. But, as yet, this is a mere matter of rational conjecture.

22. It is undeniable that, in an immense majority of cases, provided the pregnancy be sufficiently advanced, it is quite possible to ascertain with precision how the child lies in reference to the inlet of the pelvis, and the relations of its different parts both with the inlet and with the walls of the uterus.

23. For this purpose, it is not sufficient to make out with certainty the existence of the double pulsations at any one point of the uterus; the exploration must be general, for it is necessary to ascertain what I have called the *summum* of their intensity.

24. In order that the diagnosis, founded on stethoscopic results, may have all the value that can be desired, it is proper that the examination be made, or repeated, at the time when an opinion is given; for it is not impossible, though unquestionably rare, that the fœtus when well developed may, by its own proper movements, modify its position and even its presentation.

25. As it is necessary, in ascertaining the progress of labour, that an examination be every now and then made by the vagina, so, but in a yet higher degree, it is indispensable, as respects the child, to discover at short intervals the state of its circulation.

26. The modifications, which may occur in the double pulsations, ought to be studied with attention; for, when they exceed certain limits, the life of the fœtus will be found to be compromised.

27. These modifications may be only transitory; and then, either they may be sufficient to produce a hurtful influence on the life of the child, and delivery alone can save it; or they may have created merely a temporary distress, which will speedily pass away and not require the interference of art.

28. However this may be, nothing will better enable the physician to appreciate these different conditions than the state of the fœtal circulation.

29. The changes or modifications, which indicate something serious, consist more especially in an irregularity of the pulsations, and in a marked diminution of their frequency and force.

30. Excessive acceleration never indicates anything amiss; the beats may exceed 200 in a minute, and the child remain quite healthy.

31. The value of such-like facts will be appreciated when the choice of

an operation has become necessary—a choice that must be founded on the combined interests of mother and child.

32. In the same measure as the discovery of the uterine souffle is of comparatively little value in practice, so that of the fœtal cardiac pulsations possesses very great importance: no other sign or phenomenon can replace it as a means of exact diagnosis.

On the subject of the seventh of these propositions, we find the following remarks:—

"As the double sound or tictac has its seat in the heart of the fœtus, which, in the early months of pregnancy, enjoys a great mobility within the uterus, we can readily understand how it may be heard at very different spots in the hypogastric region at different times. Moreover, the considerable extent, over which it may be perceived, must serve to produce considerable differences in this respect. It may be said in a general way, that it is in the part of the organ which corresponds to the heart of the child that it will be found, and that from this spot it radiates—diminishing in intensity—over a space of from two to four inches square. In some cases, it extends over a large space at the full term of pregnancy; and it is not impossible but that it may be detected at every point of the uterine globe that is accessible to the stethoscope. The following, however, may be taken as the usual state of things. When it is first perceptible from the twelfth to the sixteenth week, it is by the fundus of the uterus, which then begins to exceed the pelvic inlet and can alone be explored, that it is transmitted. Most usually, it is by no means a matter of indifference in what way the pressure with the stethoscope is made. It is almost always necessary to give it a vertical direction, *i. e.* parallel with the axis of the uterus itself. As a matter of course, the bladder should always be quite empty at the moment of exploration.

"In proportion as the uterus rises above the pelvis, the preceding considerations are less frequently applicable, and then it is over one or other of the lateral regions of the organ that we may expect to find the sound; although it may be perceptible also upon the median region. According to my experience, however, this is of much less frequent occurrence than at an earlier stage of pregnancy. The explanation of the difference is probably this. From the fifth to the sixth month, the size of the fœtus is much more considerable, and the quantity of *liquor amnii* is proportionately less; hence there is more fixedness in the relations, and more facility to keep the uterine parietes in contact with certain regions of the fœtal ovoid. We shall presently see that these are not all equally capable of transmitting the cardiac sound.

"But it is especially in the three last months of gestation that the influence of the conditions just mentioned becomes evident; and this in such a marked manner, that, if the exact position of the fœtus were previously known, one might with almost complete certainty indicate the region upon which the stethoscope should be placed. The results of an extended series of observations warrant me in asserting that, in a large majority of cases, the double sound will be found along the trajet of a line which, extending from the left antero-superior spine, would terminate in the umbilicus; that, much less frequently, it will be found over the correspondent points, on the right side; and that, much more rarely still, it will be found above the umbilicus sometimes on the left and at other times on the right side. It is at this period also that it may extend from its point of departure over a considerable space, and that, in some instances, it is even discoverable over every accessible point of the uterus."

Dr. Depaul has some very interesting observations on the independence of the fœtal and maternal circulations, that well deserve to be attended to; but we must refer our readers to his work for particulars. We cannot,

however, pass them over altogether without noticing briefly a very interesting case, related by our author in illustration of this point.

A young woman, in the advanced stage of pregnancy, fell off the chair on which she was sitting at work, and, after two or three deep-fetched respirations, expired. In the course of about four minutes from the time of the fatal event, Dr. Campbell, an *interne* of the Maternity, ascertained that life was quite extinct; there was no respiratory murmur to be heard, nor any pulsation of the heart. In the report of the case, communicated by this gentleman to our author, the following particulars are given:—

"Immediately after the death of the mother, the presence of a living child in the uterus was ascertained; the pulsations of the fœtal heart were distinctly heard, their numher being from 120 to 140 in the minute.

"While I was examining the chest of the mother, my hand, resting on the abdomen distended by the uterus, felt some active movements; the shock was pretty strong, brusque, and repeated at short intervals. Auscultation enabled me at this time—about six minutes after the death of the woman—to detect the sounds of the fœtal heart in the left iliac fossa: they were regular, tolerably strong, and did not exceed about 100 in the course of the minute. Two or three minutes subsequently, they had fallen down to between 60 and 80.

"The circumstance of the rapid diminution of the fœtal pulsations, added to the brusque character of the active movements of the fœtus (a truly pathognomonic character of compromised vitality) on the one hand, and, on the other hand, the certainty of its almost immediate death if something were not done without delay, induced us to have recourse to the Cæsarian section, which was commenced ten minutes at least after the death of the mother. The child, although living when extracted, did not make an inspiration until 25 or 30 minutes had been spent in insufflation, by means of a laryngeal tube. It was only when a large quantity of amniotic fluid had been rejected by the mouth, nose, and tube itself, that it began to utter a few feeble moans; nor was it relieved from the state of general cyanosis and insensibility, before a small quantity of blood was allowed to flow from the divided end of the cord. It is now fifty days old, and seems to thrive very well."

From the consideration of this and such-like cases, as well as from other circumstances (of which one of the most remarkable is the fact that, whilst the inhalation of ether has the uniform effect of accelerating the pulse of the mother, it scarcely, if at all, affects that of the fœtus), Dr. Depaul is of opinion that it may be fairly regarded as proved "that there is no direct connexion between the maternal and the fœtal circulation,—that derangements of the former will sometimes persist for a considerable time without reacting in any manner upon the latter;—that nevertheless, when these derangements are of a nature to produce profound modifications, inappreciable though these may be by their physical characters, in the blood of the mother, they exert an influence, after a variable time, upon the fœtus, and this influence is evidenced first by an increased frequency of the fœtal pulsations, and then by a dimunition of their frequency, which is a sign of much more serious import;—lastly, that the moral emotions of the mother can act upon the child only consecutively, and through the intervention of the blood.

In reference to the 18th position, the following details will be read with interest. In 17 out of 26 cases, in which the period of pregnancy was from the end of the third to that of the fifth month, and in which our author could detect no fœtal pulsation, the result proved the correctness

of his diagnosis. In the majority of the remaining cases, the pregnancy had not exceeded the fourth month. As to the cases, he adds, in which he had once heard the sound, the negative result obtained subseqnently never misled him: the death of the fœtus was always demonstrated ultimately.

Out of 67 cases, in which the women were in the four last months of gestation, the absence of the fœtal pulsations, taken as the evidence of the death of the child, disappointed our author's diagnosis only on three occasions. He very properly reminds his readers that, in order that the examination may be regarded as conclusive, not only must the physician have had very considerable experience, but the exploration also must have been repeated on several occasions on two or three different days.

There is a case reported by M. Naegele, in the Medicinische Annalen for last year, that bears upon the proposition 20: it is entitled, *Results furnished by Auscultation in a case of Triplets.* A woman, in labour with her first child, was ausculted by M. N., who found in the left hypogastric region the double beat of the fœtal heart, as well as a simple blowing sound of the umbilical cord, and in the right hypogastric region two other double pulsations. Upon two observers examining with the stethoscope, one on oach side, at the same time, it was found that the beats of the fœtal heart and of the cord were rather more frequent on the right than on the left side (152 to 144). Subsequently, after the right fœtus had been in brisk motion, the heart was found to give 160 beats, and the cord only 144; there was thus a difference of 16 beats in the minute. After the delivery of a first child, auscultation was again practised; and still the pulsations of two hearts were heard distinctly, the one set in the left hypochondriac, and the other in the right umbilical, region. The head of the child, which next presented in the vagina, was found to be in the second position; consequently the cardiac pulsations on the right side belonged to it. After its expulsion, the fœtal pulsations, indicating the presence of a third child, were still readily to be heard: this child was born without the assistance of art.

Although the presence of three children was only made out after the birth of the first one, the case still serves to show how much information auscultatory means are capable of affording. It should be mentioned that there was no appreciable difference in the uterine souffle, either as respects intensity or the extent of space over which it was perceptible.

Had our space permitted, we might have felt inclined to have given, at some length, an abstract of Dr. Depaul's observations as to the possibility of ascertaining the position of the fœtus in the womb by attending to the exact spot where the tictac is the loudest, and where, consequently, the heart of the child is situated. All that we can do is to make one or two very short extracts from his elaborate narrative.

After alluding to the inapplicability of the test in question in the first five or six months of gestation, in consequence not only of the insufficient development of the uterus, but also of the small dimensions of the fœtus and its extreme mobility in the *liquor amnii*, he goes on to say:—

"But at seven months the relations of the child to the uterus are much less variable; the great axis of the child has, from this time, such dimensions as rarely permit one of its extremities from taking the place of the other. The movements

in the line of this axis are somewhat more frequent and more easily understood; but these become more rare as the full period of gestation approaches, so that at the end of the eighth month they are mere exceptions which are very seldom met with."

Still, however, cases do occur, where the child seems to have made a complete somerset within the uterus, at an advanced period of pregnancy. Allowance must therefore be made for such occasional occurrences.

To understand the following remarks, let the abdominal uterine globe be supposed to be divided into four parts by a transverse line passing a little below the umbilicus, and crossing a perpendicular one drawn from the epigastrium to the pubes.

"When the fœtal pulsation is found in the left inferior region, the presentation will be that of the head, and the back of the child will be turned towards the left side; when in the right inferior region, the presentation will be the same, and the back of the fœtus be turned to the right; when, on the contrary, the sound is heard in the left superior region, the pelvic extremity will be the part most depending, the back being turned to the left; when in the right superior region, the position will be the same, with the exception of the back being directed to the right side."

With the view of rendering his observations on this hitherto little explored subject of obstetrical enquiry better understood, our author has introduced several illustrative wood-cuts representing the relations of the fœtus in different positions in the pelvis.

We need scarcely say that we think very highly of Dr. Depaul's volume, and that we recommend it in the strongest terms to the attention of the profession in this country.

On the System of the Great Sympathetic Nerve. First Part. By *C. Radclyffe Hall*, M.D. Republished from the Edinburgh Medical and Surgical Journal. 1847.

An additional attempt to solve the enigma of the Great Sympathetic is not a very promising topic for the generality of medical readers. We have, however, no wish either to underrate the importance of the subject or to depreciate the merits of Dr. C. R. Hall's memoir; but if this paper had not been brought more directly under our notice in the shape of a separate pamphlet, it might, without any injury to science, have been left in the pages of the periodical in which it originally appeared, for the consideration of the professed physiologist, to whose acumen the experiments and reasonings of the author are more particularly suited. A few extracts will enable our readers to understand the general views here promulgated; those who require more minute information, must refer to the work itself.

We have always ourselves selected the cephalic ganglia as the type of the great sympathetic; because here, as in so many analogous instances, Nature has offered for our investigation an analysis of what, in other parts of the same system, is obscured and involved. For instance, in these scattered ganglionic masses the observer, owing to the natural severation

of the motor and sentient elements of the cranial nerves, which in the case of the spinal nerves are bound up together, is enabled to detect a undamental fact connected with the sympathetic, namely, that its ganglia receive from the cerebro-spinal system both motor and sentient branches; he can further perceive that each of these isolated nodules of gray matter is brought into anatomical relation with the rest of the sympathetic by special filaments of communication, often running a long and tortuous course to effect the junction. Similar reasons, joined to the comparative facility of access and to the ease with which the results of experiment produced on the eye can be observed, have induced Dr. C. R. Hall to choose the ophthalmic ganglion for investigation—"the lenticular ganglion represents the ganglionic system in miniature; what can be proved with respect to it, may justly be taken as a safe index of the rest."

The following observations on the varying states of the pupil are worthy of attention:—

"The pupil after death in every animal is of the medium size natural during life. The exceptions are numerous, and depend upon the state of the iris at the time of death, and upon the action of external agents after death. If the pupil is very large when death occurs, as in slow death from hanging, or prussic acid, it will retain this size for some time after death; as the body cools, the pupil lessens, but is seldom reduced to the medium size. But when death from prussic acid is instantaneous, the largely dilated pupils will contract in a few minutes after death to less than the medium size, and afterwards enlarge again. The varying degree of contraction of the *sphincter iridis* at the time of coagulation of the blood, (which *does* take place, at least in small vessels, after poisoning by prussic acid,) may perhaps account for this. If death occurs with the pupils contracted, these enlarge after a time, but do not generally attain the ordinary medium size. In practising operations on the eye of the human subject after death before the animal heat has vanished, a dilated pupil always contracts as the aqueous humour escapes. This, like the motion of a narcotised iris in extracting the lens for cataract, and like the contraction of the previously dilated pupil of a drowned kitten on exposing the animal to the heat of a furnace, in an experiment performed by Haller, is probably due to physical influence." P. 3.

The author has corrected some errors of former anatomists, such as that the ophthalmic ganglion is absent in the rodentia; on the contrary, he found it beneath the optic nerve, but so minute as to require great care in the dissection, in the rabbit, squirrel and guinea-pig. The conclusions drawn from the various experiments upon the ophthalmic ganglion and the nerves connected with it are thus set forth:—

"1. The third nerve is the only direct motor nerve for contraction of the pupil in dogs and cats.

"2. The action of the third nerve, as far as concerns the iris, is mainly under the control of the visual nervous tract.

"3. As both the third nerve and some portion of the visual tract must inevitably be injured in performing the experiment of dividing the fifth nerve after Magendie's plan, the experiments of that gentleman cannot be considered as evidence that the fifth nerve is either directly or indirectly the nerve which presides over dilatation of the pupil in dogs and cats; nor over contraction of the pupil in rabbits and guinea-pigs.

"4. In animals in which division of the fifth nerve causes contraction of the pupil it does so by excito-motory action through the sixth nerve, which in these animals supplies the iris in conjunction with the third. The sixth nerve, however,

not entering into the formation of the ophthalmic ganglion, which in these animals is extremely small.

"5. In these rodentia, pain of any kind, whether produced by irritation of the fifth, or of any other sensational nerve, will cause more or less contraction of the pupil at the instant, attributable probably to excito-motory action of the iridal fibres of the sixth nerve.

"6. Irritation of the fifth nerve, or of any other sensational nerve, in the cat, and dog, and pigeon, so long as it does not affect the brain to the extent of producing vertigo, *nor the visual sense in any other way*, has no immediate influence over the size of the pupil. Hence, although the fifth is an excitor nerve to the *sphincter iridis* in the rabbit, it is neither directly an excitor nor a motor nerve to it in the dog and cat.

"7. As the third nerve can always, under favourable circumstances, be made to influence the movement of the iris, *immediately* on the application of irritation, and as the iridal portion of the third nerve, in all animals in which theiris is active, passes through the ophthalmic ganglion, it follows that *the ophthalmic ganglion offers no check to the transmission of motor influence along the motor nerve fibres which pass through its substance.*

"8. As irritation of the fifth nerve does not in any animal affect the action of the iris *after* divisions of the cerebral connections of all the other ocular nerves, and as some portion of the fifth nerve always enters into the ophthalmic ganglion, it follows, *that the filaments of the fifth nerve have not the power of affecting those of the third nerve during their course together through the substance of the ophthalmic ganglion;* or, in other words, *that the lenticular ganglion is not a centre of excito-motory action to the iris.*" P. 25.

Some interesting cases are related with the object of showing that, although the fifth nerve, as we have seen in the above extract, is neither a motor, nor, with some exceptions, an excitor nerve to the iris, yet that it does in some way so affect the internal structures of the eye, as to influence the state of the pupil, inducing under some circumstances contraction, and under others dilatation. The author offers, as a mere hypothesis however, the following as the explanation: "It is allowable to conjecture that the small supply of fifth nerve which enters into the lenticular ganglion, passes through to the retina exclusively; that when in the ganglion, it can affect, during its own conduction of nervous force, the action of the ganglionic vesicles, and consequently call forth the influence, be that what it may, of the ganglionic nerves. Begging the question for the moment, that one effect of the ganglionic influence is the afflux of blood to the part, we can understand how the stimulation of the retina by intense light may cause not only instantaneous contraction of the pupil through the third nerve, but also an altered state of circulation through the retina, through the medium of the fifth nerve, lenticular ganglion and ganglionic nerves, producing painful confusion of sight, or in other words, dazzling and aching in the eye. The unusual supply of blood furnishing at the same time the means of recruiting the over-excited retina. We can understand how one amount of stimulation of the fifth nerve may have such a slight effect on the vascular coat as may merely stimulate the retina, and thus cause contracted pupil (*e. g.* ulcer of cornea, strumous ophthalmia): how a greater amount of stimulation suddenly applied, or the same amount greatly prolonged, may cause so much disorder of the retinal capillaries, as to produce temporary blindness and dilated pupil (*e. g.* severe *tic*. The

fifth nerve being in the one case indirectly an excitor of contraction; in the other, of dilatation of the pupil." P. 36.

Proceeding from the consideration of the ganglion of the orbit, the author enters upon a much more extended question—"the real functions of the ganglia." Although it is certain that nutrition, whether normal or abnormal, may be effected independently of a nervous system, as we see in vegetables, yet it is conceived that nervous force is required in animals to regulate and modify the process, by determining the amount of organizable material to be acted on; or, in other words, to determine the quantity of blood that is sent to the organ implicated.

"Modern researches have proved that every proximate constituent of a living body contains, complete within itself, the means of performing its specific action, so long as the requisite conditions are observed. The most important of these conditions is the due supply of organizable material. To regulate the supply according to varying demands, and to ensure order in the discharge of complicated functions which are at once mutually related and yet entirely different, there must be some means of harmonising various organic actions, and regulating the supply of blood according to the requirements of the different organs. No other system except the nervous presents the characters fitted for effecting this superintendence." P. 47.

It is subsequently proved, according to the author's belief, that the ganglia are the centres which effect this regulation; or that they furnish the true organic nerves. As, however, there is no novelty in this opinion, it is proper to state in what the peculiarity of Dr. C. R. Hall's theory consists. He does not believe that the sympathetic concurs in the functions of the vegetative life by exciting the heart's action, or by inducing any contraction of the blood-vessels, or by being necessary to the peristaltic motion of the intestine, for all these phenomena, he contends, are manifested independently of the ganglionic nerves; the influence of the ganglia is, according to the author, exerted on the ultimate molecules of the various organic tissues, and, by modifying the natural affinities possessed by these particles, the act of nutrition may be powerfully affected, and, as a consequence, the action of a muscle, of a nerve, or of a gland. The following passage is the best we have been able to select as illustrative of these views, after taking some trouble to find something more precise and clear:—

"As an hypothesis, therefore, capable of explaining all phenomena referrible to the nervous system, so far as physical action is concerned, and apparently open to fewer objections than any other, we infer that the nervous force can stimulate every living molecule endowed with a certain function, to the more active discharge of that function; that it can consequently excite, variously modify, or greatly derange all vital actions; that muscular contraction, cell-metamorphosis, the evolution of animal heat, are influenced by the nerves in precisely the same manner, the difference in result being due to inherent difference of vital endowment in the molecule subjected to the nervous influence; that the varied fulness and therefore the calibre of the capillary blood-vessels is secondary to, and dependent upon, the activity of the molecular changes going on in their contents, and under the control of the nerves only in so far as these affect the vital actions of the blood." P. 53.

In considering this hypothesis, it is necessary to recollect that the author is here referring to the proper elements of the sympathetic, the cells

and vesicles, namely, of its ganglia and the peculiar organic nerves proceeding from them, altogether excluding the white tubular nerves derived from the cerebro-spinal system, and which, it is now well known, although passing through the ganglia and along with the gray filaments originating from those nodules, are in essence foreign to the sympathetic.

We must leave our readers to form their own estimate of the value of Dr. C. R. Hall's opinions, observing, however, that this idea of the nervous force playing in organic matter a part, somewhat similar to that of electricity in organic bodies, is not novel; for theories of this kind have, under various names and with different modifications, been often advanced, but never satisfactorily established.

In conclusion, we would, in no unfriendly spirit, advise the author, in any future communication, to be more cautious in advancing mere hypotheses, which, having no legitimate ground for their support, are detrimental rather than serviceable to the real progress of science; and we should further suggest the advantage of greater attention being paid to clearness of language and expression. It would not have been a matter of difficulty, were we so inclined, to point out instances in the present paper in which both these desiderata have been neglected.

Observations on the Treatment of Lateral Curvature of the Spine, pointing out the Advantages to be gained by placing the Body in a Position to produce Lateral Flexion of the Vertebral Column, combined with the after application of Firm Mechanical Support. With Wood-cuts. By *E. F. Lonsdale*, F.R.C.S.E., &c. &c. 8vo. pp. 116. Churchill: London, 1847.

Every medical man knows that Lateral Curvature of the Spine is of much more frequent occurrence among the young females of our middle and upper classes than it ought to be; but we are certainly not prepared to believe, with our author, that "some deviation from the natural erect line, causing a greater fulness on the right than on the left side, both in the ribs and shoulder," exists "in by far the majority of them." It is unnecessary to allude to the usual predisposing and producing causes of this, unquestionably too common, deformity. The convexity of the curvature is, as just stated, very generally on the right side. The much greater use of the right arm, and indeed of the whole right half of the body, in most exercises, is probably the main cause of this. Mr. Lonsdale suggests that the greater expansion of the Lung on the right than on the left side may have also something to do with it. Speaking of the injurious effects of stays, he remarks:—

"The constriction of the stays at first tells equally on both sides of the chest; but will it continue to do so after being allowed to act for a length of time and in an increasing degree? I say, no; for the simple reason that the ribs on the two sides do not offer the same resistance, owing to the difference in the size of

the two lungs; the left side of the chest contains less air than the right, which it must, owing to the left lung being smaller than the right. Combined with this, there is the position of the Liver on the right side to be considered, to which I shall refer again, as a cause tending to support the right side of the chest, to render the ribs less liable to compression on this side than on the left. The constriction, then, produced by the force of the stays, must act more on the side where there is the less resistance, and this, as already stated, is the left; the consequence of which is, that the ribs of this side will become more compressed, and the capacity of the lung be also diminished." P. 11.

The supposed influence of the Liver existing on the right side is thus more largely explained:—

"I would ask the question, may not the liver, from its position, give a degree of support to the under surface of the ribs of the right side, sufficient to cause a difference in the resistance of the two sides of the thorax? Does it not mechanically, to a certain extent, prevent the ribs from being depressed, while on the left side no such support or resistance exists? Let any one stand in the erect position, and try to flex the spine laterally, he will find he can do so with much greater facility, and to a far greater extent, on the left than on the right side. May not this be owing to the presence of the liver on the right, and to its absence on the left side? It may be said, that the liver will be depressed, and so the opposition removed. I believe it will not; for any movement of the spine which bends it to one side or to the other, is principally produced by the action of the abdominal muscles; they necessarily press against the viscera at the same time, and so oppose the displacement of the liver; and not only oppose it, but in extreme action tend to press it upwards. If so, it is quite intelligible how the liver must give support to the under surface of the ribs, with which it is in contact." P. 20.

The description of the symptoms, the origin and progress of the deformity, is minutely and faithfully given by Mr. Lonsdale, and may be read with much advantage by those whose attention has not hitherto been directed to the subject.

With respect to the principles of Treatment recommended by our author, the reader will be best enabled to judge of them from the following remarks:—

"First: there is weakness of the vertebral column; it has then to be artificially supported. Second: there is displacement of certain bones; the vertebræ, the ribs, scapulæ, and clavicles, which lose their natural relative position to one another, at the same time that the ligaments on one side become shortened; these bones then have to be replaced, and the resistance of the ligaments has to be overcome. Finally, there is irregular muscular development of the two sides of the body, existing both as a cause and as an effect. These points then have to be considered; and the plan of treatment which will most effectually gain the desired end, is the one to be pursued. My own conviction is, that they must, in most cases, all three be combined; that no one of them will be effectual if employed alone; that is to say, it is no use supporting the spine without the displaced bones are mechanically acted on with the intention of replacing them, at the same time that means are taken to overcome the resistance of the ligaments; and that it is no use doing either of these, without the action of the muscles be attended to afterwards, by endeavouring to give increased power where it is deficient. On the other hand, it is of little use attending to the muscular system only, which is done in many plans of treatment; for it by itself is not sufficient to redress the deformity, but in many cases, as stated before, will only tend to increase it, if the spine be not first of all brought out of its curved position; a point which can be easily understood if the origin and the insertion of the muscles

are considered, and the action they will have upon the ribs and spine, when these bones are thrown so much out of their relative position. I shall first explain the means by which the spine can be well and efficiently supported: afterwards, the means to be adopted by which the resistance of the ligaments can be most effectually overcome, at the same time that the bones are pressed in a direction the opposite to that in which they are displaced: finally, the advantages that are to be gained by the exercise of the muscles, and the position best adapted to increase their development, as well as to act upon the spine itself." P. 55.

The spinal apparatus or support used by Mr. Lonsdale is a modification of that recommended by Mr. Tamplin in his work on Deformities, and certainly seems to be exceedingly well suited to its purpose—that of supporting the left or depressed shoulder, and of exercising a degree of steady pressure upon the expanded and projecting ribs on the right side, and through them, upon the convexity of the spine itself. Its construction and mode of acting are well represented in several woodcuts.

While disapproving of the use of any modification of the recumbent position, when continued for months and years, as has been so often recommended and adopted—Mr. Lonsdale adopts it as an adjunct only in the treatment of lateral curvature. But, in the employment of this means, he does not follow the ordinary plan of placing the patient either on the back or on the stomach; in other words, either in the supine or in the prone attitude. He thinks that the position to be adopted may be made a powerful means of redressing the bend of the spinal column, and he reasons in this way. If you wish to straighten a curved stick do you stretch it by pulling upon the two ends in its long axis?—no; you place the convex of the bend upon your knee, and then pull or draw back the two ends. The same principle may be carried out in the case of the curved spine, says our author; and in this way:—

"The patient should be placed on the side on which the projection formed by the curve exists, instead of on the back, and allow the legs, the head, and upper extremities, to fall to a lower level than the trunk; by this means a sufficient power is at once gained, by the simple weight that is then exerted at either end of the trunk, to gradually act upon the spine and to regulate itself; imitating in fact the straightening of a bent rod or stick: no other mechanical means are required; the weight of the legs at the one extremity, and of the head and shoulders at the other, exert a force quite sufficient to redress any slight curvature that may exist, and as much as can be borne, or it may be judicious to apply in severer cases. The object is to stretch the ligaments, and so to overcome their resistance, at the same time that the bones themselves are pressed in a direction the opposite to that in which they have been displaced, and are thus rendered more moveable and more capable of being acted upon, by any apparatus that may be afterwards employed to give them support—points to be more particularly referred to, and which I explain after describing the means by which this position is to be obtained." P. 84.

The couch contrived by our author cannot be properly explained without the aid of his diagram. It is ingenious, and must answer the purpose for which it was planned very well; but we are much disposed to think that the simple plan used by Mr. Alexander Shaw—that of placing a firm pillow or cushion beneath the projecting side of the thorax, the patient reclining upon this side and the pelvis and lower extremities resting on a somewhat inclined plane—must effect nearly all the advantages to be derived from the position recommended. "The simple object is to render the

spine more flexible; to make it more yielding in the opposite direction to that in which it is curved, to overcome the resistance of the ligaments by gradually stretching them; to render the bones themselves more moveable, by pressing against them in the direction the opposite to that in which they have been displaced,—both the vertebræ themselves as well as the ribs; and at the same time to overcome the resistance of the muscles which have hitherto been acting in one direction only,—imitating, in fact, the treatment of the muscles and ligaments in a contracted joint."

To gain these ends, an hour or two's reclination daily in the position recommended is all that is necessary. On the patient's rising, the instrument already described must at once be put on, "*to retain any advantage that may have been gained*, and to support the spine at once after having been thus bent in the opposite direction, making the instrument act more and more every day." So much for the two mechanical means, recommended by our author for *unbending* the curve of the spine, and retaining it in its normal uprightness. Let us now hear what he says respecting the third and last set of means which he describes, that of the exercise of the muscles. At page 80, he expresses himself as anything but favourable to this part of the treatment in a very great number of cases.

"I am opposed to the exercise of the muscles before the curvature has been either completely cured, or as much relieved as the nature of the case will admit of, for the reasons already adduced when speaking of the causes of lateral curvature, namely, that the muscles themselves, after the curvature is once produced, tend, by their action, to increase it; and I more particularly refer to the spinal muscles, both those which are proper to the spine itself, and those which are connected with the ribs; they both draw the concave side of the spine more and more downwards, the more powerful the action they exert. For this reason, then, I should avoid increasing their power so long as any curvature may exist and the spine itself be yielding, and therefore should not combine it with the stretching position, did I think that one the best adapted to mechanically overcome the deformity. This view appears to me to be rational, and I am not aware it has been before advanced, except by Mr. Tamplin, to whose lectures I have before referred." P. 80.

And a few pages further on, he remarks—"that to exercise the muscles of the back, including those of the upper extremities, without mechanically relieving the spine of its superincumbent weight, does more harm than good, and that it increases the deformity rather than relieves it; and that what is termed general increase of muscular development, by indiscriminately employing gymnastic exercises, is not to be recommended. I now more particularly refer to those cases of lateral curvature, where the deformity does not depend on weakness of the muscles as the principal cause, but upon confirmed and mechanical displacement of the bones of the vertebral column and ribs."

While admitting that a few cases of lateral curvature of the spine may be cured by "attention to muscular exercise—properly employed," (including, we presume, frequent rest in the horizontal posture), Mr. Lonsdale affirms that, in the majority of cases, this plan of treatment will fail: "the muscles of the weak side may have been increased in power, but the deformity may not have been relieved." The only cases—and they are declared to be *few* in point of number—in which muscular exercise is of

much service, are said to be those occurring "in girls of spare habit, in whom the whole muscular system is weak; where the vertebræ are thinly covered with the muscles, the spinous process being prominent throughout the whole length of the spinal column; the scapulæ on both sides projecting and wanting their close adaptation to the ribs, owing to the absence of sufficient power in the muscles to keep them in their natural position: where the curvature of the spine is general throughout its whole length, and can be easily altered in one direction or the other, the bones being but loosely connected owing to deficiency of strength in the ligaments; the shoulder of one side being higher than the other, though not to any marked extent, and the ribs of the left side, though less convex than on the right, still are not compressed to an extent sufficient to cause a hollow beneath the left scapula; lastly, where the curvature has existed for a short time only, and will admit of being easily redressed by pressure made with the hands."

Now compare such cases with another set, in which the exercise of the muscles is declared by our author to do more harm than good; viz.:

"Those where the curvature, although it may be confirmed, may not yet be fixed; by which I mean, the deformity may be very great, but yet there may be sufficient yielding in the spine to allow of it being moved or acted upon when pressure is made forcibly against it; where the ribs are more increased in convexity on the right side and more depressed on the left, with a corresponding projection and sinking of the scapulæ of the two sides, causing also the corresponding difference between the level of the two shoulders. *Any increased power given to the muscles in these cases, without attempting mechanically to support the ribs and spine, and to support the left shoulder*, which, by its weight, is tending to bear downwards and to increase the concavity, *will*, as before stated, *only keep up the deformity, and in the majority of cases increase it.* This opinion, I am aware, is at variance with that which is advocated by those who look to increased muscular development as an important point in the treatment of these cases; it is formed upon the reasons I have already given, and from my own experience I believe it to be a correct one. The grand point to attend to, is to bring the spine as nearly as possible into its normal erect line; to relieve the compressed ribs of the left side, by supporting the shoulder which is bearing upon them with its weight, at the same time that the opposite or convex side is pressed upon by a force that gradually admits of being increased." P. 102.

It will be observed, from the passage we have had printed in Italics, that our author somewhat qualifies his opinion as to the hurtful effects of exercises even in these cases. As a matter of course, when the deformed vertebral column has become, from the long-standing of the case and the age of the patient, rigidly fixed in its altered position, no rational man can expect that any sort of muscular exercise can ever correct the evil. All that can then be done is to have recourse to a well-devised mechanical support, frequent reclination in the horizontal position, and great attention to the general health.

The exercise, we should have remarked, recommended by our author, will be best understood from the following passages:—

"The position that I believe to be the best, is one that throws the whole spine more backwards than forwards; which tends to redress the curvature, at the same time that the muscles of the spine are brought actively into play; and the following is the one I should recommend. Attach two pullies or hooks (and

pullies answer the purpose better) to the ceiling of the room, or to an artificial framework placed in some situation about two or three feet above the head. The patient is to stand in a position, that the pullies may be about a foot and a half or two feet behind her. She is then with two hands to take hold of a stick or spindle, to which two ropes are attached, and which pass through the pullies, having weights fastened at the other ends sufficiently heavy to require some exertion to draw them up, the weights of course being increased or diminished according to the strength of the patient. I generally find six or eight pounds in each quite enough, and as much as the patient can raise without over-fatiguing herself. The ropes should be long enough to allow her to incline the body forwards on the hip-joints, without bending the spine itself, drawing the weights upwards as much as she can, keeping the arms extended above the head all the time, and bringing them as far forwards as the inclination of the body will admit of, without the necessity of moving the feet from the position in which they were originally placed. The body is then to be brought into the erect position again, by raising the trunk on the hip-joints, and letting the weights fall, and so to pull the arms behind and above the head. It may be as well to tie a knot in the ropes, to check the fall of the weights, to prevent the arms being strained beyond the point of extension to which they can easily be carried behind the head." P. 108.

This exercise should often be taken with the left arm only. He adds:—

"The grand principle I wish to lay down, is, to exercise the muscles with the arms placed above and behind the head, while the body is kept in the erect, and not in the horizontal position. If this principle be well carried out, and a strong and efficient spinal support be employed at the same time, I believe that all *slight* cases of lateral curvature may be cured without the necessity of employing couches at all." P. 112.

In taking our leave of Mr. Lonsdale's work, we cannot but express our opinion that he seems disposed to attach a somewhat exaggerated importance to the use of mechanical means, and to undervalue the benefits to be derived from the judicious employment of muscular exercises, in the treatment of lateral deformities of the spine. With this qualification, we think that many of his suggestions are sound, and well deserving of the attention of the profession.

CHOLERA, DYSENTERY, AND FEVER, PATHOLOGICALLY AND PRACTICALLY CONSIDERED; OR THE NATURE, CAUSES, CONNEXION, AND TREATMENT OF THESE DISEASES IN ALL THEIR FORMS. By *Charles Searle*, M.D., late of the E. I. Company Madras Establishment, &c. &c. Pp. 120. London: Churchill, 1847.

DR. SEARLE is surely a very fortunate man above his fellows; for his work has gained for itself a very wide circulation, and the prestige of a favourable reception, ere it yet appeared before the public, or passed through the ordeal of the press. "The Court of Directors of the East India Company, with their usually enlightened concern for the millions under their paternal dominion, and regard for the welfare of the many in their employ, have enabled me—with a liberality demanding this public acknowledgment—to present to each of you [the medical officers of their service—*Rev.*] a copy of this publication, which has for its chief object the dissemination of IMPROVED PRINCIPLES of treating that scourge of India, and it has been said—'*opprobrium medicorum*'—the Cholera."

Emboldened by so flattering a compliment, the author "calls upon the press generally to aid him in its circulation, at the same time that he calls upon the profession and all mankind either to confute him in argument, or to exhibit by facts, why the practice enjoined and the treatment recommended should not be accepted as deserving universal adoption." Now, without regularly entering into the lists with so proud and confident an opponent—the limits of our space would not allow us to do justice either to ourselves or to him—we shall do little more at present than select a few passages from his message, adding but a word or two of our own in the way of remark.

Nature of Malaria.—"Sulphuretted hydrogen, one of the offensive gases issuing from sewers, and a product of the decomposition of animal and vegetable substances, is so truly poisonous to the animal system, that a bird, or other small animal, exposed to an atmosphere containing but one fifteen-hundredth part of this gas, dies almost immediately from its effects; and a horse has been killed by exposing it to breathe an atmosphere containing but one-250th part. That malaria, which is a compound of this and some other gases equally noxious, developed by the decomposition of animal and vegetable substances, wherever they exist, may be truly affirmed to be a poison; and which, under certain conditions of the atmosphere and states of the system, will produce cholera; and under other circumstances of exposure to its influence will occasion typhus fever, dysentery, scarlet fever, erysipelas, rheumatic fever, influenza, or other modification of fever of a remitting type with local affection—either of the brain, lungs, or abdominal organs." P. 28.

In a previous page, 484, we have questioned the propriety of regarding sulphuretted hydrogen or any chemical gas whatsoever as a necessary ingredient in the malaria or miasm that produces fever, whether of a periodic or of a continued type. We know of no experiments or observations that at all warrant the idea, more especially in reference to Intermittent or Remittent fevers, and still less as respects Scarlet fever or Rheumatism. Dr. Searle does not seem to be quite satisfied himself as to the origin and morbific effects of malaria, if we may judge from the following remarks:—

"We must not," says he, "confine our ideas to the immediate or direct decomposition of such (animal or vegetable) substances, issuing from sewers, drains, and other foul sources. We must not, however, confine our ideas to the immediate or direct decomposition of such substances, or to the sources that I have mentioned exclusively; the exhalations from marshes, or the paddy grounds of India, from jungle or forest, as well as from the uncleanly persons of both men and animals, or the deteriorations of the air by respiration in crowded apartments, and imperfectly ventilated or confined situations, are quite equal, in certain conditions of the system, to produce the same effects, viz., cholera, or, as I have before said, under other circumstances, having the same common origin, and being intimately associated in character; and hence, the latter in India, and the former in Europe, frequently becomes the sequel of the former affection." P. 35.

Are we to understand from this that exhalations "from the uncleanly persons of both men and animals, or the detoriations of the air by respiration in crowded apartments," ever produce a case of genuine Remittent fever? If this be the meaning of our author, we must entirely dispute the assertion, and call upon him for his proofs.

Among the causes of Epidemic Cholera is enumerated a thunder-storm, and its *modus operandi* is thus attempted to be explained analogically:—

"A thunder-storm may also be considered such a cause, as evinced by its capability of addling or destroying the vital qualification of eggs; or arresting the process of fermentation, and souring beer, exposed in a bad cellar to its influence; or, as it is said sometimes to do, to kill the fish in a pond—effects which are known to succeed a thunder-storm of even half-an-hour's duration. Now

as the vitality of fish is dependent upon certain chemical changes which are going on in the body of the animal, which are common to the rest of the animal creation, and the incubation of the egg is dependent upon changes of a like character, and the fermentation of beer also dependent upon the same, and which, in either case, are effected by the agency of the oxygen of the air, in its combination with the carbon of the blood of the animal, or that of the white of the egg, or of the carbon of the sugar of the beer—we are naturally led to infer, seeing the same chemical process is going on in man's system, and upon which his life is dependent, that this process may be arrested or impaired by the same cause, or some analogously suddenly altered electrical condition of the atmosphere—and the disease, as a consequence, be induced, in persons predisposed by previous derangements of health, or subjected to a greater amount of exposure to the influencing cause, or possessing a greater susceptibility of system to be affected by it." P. 107.

In the Treatment of Cholera, Dr. Searle seems to regard *calomel* as an all but specific remedy, not only from its potent action on the hepatic functions, but also as a direct antidote to the morbific cause. His own words are—

"With respect to the first-mentioned indication (restoring the liver's function), universal experience testifies, that calomel has a direct and immediate exciting effect on the liver, increasing its secretion and the flow of bile into the bowels; and further, universal assent will be given by the profession to the fact, that it not only excites secretion of the bile, but all the secretions; and if it excite all the secretive organs, it must necessarily act generally upon the system, and excite all the functions, including those of the heart and brain. That it does so, thirty years' experience justifies me in confidently asserting, the pulse manifesting its exciting operation. And further, as it can only thus operate in admixture with the blood, into which it must be admitted by absorption from the stomach, it must of necessity operate, as it is a stimulant, as an antagonist agent also, in supercession of the depressing influence of the poisonous cause of the disease; and, if this be the case, it is a remedy to which we might, under ordinary circumstances, apply the term specific in the cure of this disease; and, as the fruit of all my experience, I fearlessly aver, that it is as much so as it is possible any single remedy can be." P. 49.

So bent is he upon affecting the system with Mercury, that, when calomel appears to have little effect, he recommends a solution of corrosive sublimate to be given both by the mouth and in the way of injection—an eighth part of a grain every half hour, or so, until the pulse improves and excitement becomes developed. He suggests also the trial of mercurial inhalation. "As the simplest mode of practising it, a tile, or brick, being made red hot, and put upon some sand in a dish, may be placed beneath the bed-clothes, and the patient, enclosing his head, may be allowed to breathe the vapour developed by throwing half a drachm of calomel, or red sulphate of mercury, on the heated object: or it may be inhaled from the tube of a funnel inverted over the dish. This might be repeated every hour or two, till amendment takes place."

The profession is not likely to have much confidence in the judgment of a writer who suggests such extravagances as these, nor in the sober sense or honest word of any one who pretends that the early and steady application of any means whatsoever will *in general*, far less *invariably*, prove successful, as Dr. Searle does not hesitate to assert, in the cure of so lethiferous a disease as Pestilential Cholera.

As to our author's views respecting Fever, that "will-o'-the-wisp," as he calls it, and whose proteiform disguise has, in his opinion, so baffled the efforts of all writers to reveal its true nature, until he took the subject in hand himself, it is far from being easy to make them out with any degree of precision. We must therefore beg our readers to consult his work for themselves, if they wish to know more about them.

REMARKS ON VIVISECTION AND ON CERTAIN ALLEGATIONS AS TO ITS UTILITY AND NECESSITY IN THE STUDY AND APPLICATION OF PHYSIOLOGY. By GEORGE MACILWAIN, F.R.C.S., &c., &c. 8vo. pp. 27. London, 1847.

THIS is an excellent pamphlet, well argued and very gracefully written. It contains much that deserves the serious attention of the truly philosophic, as well as of the purely benevolent, physician. The author, not satisfied with protesting against the cruelty, exposes the worthlessness in a mere scientific point of view, of *many* (we do not say *all*) of the experiments on living animals, which have at different times been adduced in favour of the practice he condemns. He comments more particularly on certain experiments of Hunter, Orfila, Sir A. Cooper, and Sir C. Bell.

It has been often alleged that the great improvement, which Hunter introduced into the treatment of aneurism by operation, was the result of this mode of investigation. That such was not the case, but that the happy thought of tying the artery at some distance from the seat of disease sprung solely and entirely from the observation of pathological phenomena—we allude more particularly to his knowledge of the fact that "the sac often by its increase presses on the sound part of the artery, and becomes the cause of its obliteration," whereas the diseased portion of the vessel, when encircled with a ligature, will seldom unite—and from the simple deductions of legitimate reasoning thereon, is clearly made out by referring to the history of the very case which led Hunter to its adoption. It is given at page 10 of the "Remarks."

Mr. Macilwain's observations on Orfila's experiments, which have certainly been among the most barbarous of the present age, are so good that we could wish to have been able to give them entire. We shall select the most striking portions.

"The nature of the experiments, and M. Orfila's narrative, alike demonstrate the tortures by which they were accompanied. He tells us of animals dying after hours of pain, in the midst of the most horrible sufferings, 'au milieu des suffrances les plus horribles, douleurs les plus atroces,' 'poussant des cris les plus plaintives'—'in most dreadful pain,' 'uttering the most plaintive cries,' and similar unequivocal expressions of sustained agony. It would be some little satisfaction could we assert that these experiments had contributed to the advance of science, but, as will be immediately shown, this is far from having been the case.

"The experiments varied; those chiefly referred to, consisted in exposing an animal's œsophagus (or gullet), by dissection, making an opening into it, and injecting thereby poison into the stomach; the gullet was then tied! and the animal left to itself. In some cases the ligature was removed and more poison, or its supposed antidote, injected. Other experiments consisted in making a wound, and sewing up some poison in it, to see what effect would be produced. Although the influence on science exerted by these experiments is the point with which we have to deal, we may venture to remark that there were philosophical objections to them, on the very threshold—insuperable, because loaded with so many elements of fallacy. I shall very briefly advert to them: 1st. To ascertain the effect of a poison introduced coincidently with the infliction of a severe operation, it is obviously necessary that we should be able distinctly to separate the disturbance occasioned by the operation, from that resulting from the poison; but this cannot be done because the same amount of local injury even in animals, by no means always produces the same consequences.

"2ndly. Every one knows that many substances in nature excite very different effects in dogs, horses, hares, goats, &c., from those which they produce in man-

The torture too (I mean of the operation), as distinct from that resulting from the poison, is objectionable in a philosophical sense, because it places the nervous system in a factitious state, of which system you are really asking the question. What should we think of estimating the natural action of the stomach on any substance in a patient who was ill or in pain from other causes, or in whom we had just tied the œsophagus ! !"

"The highest claim to attention that *any* experiment *on any* animal can have, as regards the physiology of man or its relation to medical science, is its being resolvable into analogy. But analogy to be anything must be real, freed from all things which can distort the implied parallelism; the moment you institute any process which you cannot imitate in man, the parallelism is destroyed, the analogy is at an end; and lastly, no one thinks of reasoning from analogy when he can reason from fact. This we shall show to be practicable in the present instance, on the testimony of Mr. Orfila himself, whose book contains examples of the effects of almost all sorts of poisons on *Man*, and which, in fact, constitute the chief value of his work on the subject. The truth is that accidents, mistakes, murders, and fatal as well as failing suicides, have furnished a series of facts and experiments in man which it is of course impossible, strictly speaking, to repeat in any other animal. With this fact before us, I would ask any man, whatever his sentiments may be on the general question, whether he would, in any case of poison, dare to institute a treatment founded on observation and experiments in animals, whilst he knew of any which had been deduced from observation on man." P. 13.

Mr. M., while he admits the value of M. Orfila's work upon poisons, very truly remarks that its practical value is almost quite independent of the dreadful experiments which it records.

"Let the book," says he, "be read, *excluding first* all the experiments on living animals, and let the conclusion legitimately deducible from the facts enunciated, be noted; then let the experiments on animals be examined, for the purpose of testing their influence on the conclusions which have been noted, and it will be found that they affect them in no way whatever." P. 14.

Who will gainsay the perfect truth of this valuation? If any one does, he has only to compare it with the rival work of Christison, a name of as high repute and authority as that of Orfila, and he will be constrained to admit that the advancement of toxicological science has been infinitely more indebted to the observation of clinical and necroscopic phenomena, and to the results of chemical research, than to all the horrible disclosures of the experimental torture-house.

Passing over our author's strictures on the very unnecessary and most unscientific experiments of Sir A. Cooper, in which he fractured the neck of the thigh-bone in dogs and other animals, with the view of determining the disputed question whether true osseous union ever takes place after the accident when it occurs in the human subject, we shall close our notice of the "Remarks" by quoting the following passage in reference to the opinions of him, who may fairly be deemed the foremost of modern physiologists. Would that his example and his precepts were more generally followed!

"It is interesting," says Mr. Macilwain, "to observe that we not only have the evidence of Sir Charles Bell, as to the sources whence he adduced his own *discoveries*, which, as it will be seen, were altogether independent of any experiments on living animals; but that he has left us the gratification of knowing what his opinions were *generally* on that mode of investigation. After comparing Chemistry and Anatomy, with a view to impress that the former is emphatically a science dealing largely in experiment, the latter, one which depends chiefly on observation: this distinguished physiologist thus proceeds—'Anatomy is already looked on with prejudice; let not its professors *unnecessarily* incur the censures of the humane; *experiments have never been the means of discovery*, and a survey of what has been attempted of late years will prove that the opening of living

animals has *done more to perpetuate error*, than to confirm the just views taken from Anatomy and the natural motions.' This is perhaps enough, but again Sir Charles Bell observes, 'In a foreign review of my former papers, the results have been considered as a further proof in favour of experiments; they are, *on the contrary, deductions from Anatomy*—and I have had recourse to experiments, not to form my opinions, *but to impress them on others.* It must *be my apology* that my utmost powers of persuasion were lost whilst I urged my statements on the ground of Anatomy alone. I have made few experiments,' &c. Once more I will quote Sir Charles—'Much has been said (he writes) in favour of experiments made by men unbiassed as to the results. The only instances of this which I can allow are, (he is speaking of nerves, illustrative of his own enquiries,) when surgeons cut the nerves on the face in surgical operations. In such operations as those for Tic Douloreux he is indeed unbiassed, and we have seen the result, that after *fifty years of such experience we remained quite ignorant of the functions of these nerves.*'*

"Surely any one who reads these quotations, whatever may be his own views of Vivisection, will scarcely venture to quote Sir Charles Bell in support of it. The fact is, that to see an animal writhe with torture, and on a particular nerve being divided, to exhibit manifestations of loss of motion or sensation, was easy, mere 'eye-work,' which could *per se* prove nothing; but diligently to investigate the courses of nerves in the dead animal, to trace the filaments of which they were composed to their respective relations in the brain and spinal marrow, and then to test the suggestions arising out of these proceedings, by careful observation of phenomena in man, whether arising from disease, accident, or surgical operations, was difficult, tedious, and requiring the exercise of the higher faculties, whilst it constituted the only mode of eliciting the truth; and especially Sir Charles' ultimate object, viz. the application of the whole to the explanation and relief of disease." P. 24.

REPORT ON THE ARCHETYPE AND HOMOLOGIES OF THE VERTEBRATE SKELETON. By Professor *Owen*, F.R.S., &c. (From the Report of the British Association for the Advancement of Science for 1846). 8vo. pp. 171. London: R. and J. Taylor, 1847.

HAVING, in the review of the distinguished author's Lectures on the Comparative Anatomy and Physiology of Vertebrate Animals, in our last number, made the reader acquainted with the more prominent features of his theory of the cranial vertebræ, it is unnecessary to do more, upon the present occasion, than merely recommend this Report to the studious perusal of every one who feels an interest in the philosophy of osteological science. Even if space permitted, a lengthened notice of its contents would not be exactly suited to the pages of a Journal whose main object is the advancement of practical medicine. To show, however, that such

---

* "Sir Charles Bell's writings strongly suggest that it was the *fallacy* of Vivisection which created his *distrust* of it, as purely as his humanity had engendered his *dislike* of it. In one of his earlier proceedings, after stating the *apparent* result of the experiment, he says, 'but here there was *confusion* because of sensation, *therefore* the animal was instantly destroyed by a blow on the head; in other words, the suffering was put an end to, because sensation obscured the reasoning on the experiment.'—*Bell on the Nerves.* 8vo. edition, 1st part."

enquiries, notwithstanding their subtle and somewhat abstruse nature, are not wholly devoid of interest to him who is engaged in the exercise of the healing art, we gladly find a place for the following observations. Most of the terms used in it will be found explained in our last number.

"The abnormal conditions of the human skull give further illustration of the truth of the general homologies of the cranial bones, and reciprocally receive light from such determinations. In the case of idiots from defective growth or development of the brain, where the cavity of the cranium is reduced to half or less than half its normal capacity, as *e. g.* in the skull described and figured in my 'Memoir on the Osteology of the Chimpanzee,' it might have been expected from the anthropotomical ideas of the cranial bones,—according to which no one bone is deemed either more or less important than another in its essential nature, and where the squamosal is as little regarded in the light of a superadded or intercalary piece as the alisphenoid,—that all would be reduced in the same proportion in forming the parietes of the contracted brain-chamber. But this is by no means the case. In the instance above-cited the basioccipital and basisphenoid have been developed to their usual size, and the distance from the posterior boundary of the bony palate to the anterior border of the foramen magnum is as great as in any normal skull. The exoccipitals (condyloid portions of the occiput,) the alisphenoids and the orbitosphenoids retain in like manner their full dimensions. The distance between the frontal and temporal bones is as great as in the average of fully developed Caucasian skulls, and is greater than in most of those from the Melanian race, in which the direct junction of the frontal with the temporal, as in the chimpanzee, is by no means rare. The contraction of the capacity of the brain-chamber is due chiefly to arrested development of the frontals, parietals, supraoccipital and squamosals. By the reduction of the supraoccipital and the retention of the centrums of the cranial vertebræ of their normal proportions, the foramen magnum becomes situated nearer the back part of the basis cranii than in the normal skull.

"In a still smaller cranium of a female idiot, who reached the age of twenty-one years, which is preserved with the male idiot's skull above-mentioned in the anatomical museum of St. Bartholomew's Hospital, the contrast between the normal proportions of the basioccipital, basisphenoid, exoccipitals, alisphenoids and orbitosphenoids, on the one hand, and the reduced dimensions of the supraoccipital, parietals, frontals and squamosals on the other, is still more striking and significant of the true nature of those bones. The normal growth of the centrums, indeed, might be explained by the concomitant nearly normal size of the medulla oblongata, base of third ventrical and optic chiasma, in the brain of the same idiot; but it is not so obvious from the condition of the brain itself why the alisphenoid should not have shrunk in the same proportion as the parietals, frontals and squamosals. To the homologist, however, the recognised difference of subjectivity to modification presented by the neurapophyses, spines and diverging appendages of the typical segments, renders very intelligible the partial seats of arrested growth in the bones of these idiots' crania.

"In reference to disease, also, one sees not why the alisphenoid should have a minor attraction for the morbid products deposited, or be less subject to the destructive actions excited during syphilitic or mercurial disease, than the parietals, or the orbitosphenoids than the frontals, or the exoccipitals than the supraoccipital; yet it needs but to examine any series of such morbid skulls in our museums of pathology to be convinced that the variable and peripheral elements of the neural arches, viz. their expanded spines, are almost exclusively so affected: the frontal and parietal being the most common seats of the disease: the supraoccipital a less frequent one, concomitantly with its minor deviation from the typical standard of the element. I have yet seen no example in which either a cranio-vertebral centrum or neurapophysis was so affected; but the nasal bones are notoriously attacked."

THE MICROSCOPIC ANATOMY OF THE HUMAN BODY IN HEALTH AND DISEASE. Illustrated with numerous Drawings in Colours. By *Arthur Hill Hassall.* London: Highley, 1847.

THE Plates of this most useful work which have lately appeared, especially those of Part X., are extremely accurate and well executed. Those of our readers who have examined the figures illustrative of the minute texture and development of bone, will, we are assured, agree in the opinion here expressed. The drawing (Plate 33, fig. 6) taken from a specimen prepared by Mr. Tomes, showing the coloration of the walls of the Haversian Canals from the effect of madder, is particularly interesting as indicating the share taken by the endosteum in the formation of new bone. We can, with much satisfaction, strongly recommend Mr. Hassall's Microscopic Anatomy to all who desire to become acquainted with that most attractive branch of the science of organization, and it is fortunate that the extremely moderate price at which these plates are published, renders them generally attainable. We trust the whole work will be speedily completed, and that the author will receive that general support to which his labours so well entitle him.

THE PRESERVATION OF INFANTS IN DELIVERY. Being an Exposition of the chief Cause of Mortality in Still-born Children. By *Richard King*, M.D., M.R.C.S. 8vo, pp. 60. Churchill, 1847.

IT seems this tends to prove a revolutionary era in the established usages of the practice of Midwifery. We have Dr. Simpson counselling us to abstract the placenta as a remedy for unavoidable hæmorrhage instead of turning the child. Mr. Adams tells us it is all a mistake to suppose that absence of contraction of the uterus has anything whatever to do with hæmorrhage from that organ, the blood really coming from certain hypothetical and imaginary rents and lacerations of the passages. And now we have Dr. King standing forth to protest against the error all our celebrated teachers have fallen into, in supposing that death of the still-born child results from compression of the funis during labour. On the contrary, this very pressure may become the means of saving its life, for "the frequent death of the infant in preternatural deliveries arises from syncope, and not from asphyxia; in fact, from the want of the compression of the cord, and not from the dreaded compression of it." The author's idea (another, by-the-bye, utterly at variance with all authority, and we believe all fact) is, that the after-birth is detached from the uterus simultaneously with the expulsion of the first portion of the child; when it expands, "as a moist sponge would act when released from the hand and placed in contact with water," and favours the escape of the blood from the child, who perishes from the consequent syncope. His practice is therefore, as soon as the breech is expelled, to compress the cord before the delivery of the rest of the body; and he seems even to have invented some description of forceps for the purpose!

Feeling convinced that his supposition that the placenta is detached prior to the birth of the child is only correct in some rare exceptional cases, we likewise believe that the practice he founds upon it may often prove dangerous by diverting attention from a more correct procedure, or by itself inducing an asphyxia, which otherwise might never have happened. The distinction between the asphyxia and the syncope of new-born infants Dr. King lays so much stress upon, as of novel discovery, is borne in mind by every accoucheur, at least if he allows himself to be guided by the rules laid down in our standard works.

Periscope;

OR,

CIRCUMSPECTIVE REVIEW.

Selections from the Foreign Periodicals.

On the Frequency of the Pulse and Respiration of the Aged. By C. W. Pennock, M.D.

"Physiologists, generally, have considered it as an established fact that the frequency of the heart's action diminishes in advanced age; and no one has called the correctness of this view into question until Leuret and Mitivié in 1832, whilst engaged at the Salpêtrière in observations relative to the pulse of the insane, were astonished to find that the pulse of 34 sane women, in good health, whose medium age was 71 years, presented the average of 79 beats in the minute. This fact induced them to make further observations, and to institute an enquiry as to the relative frequency of the pulse of the young adult and that of the aged. On the same day, at the same hour, and under analogous circumstances, the pulses of the young men at the Veterinary School at Alfort, and those of the old men in good health at the Bicêtre were examined. The number of the veterinary students was 110, that of the aged men 27; the average age of the students was 21, that of the aged 71 years. The result of the examination proved that the medium pulse of the young men was 65, whilst that of the aged was 73. Temperature 32 F.

"Evidence confirmatory of the view of Leuret and Mitivié was offered in 1835, by Drs. Hourmann and Deschambre of Paris, who re-investigated the subject of the pulsations in connexion with that of the respiration of the aged. Their observations were made on 255 females, in good health, between the ages of 60 and 96 years of age, the average being 74·33 years. These researches were chiefly made between 6½ A.M. and 7½ A.M. at the temperature of from 46° to 48° F., soon after the individuals had left their beds and previously to eating. The result of the investigation was as follows: viz. medium age 74·33 years; medium number of pulsations 82·29; medium number of respirations 21·79. The ratio of the frequency of the respiration to that of the pulse as 1 : 3·41."

Dr. Pennock next furnishes us with an account of the results of a series of observations upon the pulse and respiration of the aged inmates of an Infirmary adjoining the Philadelphia Hospital, Blockly. They were usually made at least four hours after breakfast, when the individuals were undisturbed by exercise and free from mental excitement, the precaution being taken in order to familiarise them with the proceeding of making observations prior to those which were recorded. Various medical friends likewise forwarded to him the results of their own investigations which he has embodied in the present paper.

"Rejecting all observations of individuals in whom any rational or physical signs of cardiac, pulmonic, or other disease existed, the number of persons whose pulse is reported is 170 men and 203 women: being an aggregate of 373: the ages of the men being between 50 and 90, those of the women between 50 and 115. The frequency of the respiration does not seem to have claimed that attention from physiologists which it merits. It has been an interesting subject of enquiry in connexion

with that of the pulse, and the effort has been made to ascertain the ratio of the inspirations to the cardiac contractions."

The particulars of these observations are set forth in tables; but we must confine ourselves to the general statement of results furnished by the author.

"Table A is derived from the observation of the pulses of 170 *men;* the aggregate of whose age was 10,895, and that of the pulsations 12,211. The respirations were counted in 146 instances, the total number of inspirations being 3045. The medium age was, therefore, 64·09 years: the medium pulse 71·83: the medium respiration 20·51; and the ratio of respiration to pulsation as 1 : 3·51.

"Table B is derived from the observations of the pulse of 203 females, the aggregate of whose ages was 14,326, that of their pulses 15,838. The respiration was counted in 143 individuals, and its aggregate was 3154. The medium age was 70·57 years; the medium pulse 78·02; the medium respiration 22·06; and the ratio of respiration to pulse as 1 : 3·53.

"From the preceding facts it would seem to follow that the medium pulse of the aged men may be stated to be 71·83; that of aged females 78·02. Whilst the respiration of the former is 20·51, that of the latter 22·06 per minute. The ratio of the respiration to the pulse in aged men is as 1 : 3·51, in women as 1 : 3·53.

"The idea may be entertained that the frequency of the pulse of the aged, as stated in the preceding tables, may depend upon some accidental instances of extreme frequency, which should be rejected. In order to the proper appreciation of the truth, the tables C and D are presented, in which the individuals examined are grouped agreeably to the frequency of the pulse. From the table C it is very apparent that, in above one half of the aged men, 52 per cent., the pulse ranges from 76 to 84; that in more than one-third, 43 per cent., it is over 63; whilst in about 2 per cent. it averages, say 55, and in rather more than 3 per cent. it is over 96. From the table D it is evident that the pulse of aged females varies from 70 to 104 in nearly four-fifths of the individuals—79·93 per cent. of those examined; that in more than two-thirds, 69·45 per cent. the range of the pulse was between 75 and 86; that in 7·39 per cent., the pulse was between 95 and 96; that in 2·41 per cent. it was at 104; whilst it was below 70 in but a small number, namely, 37 out of 203, being rather less than a fifth of the whole, or 18·34 per cent. The pulse was below 60 only in 5 instances, or in 2·41 per cent. of the whole number."

After comparing these results with those obtained by the above-cited Parisian enquirers, Dr. Pennock thus concludes:—

"From the preceding facts and researches, it is evident that the frequency of the pulse of the aged is much greater than that usually assigned to it; whilst that of the respiration is equal to that generally admitted in reference to the adult in middle age. 'Generally,' say Hourmann and Deschambre, 'it is in old age that the pulse presents extremes of slowness or of frequency; but the first case is the exception, the second is the rule: the error of past time has been to take the one for the other.'"—*American Journal Med. Sciences*, July, 1847.

[Every attentive practitioner must have had occasion to verify the truth of the above proposition respecting the pulse, though probably on a smaller scale and less exactly. It is a point of great importance to be borne in mind, not only while directing the treatment of a case, but in prognosticating the soundness of a convalescence, which the persistence of a supposed anormal frequency of pulse might otherwise seem to forbid.—*Rev.*]

On Scorbutus as observed in the Salpetriere in 1847, and on the Composition of the Blood in this Disease. By Dr. Fauvel.

Since Scorbutus, yielding to the progress of hygiene, has almost disappeared in localities wherein it was heretofore endemic, no important work has appeared upon the history of this disease, of which the physicians of the last century have described the frequency and terrible effects. The work of Lind, so replete with facts and erudition, has not been surpassed, and the picture he sketched of the symptoms of the disease has served for the model of the most recent descriptions. The researches upon the composition of the blood in various diseases, undertaken with so much *éclat* in our own times, have however recalled the attention of physicians to this malady of a prior epoch. The reason of this new interest may be readily understood, when we consider that the fate of an entire doctrine depended upon the solution of the problem of the condition of the blood in Scorbutus.

Chemistry having confirmed the importance of the inflammatory crust of the blood which clinical experience had long since ascertained, it was next enquired, whether, in another pathological condition, an inverse appearance of the blood did not correspond to an inverse chemical change. This question was only imperfectly answered by the analysis of the blood in some of the pyrexiæ, since in these a diminution of the fibrine was found to be far from manifesting itself as a constant fact in these diseases. Was it otherwise in regard to scorbutus? Everything led to the supposition. The dissolved state of the blood, mentioned and differently interpreted by Hoffman, Boerhaave, Huxham, Lind, and many others, gave a great probability to the conjecture: while Magendie succeeded in inducing phenomena in animals, analogous to those of Scorbutus, by the injection into the veins of defibrinated blood or alkaline solutions.

Such was the state of the question when, in 1841, Andral, analysing the blood of a man affected with scorbutus, found the proportion of fibrine less than the normal mean; and from that time the former conjectures were considered as confirmed; a rational explanation of symptoms was given; and an ingenious theory of hæmorrhages, which appear in certain diseases of which scorbutus may be considered the type, was furnished. In the phlegmasiæ in which there is an increased quantity of fibrine in the blood, no hæmorrhagic tendency is observed; but in typhus scorbutus, and in other diseases in which the blood is in a more or less dissolved state, that is containing little fibrine, hæmorrhages are frequent, and are produced independently of the essential differences which separate the various diseases. The hæmorrhage is, then, a common phenomenon, dependent upon a common change in the composition of the blood in all these cases. If to this we add, that in certain other morbid states, in which the quantity of globules is greater than in the normal state, at the same time that the fibrine is unchanged, the hæmorrhagic tendency is produced but with different characters, we naturally arrive at the distinction founded upon a change in the composition of the blood peculiar to each of these.

In vain did Mr. Busk declare that in several scorbutic patients he had found the fibrine augmented; and in vain did M. Stæber refer to the conclusions of Parmentier and Deyeux that the scorbutic blood they examined was identical with inflammatory blood. These statements were received with incredulity, as arising from some error in diagnosis or the process of examination. Yet it was desirable that new and numerous analyses should sanction the principle laid down by M. Andral from a soli-

tary case : and this year an unusual opportunity offered itself to me at the Salpêtrière. Among the old women there, a few spots of ecchymosis on the limbs are not infrequently seen: but for more than a year no case of scorbutus had shown itself; when last March, a woman æt. 76, strong and well for her age, became suddenly attacked by symptoms of it. Certain of these symptoms led to her being bled, and as no inflammatory complication was present, the usual state of the blood described by authors was expected to be found; but to my astonishment next day I found the clot covered by a yellow, firm, elastic, fibrinous layer, just such a one as is met with in simple pneumonia—the remainder of the clot offering no signs of dissolving. MM. Becquerel and Rodier were made acquainted with this unexpected result, and, as a little epidemic of about thirty cases manifested itself during the next two months, they had ample opportunity of instituting more exact enquiries into the condition of the blood. The blood of five patients was submitted by them to analysis, and the following are the conclusions arrived at by these able chemists :—

" 1. That far from presenting a dissolved state, as the usually received theory supposes, the blood in these cases offered a well separated, and sometimes very firm clot, swimming in a limpid serum uncoloured by the presence of globules. 2. The density of the defibrinated blood was in all the cases below the normal mean (1057), and the figure 1038·3 observed in one case was lower than had ever yet been met with by MM. B. and R. Moreover this diminution of density, in the five cases, was not in relation with the respective proportions of water and solid matters of the blood —an extraordinary fact quite inexplicable. 3. That the density of the serum was notably lower than the normal figure. 4. That the quantity of globules was always less than normal, and was found very much diminished in the case corresponding to the lowest density of the blood. 5. That the *fibrine*, contrary to received ideas, *was not diminished in any of the cases.* In two cases it offered the normal proportions (2·2), while in the three others it was sensibly augmented, viz., 3, 3·6, and 4·1. It appeared to be endowed with all its ordinary physiological properties. 6. That the organic matters of the serum, of which *albumen* constitutes the larger portion, had undergone a notable diminution, while the water existed in a considerable proportion. 7. That in no case were the inorganic matters superabundant, and nothing proved an excess of alkalis, which sometimes authors have declared to be the case in the blood of the scorbutic."

Is it not remarkable thus to see the most rational provisions entirely overturned ? Theory declared that in scorbutus the quantity of fibrine was diminished, whence arose a defective plasticity, the cause of the characteristic hæmorrhages. It added that such diminution of fibrine might be the sole change, without necessitating that of the other organic elements of the blood. But the exact reverse of this has come to pass. The *globules* and *albumen*, which should have remained uninfluenced, are found to descend much below the physiological mean; while the *fibrine* exhibits an opposite tendency, so as to approach it to the condition observed in inflammations. Since the above analyses have been made, M. Andral has himself made another, and found the fibrine as high as 4·5; and M. Prus states that he has frequently observed, at the Salpêtrière, facts analogous to those now brought forward. On the other hand, is it not interesting to see so large a diminution of *albumen* without any dropsy resulting, save a little œdema in the neighbourhood of much ecchymosis ? Is there not here something which materially interests the theory of the production of dropsy by a diminution of the albumen of the blood ?

But it will be said, do you pretend by these few cases to invalidate all the descriptions of the state of the blood in scorbutus handed down to us by the old writers ? By no means. Only we would draw different conclusions from them. In fact, if we consult the principal authors who have written upon scorbutus, it becomes evident that they have given this name to diseases of a different nature, to true typhus ; or that they have not taken into account the intercurrent affections arising during an epidemic of scorbutus, as of those which result from a putrid infection produced by the crowding together of patients in too small a

space, as on board a ship. The dissolved state of the blood occurring under these circumstances would be easily explained.

And even supposing that in the great and destructive epidemics of scorbutus, the blood, deprived of fibrine, lost its power of coagulation, does it follow that this should always be the case? Is the quantity of fibrine constantly diminished in typhoid fever? To arrive at a knowledge of a disease, is it not necessary to study it in its most simple form, and to disengage it as much as possible from the complications which change its essential characters? It is precisely because the scorbutus we have observed presented itself in a simple but characteristic form, that the results we have adduced have an undeniable importance. They are neither numerous nor conclusive enough to serve as a basis for any particular doctrine. They have only a negative value as tending to cast doubt on the doctrine of scorbutus prevailing in our time. They demonstrate the necessity of new researches, directed in other manners and by other proceedings, and undertaken upon a vast scale. Lastly, they confirm the utility of bearing in mind the maxim which Lind called the attention of the physicians of his time to—*Chymia egregia ancilla medicinæ; non alia pejor domina.*

Dr. Fauvel supplies a minute medical history of the cases observed by him, in proof of their being examples of genuine scorbutus; but to this we need very briefly refer. It showed itself chiefly in persons advanced in age, the youngest being 69 and three more than 80. They were all previously in good health, and fed upon substantial food. Among the principal symptoms observed were—(1). *A yellowish colouration of the entire skin, quite sui generis*, most comparable to the light yellow tinge which succeeds to ecchymosis. It affected especially the face, and extended to the conjunctivæ; but the urine giving no traces of colouring matter, proved it was not icterus, which it so much resembled. This colour diminished and disappeared in proportion as the case proceeded on to recovery. (2). The *hæmorrhagic spots* offered very different characters even upon the same subject, varying from mere red points to true purperal petechiæ. The most important variety was a true scorbutic hæmorrhage, consisting of large ecchymoses or infiltrations into or below the subcutaneous cellular tissue of the lower extremities—occasionally constituting quite projecting tumefactions. Great pains frequently accompanied these effusions; and indurated œdematous infiltrations of the limb, together with pain and tenderness of the joints, were sometimes found. (3.) The *changes of the gums* were quite peculiar, not consisting in a general tumefaction and softening of their tissue as seen in some stomatites; but of fungous vegetations developed around the neck of each tooth, and being therefore only numerous in proportion to the number of teeth which remained. They were soft and bled easily. Mastication was difficult, and the mouth exhaled a very fetid odour. (4). The *prostration of strength* of body and mind was remarkable; although the tendency to syncope, mentioned by Lind, was not remarked—the patients, however, retaining the recumbent posture.

The *treatment* employed consisted in the use of a tisane acidulated with lemon, a mixture containing tannin and antiscorbutic syrup, linseed cataplasms watered with a decoction of bark, as substantial a regimen as the state of the mastication admitted of, composed of rich soups, roast meat, and as large a proportion of green vegetables as possible. In the course of treatment the juice of antiscorbutic herbs was substituted with great advantage for the tannin. Of nine patients, four were completely cured in an average period of two months, four still continued under treatment, and one died.—*Archives Generales*, t. xiv., pp. 262–287.

Subsequently to the results of MM. Becquerel and Rodier's investigations being communicated to the Academy, *M. Andral* read an account of a new analysis which he had at the same time made of the blood drawn from a well-marked case of scorbutus under his own care at La Charité. The following is the substance of his important remarks.

"I expected to find it diffluent and dissolved, but to my great astonishment

it was not so. It consisted, in fact, of a small dense coagulum as resistent as that of phlegmasiæ, and covered by a well-marked buffy coat, and was suspended in a large quantity of serum. In the 1000 parts it gave the following proportions:

Fibrine	4·420
Globules	44·400
Solid matters of the Serum	76·554
Water	874·626
	1000·000

"This blood in its composition resembled that of chlorotic patients by the diminution of its globules, and the large quantity of water, although the patient presented other symptoms than those of mere chlorosis. As to the fibrine, instead of being less than, it exceeded, the physiological mean. This is quite a different result from that I obtained in the other published case, in which the globules were in nearly their normal proportion, and the fibrine was in very small quantity. So, too, I found very little (about 1 per 1000) in a patient attacked with purpura hæmorrhagica.

"The fact which I have narrated, and which is confirmatory of those recently laid before the Academy, proves that the symptoms ordinarily characterising scorbutus may be produced without being necessarily accompanied by a diminution of fibrine. It is not then in such diminution we can place the proximate cause of scorbutus; and it is not by it even we can hope to explain several of its symptoms, especially its characteristic hæmorrhages. In this respect, as well as in several others, we may perhaps compare scorbutus with *typhoid fever*. In the latter, in fact, the lowering the proportion of fibrine is often met with, but it is not necessary for the existence of the disease. Observation only authorises us to state that the diminution is considerable in proportion as the dynamic form of the disease is well pronounced. The same thing takes place in the eruptive fevers; so that, in these different cases, the diminution of fibrine cannot be considered as one of the necessary elements of the disease, but only as one of the more or less frequently occurring effects of the cause which has produced it, and which exerts its influence alike upon the vital forces which it tends to depress, upon the nervous system which it throws into great perturbation, and upon the blood in which it tends to produce an alteration, the reverse of that observed in phlegmasiæ. The same thing appears to me to take place in scorbutus. Like typhoid fever, it may become developed without the blood having previously lost its fibrine, and therefore a diminution of this is neither a necessary and constant occurrence, but only an effect, a result of prior morbid modifications, a result which is produced more or less frequently according to the severity, duration, &c. of the disease.

"Are we to refer the hæmorrhages of scorbutus to this diminution of fibrine as their cause? There can be no doubt that hæmorrhages which, like those of scorbutus, are multiplied and repeated at a great many points of the economy, are especially seen in those cases in which the blood itself has become more poor in fibrine than in the physiological state. But these are two facts which it seems to me are only coincident in their relation: both doubtless being most frequently produced under the influence of a common cause: both are but manifestations of that cause, but the one does not seem to me to engender the other. The case I have related presents an example of remarkable hæmorrhages without any such diminution. Moreover, is it so easy to understand theoretically why a diminution of the quantity of fibrine should lead to a flow of this fluid from its vessels? Is the diameter of the globules diminished, so that they can more easily traverse the vascular parietes? Are we to admit that this passage may be favoured by an antecedent hypnæmia, when, after the violent distension of the vessels produced by inflammatory congestion such effect is not produced, and

the globules do not leave their vessels but when these have been torn? This opinion, reproduced in our times, and which I myself have maintained, making the hæmorrhages of scorbutus and pernicious fevers dependent upon a dissolved state of the blood, arose at a period when it was believed that this took place from the destruction of its globules; but such destruction, supposed by Huxham and so many others, is a pure hypothesis. Many a time, in cases in which the blood presented different degrees of this dissolved state, have I examined it with the microscope; and I have constantly found its globules with their ordinary appearance, and perfectly untouched. So, too, the globules in the blood from the hæmorrhages possess all their physiological characters. I repeat, then, it is not easy to understand how the simple diminution of the fibrine should render their issue from the vessels more easy. In considering the augmented portion of fibrine observed in this patient, we must bear in mind, however, that when the bleeding was practised there were symptoms of acute disease of the respiratory organs, with much febrile re-action, and I believe it is to this circumstance we ought to attribute so large a proportion of fibrine. Some days after this bleeding had been practised, and probably in consequence of it, we could discover no traces of inflammatory action. Although the blood, at least three weeks prior to death, had not lost its fibrine, the spleen, nevertheless, was none the less soft and reduced to a pulp; so that we here see a change which, although generally coinciding with a dissolved state of the blood, may yet exist without it. The highly congested lungs, containing here and there apoplectic kernels, contrasted by their dark colour with the extreme paleness of the liver and kidneys."—*L'Union Medicale*, No. 78.

M. *Marchal (De Calvi)* in a communication to the Academy of Sciences, expressed his doubts of the correctness of the conclusions recently arrived at, from the fact of fibrine being sometimes found in excess in the blood in scorbutus. He believes the inflammatory element, to which this is due, has been overlooked. He lays down the following propositions:—1. There are two species of hæmorrhagic phenomena in scorbutus; sanguineous infiltration or interstitial hæmorrhage, and hæmorrhage properly so called. 2. Each of these may be external or internal. 3. In internal hæmorrhage the infiltrated parts may re-act and inflame. 4. It is such re-action that explains the maintenance of the normal or production of the excess of fibrine. 5. In hæmorrhage properly so called it is to be presumed that, as there is no local re-action, the amount of fibrine will continue less. 6. There is no reason, from our present information, to change our opinion of the defibrinated condition of the blood in scorbutus. 7. Even were this the case, it would offer no reason to deny the defibrination of the blood in the pyrexiæ, and the dependence of the hæmorrhagic phenomena upon this circumstance. 8. In the present epidemic, besides the changes introduced into the regimen, another indetermined cause has acted. 9. The albumen and globules are diminished in scorbutus, and yet, in general, there is neither dropsy nor arterial murmurs. 10. As to the absence of dropsy, we may explain it easily, the *deficiency in the formation* of albumen being a very different thing from the *loss* of this principle. 11. Scorbutus and Typhus are not analogous. In the one there is impoverishment, in the other, a poisoning of the blood.—*Gazette Medicale*, No. 34.

In relation to one point, alluded to in M. Andral's communication, the *analogy prevailing between Scorbutus and Typhoid Fever*, we may notice a short discussion which recently took place at the Academy of Medicine.

An epidemic of Scorbutus having broken out in the garrison at Givet, M. *Scoutteten* was sent by the Government to investigate its causes. He considered that the causes of the disease were similar to those capable of inducing typhoid, the hospital being placed in a very unhealthy locality, and the regiments whence the patients were derived having suffered under a defective alimentation. The patients were removed from the damp locality in which they were placed, and soon did well upon a good diet. The particulars of the diet the soldiers had been subjected to he was

not at liberty to divulge. (This disgraceful fact has been well and severely commented upon by the entire Parisian medical press). M. *Rochoux* having stated his opinion, that a vitiated state of the air had always been the leading cause of the production of this disease, M. Scoutteten felt certain that, however much other causes might aid, defective alimentation was the principal one. Thus, when the new prison regimen, which included a diminished diet, was adopted, scorbutus quickly showed itself; and as quickly disappeared when the diet was improved, all other circumstances remaining the same. In reference to the present epidemics, two regiments occupied the same barracks, one being much better fed than the other, and the latter alone suffered from the disease. M. *Ferrus* observed that scorbutus much prevailed in prisons and lunatic asylums at the present time; and that, although bad diet was the principal, it was not the only cause. He had found it very often co-existent with other diseases, especially typhoid, owning common causes. "All these affections seem connected by one common tie—general debility, and are we to attribute this to humidity, alimentation, or over-crowding? To all of them united. We find in such places an assemblage of a great number of individuals in a too circumscribed space, an insufficient and too uniform a diet, a sedentary life for persons mostly accustomed to laborious occupations, and the demoralization and despondency contracted by their situation." M. *Bouillaud.* "We have heard nothing but assertions unsupported by facts. It is quite evident to those who have meditated upon the causes of scorbutus and typhoid, that not only is there no identity, but not even any analogy. Daily may we see, in the hospitals of Paris, cases of typhoid fever, and for my part I have seen more than 700 in 25 years, and yet I have never seen a single case of scorbutus. If the analogy is so striking, why do not the same causes produce both effects? Some persons admit that typhoid fever is contagious, but no one will allow that scorbutus is so. In typhoid there is febrile re-action, none in scorbutus." M. *Scoutteten.* "I am much surprised to hear that M. Bouillaud has never seen scorbutus. He has only to visit the wards of Val de Grâce, where an epidemic now prevails; and he may there see typhoid fever and scorbutus developed in soldiers subjected to the same influences."—*Gazette Medicale*, No. 29.

[The subject of Scorbutus has excited so much interest during the last few months in various parts of Great Britain—practitioners who had begun to consider it as a mere traditional disease, having now had ample opportunities of studying its various features—that we have deemed it right to lay the above somewhat lengthy extracts before our readers. We cannot consider that the analyses here referred to definitively settle the question of the amount of fibrine ordinarily contained in the blood in this disease, and are disposed to adopt M. Marchal's suggestion. Our means of investigating the condition of this fluid are far superior to those possessed by the older observers; but they were as well able to estimate the mere appearance of the coagulum, as influenced by the presence of more or less fibrine, as ourselves. As to the causes of the disease, these, in common with those of other epidemic visitations, are enveloped in the obscurity implied in the phrase "epidemic constitution of the air;" for, although a defective or ill-adjusted alimentation seems essential to the production of the malady, it does not *alone* suffice, as, did it so, the disease would have been found still more generally prevalent during the late season of distress and privation, and would have also frequently manifested itself on former similar occasions, instead of being a new affection for almost the whole race of living practitioners.—*Rev.*]

Influence of Electricity in the Production of Diseases. By M. Pallas, Principal Physician in Algeria.

Dr. Pallas has addressed a note to the Academy of Medicine prior to publishing a work he has undertaken upon the importance of electrical isolation in the treatment of certain diseases. The subject, he states, is especially important in relation to the etiology, nature, and treatment of the diseases of hot climates; and may be summarily stated in the following propositions.

1. The greater number of diseases, and especially those which belong to the class of neuroses, are occasioned by the exaggerated influence of general electricity, of which clouds, storms, and marshy regions are the most fruitful sources. 2. Marshes, in their geographical constitution, and the effects which they produce upon the economy, present the greatest analogy with the galvanic pile. Thus their action is so much the more baneful as they contain certain proportions of water, and their activity is considerably increased when the water contains organic or saline matters in a state of solution. This explains why salt marshes and such as are near maritime rivers are the most insalubrious. The drying up or submersion of marshes produces analogous conditions to those of a galvanic pile deprived of humidity, or which is under water, and the effects of which are then insignificant. 3. The researches of philosophers and physiologists have shown that the electricity produced by our machines exerts a special action upon the nervous system. Experience and rigorous observation of facts prove that the diseases which are produced in a marshy atmosphere are primarily nervous, and become inflammatory only by the re-action of the nervous upon the vascular system, inducing consecutive local or general irritation. 4. The neuroses are occasioned, generally, by the effects of electricity, and intermittent fevers have a similar origin, that is to say, they are due to the electrical emanations of the marshy pile, which are very active in hot countries, and not to miasmata, which have never been met with. 5. Electrical isolation is a rational means of modifying this morbid influence, and is accomplished by attaching to ordinary beds, sofas, or chairs, legs of glass or resin. A great number of cases prove that the patients whom I have thus isolated, have been cured or relieved, several of whom had resisted all the ordinary means of cure.

"6. Just as light and air are the essential agents of vision and respiration, electricity is the functional agent of innervation, whose injurious action may be modified by isolation, which is to electricity what a shadow is to the solar light."—*Bulletin de l'Academie*, Tom. xii., p. 743.

On the Diagnostic Signs furnished by Inspection of the Vulva and the Entrance of the Vagina. By M. Huguier.

By the aid of this simple means we have often been enabled to recognise certain facts, and diagnosticate some of the diseases of the uterus, the nature of which were sometimes misunderstood, or could only be ascertained by long and repeated examinations, insufficient to the end, or even inapplicable in some cases. We meet with women who, whether from fear, excess of sensibility, or real or pretended modesty, will not allow of the finger, or any instrument whatever, being passed within the sexual organs; while in virgins, and in women suffering from severe inflammation of the genital organs, or from original or acquired vicious conformation, it becomes impossible to carry our means of exploration beyond the vulvar orifice.

These circumstances may often render the vulvo-perineal inspection a valuable aid to diagnosis in the cure of certain uterine diseases. And it is so in certain physiological conditions bordering upon morbid states of the womb.

Inspection during Pregnancy.—The reddish-violet colour of the vulva in pregnancy has been very insufficiently indicated by authors. It is quite characteristic, but is not uniform. Little apparent upon the internal surface of the labia, it becomes very sensible upon the inner surface of the nymphæ, near the meatus and clitoris, and upon the tubercle anterior to the vagina—wherever, in fact, the mucous membrane becomes more delicate, thin, and transparent. This colour cannot be confounded with that which is caused by the varicose state of the veins of the genital organs in some women, and which is inseparable from the course of the venous trunks whose form and direction it reproduces. The coloration of pregnancy is diffused, and differs in its shade. It is first seen at the end of the second month, and becomes very evident during the third, so that it precedes all other sensible signs of pregnancy. Moreover, we have never seen this state in persons who did not prove to be pregnant—a circumstance of importance in a medico-legal point of view.

Another peculiarity in the vulva of pregnant women is its *humidity*, which may amount to even a discharge. Its cellular tissue also becomes the seat of a slight *serous infiltration*. The *piliferous follicles*, moreover, become hypertrophied, and assume on the external surface of the labia a nipple-like, granular appearance. The sudorific apparatus, and the sebaceous glands also secrete more actively, from whence results an anormal secretion, and a gluey, sticky condition of parts. On the other hand, in cases which only simulate pregnancy, the same parts manifest a withered, wasted appearance.

2. *After Delivery.*—In a female newly delivered, we find an anormal dilatation of the vulvar orifice, a depression and a kind of shortening of the perineum, an enlargement of the *fourchette*, with or without lacerations, or recent cicatrices; the vulvar folds, and the nymphæ are a little fuller than usual, and dirty, earthy reddish fluid flows out, having its specific odour. The vulvar lacerations will be found especially at the back part of the vulva, at the fourchette, and next especially at the left side, and least frequently near the union of the nymphæ with the labia, especially on the left side. They are usually triangular in form, the apex looking towards the centre of the vulvar orifice, and the base towards the thighs; which is not the manner in which solutions of continuity are disposed that result from external violence from the introduction of foreign bodies into the genital organs.

3. The aspect of the genital organs allows us likewise to indicate other particularities in certain pathological and constitutional conditions of women. In fair, flaccid, lymphatic women; in those who are cholrotic or scrofulous, or who are liable to a habitual amenorrhœa, the labia, nymphæ, and clitoris are but slightly developed; the vulva is covered with little hair, the mucous membrane is pale, shining, and cool; the papillæ and follicular prominences are but little marked; the colour of the entrance of the vulva differs but little from that of the labia and nymphæ (as I will show you upon two women suffering under uterine affection, whom I will submit to your examination after lecture!); and the skin of the vulvar region is destitute of that more or less abundant pigment which we observe in dark and well-constituted women, and which contrasts very sensibly the colour of this part with that of the abdomen and thighs. It is also in these lymphatic and chlorotic women that we find the vulvar orifice and the entrance to the vagina frequently bathed with a hypersecretion of the vagina resulting from atony, or an uterine catarrh of the same nature—cases in which tonics and ferruginous preparations are indicated.

4. A few words may be added concerning the fluids which bathe the vulva. If on separating the labia we see a reddish, sanguinolent, abundant, strong-smelling fluid flow out, we may suspect the existence of a cancer, a polypus, or a fibrous body projecting into the cavity of the uterus. If the fluid is creamy, of a yellowish white, more or less consistent, and neither clinging nor elastic, it proceeds from the vagina. If there are albuminous, whitish, tenacious, transparent, hyaloidean flocculi, of a greenish-white or yellow, the liquid proceeds from the uterus, and results from a simple hypersecretion or a purulent uterine catarrh. It is to be remarked, however, that in the case of cancer, polypus, or fibrous tumour of the uterus, the folds and wrinkles of the mucous membrane at the entrance of the vulva are almost entirely effaced; the papillæ and follicles have, so to say, disappeared; the parts take on a smooth, polished and shining aspect, and a pale colour.—*Gazette des Hôpitaux*, No. 74.

[Although we imagine the modesty spoken of by M. Huguier as occasionally preventing women allowing of an examination per vaginam by the finger, would in this country certainly likewise extend to the ocular inspection of its entrance, we may as well make use of the information derivable from the proceedings of patients and practitioners among whom objections upon this ground must obviously be of rare occurrence. Some of the conclusions are of too disgusting a character, such as those relating to the habits of intercourse, &c., of the patient, to be here referred to, but others may be useful occasionally.—*Rev.*]

On Stricture of the Intestine in Hernia. By Louis Chapel, of St. Malo.

An enfeebled old woman, (æt. 74), unable to give any account of its prior history, was brought into the surgical ward of the Hôtel Dieu, St. Malo, with a strangulated femoral hernia upon the right side. After the taxis had been repeatedly tried, and frictions with the belladonna ointment employed, the operation was performed. She seemed to be doing very well for some hours after the operation, and had an evacuation; but she sank within 48 hours of its performance. At the *post-mortem*, slight injection of the convolutions and some recent adhesions between these and the parietes were observed; and, at one point, a portion of the intestine was observed to be extremely strictured, just as if it had been compressed by a ligature. It felt just like a fibrous ring, and its circumference was not above 27 or 28 millimetres, although that of the portion above and below was respectively 96 and 70 m. The mucous membrane near the softened part was slightly inflamed, but no other change in the rest of the canal was observed. The neck of the hernial sac was found thickened.

M. Chapel, in commenting upon this case, remarks that authors have made very few observations upon the circumstance, which he does not doubt is of more frequent occurrence than is generally believed. *Ritsch*, in 1765, presented an account of a case to the Academy of Surgery, which he believed to be unique, but in its discussion before that body, others, related by *Mertrud* and *Cutavoz*, in which marked stricture likewise existed at the point corresponding to the seat of strangulation, were referred to. *John Hunter*, in his Treatise on Inflammation, states that he has met with an occlusion of the intestinal cavity by plastic lymph, as a consequence of strangulated hernia; and *Pelletan*, in his "Clinique Chirurgicale," relates the fatal termination of an operation for strangulated hernia induced by stricture of the intestine. M. *Cruveilhier*, in his "Essay upon Pathological Anatomy," even divides such strictures into chronic and acute, and believes they may be induced by the continuous pressure of a badly applied truss. *Boyer*, treating upon the symptoms which may follow the operation, states that there are no distinctive signs proving

these as resulting from a stricture or strangulation of the intestine; but that, when they do not yield to antiphlogistic treatment, it is to be presumed that they depend upon one or other of these causes. "I have observed some of these cases of stricture," says *Velpeau*, "but it seems to me that their nature has been mistaken. There is nothing permanent or organic about them. They may be dissipated in a few seconds with the fingers, and a purgative usually easily triumphs over them after the operation; and it would be a piece of barbarism to stretch the organ below, as has been proposed." M. *Maisonneuve*, in his Memoir upon the "Enterotomie of the Small Intestines" (*Archives Generales*, 1845), enumerates the causes which may stricture the intestine, states that the most frequent of all is the firm constriction of the intestinal tunics at the hernial orifice. M. *Vidal*, in his "Pathologie Externe," likewise admits this cause of stricture of the intestine, observing that this is more complete and persistent in proportion as the strangulation has been of long continuance.

The case already related, proves that M. Velpeau's opinions upon the subject will not always hold good, although a temporary obstruction is in all probability the general circumstance. The constriction could not have been induced here by a truss, as suggested by Cruveilhier, inasmuch as the woman had worn none; and it is most probable that it resulted from organisation of plastic lymph, as stated by Hunter.

During the operation, the surgeon should very carefully examine the condition of the intestine, and it is from neglecting to do this that so many see their patients die afterwards. "I perceive," says Ritsch, "in these cases, how importaut it is not to proceed to reduce the intestine after the division of the ring, until a portion of it has been previously drawn out, and the effect of the constriction at the seat of strangulation examined; and, if I found any obliteration of the canal which would render the passage of the fæcal matters impossible, I would take great care not to make the reduction, which would only be to devote the patient to a certain death." MM. Velpeau, Tessier, and others recommend the speedy recourse to purgatives, after operations for hernia. This was warmly opposed by Sanson in 1833, who was a partisan of the physiological medicine; and certainly injections or very mild laxatives would seem to be preferable to purgatives of a drastic nature.

Supposing the surgeon has recognised the stricture, his line of conduct is still a difficult one: but the facts brought forward by Louis, in his Memoir upon the cure of hernias affected with gangrene, should lead the surgeon to endeavour to form an artificial anus. If the intestine was returned in spite of the endeavours of the operator, or if he had not remarked its condition, and symptoms of strangulation still continued, he should imitate M. Maisonneuve, who determined upon searching for the intestine, two days after he had operated upon a lady 64 years of age, and whose life, by this bold procedure, he had the happiness to save.—*Revue Medico-Chirurgicale*, Tom. 1, p. 329.

Researches on the Normal and Anormal Capacity of the Cavities of the Heart. By Dr. Beau.

This paper is intended by its author as an Appendix to the one on Arterial Murmurs noticed at length in our Number for October, 1846. In that, he sought to establish that arterial murmurs, and especially the carotidean, are produced whenever the wave of blood, rendered too large by reason of the dilatation of the cavities of the heart, exercises too great a degree of friction upon the arterial parietes. In support of this proposition, he advanced that post-mortem examinations exhibited to us dilatation of the heart as a lesion connected with those

diseases which are characterised by these murmurs. On the present occasion his object is to state the confirmation of this proposition he has derived from actual *mensuration*. His mode of mensuration is a great improvement upon that usually adopted; for, instead of attempting to give the absolute capacity of the cavities, which may normally vary much, he furnishes the *comparative* capacity as derived from its comparison with that of the arterial orifices—which, from their fibrous character, are preserved nearly invariable during the changes the muscular structures of the heart are liable to. He has, however, confined himself to the comparative mensuration of the *left ventricle and aortic orifice;* since with most observers he has found the cavity of the auricle very irregular and difficult of appreciation, while the right ventricle and its auricle are found of varying capacity according to the quantity of blood that may have accumulated there during the last movements of life. Nevertheless, although exact mensuration has not been attempted of these, the right cavities were always found to possess a greater capacity than the left, and the left auricle exhibited the same variations of capacity as did the ventricle of the same side. So that, in fact, the comparative mensuration of the left ventricle and the aortic orifice supplies a statement of the capacity of the entire heart.

"I divided the heart across and perpendicularly to its axis in the middle of the space comprised between the apex of the organ and the origin of the arterial orifices. I took the diameter of the circumference formed by the ventricular walls of the left side upon the section belonging to the base of the heart, having first gently compressed it to dispel the cadaveric rigidity when this existed, and taking care to maintain the ventricular walls as rounded as possible. I then measured exactly the diameter of the cavity, without comprehending its walls, and in avoiding the *columnæ* attached to the bicuspid valves. Having taken the diameter of the ventricle, I divided the aorta at the level of the free border of the semilunar valves, and measured the diameter of the circumference of the vessel upon the section attached to the base of the heart, taking care to maintain its calibre rounded, and not including the arterial walls in the admeasurement."

The hearts measured belonged to three categories of patients. 1. Those who had died of diseases in which no arterial murmurs were heard. 2. Those who had manifested such murmurs until the period of their deaths. 3. Those in whom the arterial murmurs having existed, disappeared during the latter period of their respective diseases. The reproduction of the figures would demand more space than we have at command; and, as the real point of importance is the *comparative* size of the parts measured, the general results arrived at will suffice. In the *first* category then, in which, as far as regards the murmurs, the heart and vessels may be considered as normal, the diameter of the left ventricle equalled the aortic, or was a little larger than it. And although in different individuals the size of the ventricular diameter varied, yet, on comparing this with that of the aortic, the same relation was found to be maintained. In the *second* category, in which the murmurs were persistent, the ventricular diameter was found to be about double that of the aortic; sometimes a little more, sometimes a little less. It is on account of this relatively narrow calibre of the large vessels, that the blood entering these from the dilated ventricle exerts the degree of friction which induces the murmurs. In respect to the generation of this and some other symptoms, this *relative* dilatation of the ventricle is a matter of far higher importance than its *absolute* dilatation. In the *third* category, in which murmurs once heard disappeared prior to death, the capacity of the ventricle was found in a very slight degree larger than in the first category, and less considerably than in those of the second. This dilatation of the heart depending on its atony is not a fixed, unchangeable condition, like that which is connected with lesion of the orifices, and it diminishes or disappears with the sounds which it gives rise to—a diminution of the volume of the pulse, and of the extent of dulness on percussion in the præcordial region, being coincident events.

Phthisis is the disease in which a disappearance of the sounds and dilatation most commonly takes place as the patient becomes exhausted by the disease. In like manner, in certain cachexiæ, as the cancerous, the same thing occurs—a permanent diminution of the mass of the blood taking place, and a corresponding gradual retractation of the cavities to near their normal size.

The mode of relative admeasurements here advocated, may also be employed advantageously for the determination of the different degrees of thickness of the cardiac walls and the ascertaining whether this is normal or not: for, if Nature accommodates the cardiac cavities to the extent of the aperture of the arterial orifices, it is but reasonable to suppose she would establish a relation between the size of this orifice and the thickness of the muscular substance which propels the wave of blood through it. As in the normal state, these orifices being narrow, and the cardiac cavities likewise small, the wave of blood, then of little volume, does not require such strong and thick muscular walls for its propulsion as in individuals in whom this capacity is larger. To ascertain the thickness of the walls of the ventricle precisely the extent of the capacity of its cavity must be taken into consideration: for by this alone can we clear up the questions of passive dilatation and concentric hypertrophy. This subject being, however, only secondary to the one whose illustration he had in view, M. Beau has as yet made none of the necessary measurements, and we need not therefore follow him in the interesting speculations he enters into respecting it.

In the categories already described, the *diameters of the orifices of both the aorta and the ventricle were found to be more considerable in proportion to the age of the subject;* confirming as far as they go the law laid down by M. Bizot, that the calibre of the heart and arteries, and the thickness of the cardiac and arterial parietes indefinitely increase with age, so as to be found at their height in old persons. M. Bizot employs this conclusion as a means of attack against *à priori* reasoning, by exposing the powerlessness of theory in reconciling this excess of material energy of the heart and arteries in the aged with the physical and moral debility which is their attribute: but the difficulty is not perhaps so inexplicable as M. Bizot deems it; for a dilated state of the heart and arteries may easily be conceived to be connected with general debility of the entire organism. Thus in old age we find the veins, especially of the extremities, the sphincters, and the aponeurotic rings lose their elasticity, or rather tone, and yield to forces tending to increase their diameter; and this same atony leads to an augmented capacity of the heart and arteries. The simultaneous increase of the thickness of the heart and arteries, takes place as admitted by M. Bizot in the intestines, stomach, or any organ submitted to dilatation; and did not the heart and vessels acquire from this cause some additional force, they would be powerless for the movement of the increased mass of blood their dilated condition admits the entrance of. This increase of substance is, however, in its nature, a morbid phenomenon, and not to be likened to that which occurs at an early period of life. In the latter, the normal increase of substance takes place alike in all the constituent parts of the heart, and especially at the semi-lunar valves. But, in the aged, these valves do not increase in extent simultaneously with the arterial orifices, and are not consequently sufficiently large to prevent some reflux of blood into the heart. There is, in relation to the cause, the greatest possible analogy between the amplification of the heart as a consequence of old age, and that which is developed in diseases characterised by the presence of arterial murmurs; since in both cases it depends upon atony arising from a relaxed condition of the muscles of animal life. There are these differences however—the amplification taking place in the aged very slowly, the hypertrophy resulting is far more considerable than in the pathological amplification; and, as in old age, the dilatation of the heart comes on so very gradually, the arteries have time to become proportionally enlarged: so that the normal relative capacity of the respective cavities is maintained. Consequently, though the wave of blood sent through the orifices

is larger, the violence of the pulse of old persons fuller, and the dulness over the præcordial region greater, there is no production of the anormal murmurs which manifest themselves in that pathological dilatation of the heart, which creates a disproportion between its cavity and that of the aorta. Nevertheless, in the aged, any increase of dilatation due to a morbid cause may induce such murmurs, by destroying the relation; but the defective proportion necessary for their production exists less often in the aged than in the adult or adolescent, notwithstanding their greater liability to adynamic diseases.—*Archives Generales*, T. xiv., pp. 133–161.

[We consider the above a very interesting communication, and forming a worthy complement to the elaborate series of papers referred to. The mode of comparative admeasurement suggested is the only one that can convey any precise idea of the capacity of the heart in relation to the functions it has to execute.—*Rev.*]

Eschars over the Sacrum.

From the earliest period of his medical career, M. Blandin has always entertained most serious fears for patients in whom sloughing over the sacrum occurs; and in his *Anatomie Chirurgicale*, published in 1826, drew the attention of the profession to the almost sudden manner in which they often prove fatal. He believes he has discovered the explanation of this in the following circumstances. The point which suffers most from pressure in dorsal decubitus corresponds to where the sacrum is joined to the coccyx—exactly there, where the vertebral canal is only formed by the posterior sacro-coccygean ligament. Sphacelus in this way may easily reach the termination of the arachnoid membrane, and air, pus, or sanies gain admission into its cavity, producing a violent inflammation, which at first attacks the nerves of the cauda equina. Necrosis, too, may open a way into the vertebral canal with the same results; and in both cases the phlegmasia which results induces the phenomena of paralysis of the rectum, the bladder, and the lower extremities. "When I made my earliest observations I was in the medical wards, and the accident is of no unfrequent occurrence in typhoid fevers. You have observed in the patient who has given rise to these remarks (a case of amputation of the thigh otherwise proceeding favourably) retention of urine and paraplegia."—*Gazette des Hôpitaux*, No. 71.

Painful Crepitation of the Tendons. By M. Velpeau.

The man whom you have just seen is a dyer by trade, æt. 49, and his case deserves a moment's notice. A week since he endeavoured to raise a load, having his left hand applied to his hip. He felt a violent pain in this arm, and now we may perceive a slight swelling at the lower and external part of the forearm, unaccompanied by any change of colour or fluctuation. Of a regular and elongated shape, it is only painful during motion, while on applying the hand over it we may perceive a fine, characteristic crepitation; and it is an example of the *painful crepitation of the tendons* which was vaguely indicated by Boyer and Desault, described by me first in 1825, and has since formed the subject of the special writings of several authors. I first met with it in a case in the hospital of Tours, where it was suspected to be a fracture of the radius. The affection is especially observed among washerwomen, mowers, blacksmiths, locksmiths, and joiners, and when it is seated in the foot, among soldiers, huntsmen, &c. Excessive friction is the condition necessary for its pro-

duction. In the forearm and wrist, where it is especially met with, its recognition is very easy, the crepitation it gives rise to being quite pathognomic, being neither like that felt in fractures, that of cartilage or emphysema: but which has been compared to the crepitation of starch or of hoar-frost—such as is produced by walking on the snow. Its seat is evidently the sheath of the tendons, and it is probably due to a slight inflammation, first causing too great a dryness of the mucous membrane, and afterwards giving rise to effusion. It is generally in no-wise serious, disappearing in a few days by rest alone; but it must not be absolutely neglected, for I have seen it in some cases give rise to a fungous transformation of the sheaths; and indeed there is no reason why all the changes which occur in diseases of the joints should not take place here. If there is much pain we apply leeches and poultices, and the resolvent lotions and compression: but rest is indispensable.—*Gazette des Hôpitaux*, No. 82.

M. Royer-Collard on the Statistics of Insanity.

In a Report to the Academy of Medicine upon an Essay by M. Baillarger, entitled "*Statistical Researches upon the Hereditariness of Insanity*," M. Royer-Collard made some interesting remarks upon the undue importance given to this description of statement, a few of which, as having a far more general bearing than upon the case eliciting them, we here quote.

M. Baillarger's figures are as follows: they relate to 600 patients. Of 453 attacked by insanity in the direct line, the disease was transmitted 271 times by the mother, 182 by the father. Of the 271 families, in which it was transmitted by the mother, it occurred 70 times in more than one child, *i. e.* in more than a fourth: while of the 182, in which it was transmitted by the father, this was the case only in 30 instances, *i. e.* in one-sixth. In 346 cases in which the mother was mad, 197 girls and 149 boys were affected; and in 215, in which the father was mad, 128 boys and 87 girls were so. M. Baillarger arrives at the following conclusions:—1. Insanity of the mother, in relation to hereditariness, is more serious than that of the father, not only because it is more frequent, but also because it is transmitted to a greater number of children. 2. The transmission from the mother is more to be feared for the girls than the boys, and, on the contrary, that proceeding from the father is more to be feared for the boys. 3. The transmission from the mother is generally to be more feared for the boys than that from the father: but it is to be much more feared for the girls."

"If the choice of the subject merits nothing but praise," observes Royer-Collard, "I must be allowed to express some objections to the method by which M. Baillarger has pursued it. These objections he has indeed foreseen, declaring that his observations are as yet but insufficient, and stand in need of completion. But I do not hesitate to say that it is Statistics which is guilty of this imperfection. In the eyes of many, Statistics, which it has been so improperly attempted to raise into a science, is of all our methods of scientific investigation the most positive and certain. So much have what are called facts been of late abused, that their very images have been elevated into a worship, or rather an idolatry. Figures for some, and even enlightened, men, are an irrefragable expression of the truth. But let us not be deceived here: in the sciences, as elsewhere, figures, as well as words, have only a representative value. Statistics, which assembles them together and manœuvres them, is, of itself, blind and unreasoning; so that, to reach the truth which it promises us, we must absolutely penetrate all this drapery, and proceed straight to the things represented through the signs representing them."

After alluding to the well established general facts of the hereditary character of the physiological and pathological condition of the economy, and of insanity

among these latter, M. Royer-Collard observes that error and difficulty surround the subject when we attempt to submit this to arithmetical precision. In the first place, how are we to determine the hereditary relations of insanity? Admitting that we may be able to determine the insanity of individuals submitted to our examination; it is far from being always the case when we try to appreciate the mental condition of those we have never known. Where are the limits which separate sanity from insanity? A man has manifested peculiar tastes or strange ideas, without ever becoming mad. His son is manifestly insane, and will you call this hereditary or not?

"Another source of error deserves consideration. It is not only mental alienation, properly so called, that may induce insanity. All kinds of cerebral lesions, various nervous affections of uncertain nature and seat, certain congenital diseases of the organs of the senses, may exert the same influence. I may cite especially Epilepsy. What disease has more affinity with insanity, or is more frequently hereditary? Then again there is hysteria, hypochondriasis, and even that exaggeration of the general state of the economy termed 'the nervous temperament.' Apoplexy itself, and the habitual disposition to cerebral congestion, may easily, in transmission from parents to children, become in these a hereditary cause of insanity. The latter is only the result, the consequence of the original disposition which the son has inherited—only the chance of circumstances has terminated in him what nature had commenced. An accidental disease, a violent moral commotion, a change of climate, may determine in him the invasion of insanity, which the same would doubtless also have caused in his father. Lastly, there is the singular fact of some diseases which do not seem at first to tend toward any particular condition of the brain, and yet do produce, either directly or by hereditary transmission, an entire alteration in the intellect. Pellagra, a disease as strange as terrible, almost always gives rise in succeeding generations to a description of insanity hence named pellagrinous. So, too, physicians of lunatic asylums know how frequently ulcerations are met with near the ileo-cæcal valve in the melancholic insane having a propensity to suicide. And who can say that this is not one of the tortuous paths by which insanity is transmitted from parent to child?

"I had occasion lately to consult upon this subject one of the men whose opinion carries most weight in question of mental disease, Dr. Calmeil, and this is his reply. 'During 15 years I took the greatest care in interrogating those who brought their relatives to Charenton. I insisted much upon questions relating to hereditariness, and went into the minutest details. I noted apart all that might some day throw light upon this important fact. But when I had during several months, a year or more, continued to observe, to question, and to verify the exactness of the accounts which had been furnished me, I discovered eight times out of ten that I had been deceived by the ignorance of some, and the voluntary falsification of others. Every year I was thus obliged to undo my work and begin again, until I have finished by abandoning it.'"—*Bulletin*, T. xiv., 760—776.

M. Civiale on the Statistics of Lithotrity and Lithotomy.

M. Civiale recently read a paper upon the Value of Lithotrity before the Academy, in which he detailed the results of his large practice. These are thus briefly summed up. From 1824 to 1836 he had been consulted by 506 stone patients, of whom 307 were submitted to lithotrity, and 199 were considered as unfit cases. Among those lithotrized 9 were between 7 and 20 years of age; 55 between 20 and 40; 105 between 40 and 50; and 138 between 60 and 80. The number of cures amounted to 296; incomplete cures to 3; and deaths to 7.

From 1836 to 1845, of 332 stone patients, 241 were lithotrized, and 91 deemed

unfit. This gives a total of 838 patients, of whom 548 were treated by lithotrity. To these are to be added 25 operations on account of relapse; 8 combinations of lithotomy and lithotrity; and 10 treated since the tables were drawn up; giving a general total of 591 cases in 22 years, 566 of whom were cured, 14 died, and 11 were more or less incompletely relieved.

He comments at length upon the disingenuousness of the adversaries of the new operation, who in their criticisms upon the publication of the first portion of the above table, chose to comprehend in their enumeration of his failures the cases which he had declined to operate upon as unfitting. Out of the 838 stone patients, there were 290 in this predicament; and he observes that surgeons have not hitherto paid attention to this element in their calculations. In a statistical table which he long since published, containing 5,900 cases, in 859 lithotomy was not practised, and in 595 others all indications are absent; so that from the 5,900 we have to abstract 1434. Most lithotomists have indeed omitted to keep any account of the numbers in which the operation was either declined or considered unjustifiable.—*Bulletin*, T. xiv., p.821.

At a subsequent sitting, after referring to the various statistical tables of the sults of Lithotomy he thus states his appreciation of the two operations:—"1. That lithotrity, well performed, and within the limits of its legitimate application, saves 96 or 98 patients per cent. 2. That about a fourth part of stone cases which are refractory to lithotrity may be treated by lithotomy. 3. That by lithotomy employed exclusively, and without distinction of age, from 20 to 30 patients per cent. are lost. 4. Applied to children alone, it saves nine-tenths. 5. Applied to adults and old persons, it saves from 50 to 75 per cent."—*Gazette Medicale*, No. 34.

On the Induction of Cystitis and Albuminuria by Blisters.

M. Morel-Lavallée communicated to the Academy of Medicine the results of his researches upon the induction of Inflammation of the bladder by the use of Cantharides. These are, first as regards the *Etiology*. 1. The mode of preparing the blister is indifferent. 2. In general the action is more certain and more marked in proportion to the size of the blister; although the author has seen a blister not larger than half-a-crown placed upon the forehead determine the production of a false membrane of the bladder. 3. The distance of the topical application from the bladder is a matter of indifference. 4. It may happen that, upon the same subject, one blister produces no ill-effects upon the bladder, and yet, after a short interval, another of precisely the same size and make, re-acts powerfully on that organ. Generally, it is the last one which produces the inconvenience, though it may have been placed in quite another place, in which the epidermis offers the same defensive characters. Is this because the progress of the disease, which renders the topical application necessary, induces a debility favourable to absorption? 5. The preventive power of camphor is illusory.

Symptoms.—1. *Albuminous Secretion.*—The albumen may exist in three states: 1, in that of solution; 2, as a deposit at the bottom of the vessel; and 3, as false membrane forming in the bladder. The albumen existing in a state of *solution* is infinitely more abundant than in Bright's disease. In exceptive cases there are mere traces of albumen, but then the symptoms referable to the state of the bladder, as pain, &c. are few or none. In the third stage of the affection the *false membranes* vary in thickness from that of the merest pellicle to that of half a playing card. Sometimes they are rejected in such considerable rolls as to require traction to aid their expulsion from the urethra. Albumen is always found

dissolved when it also exists as a deposit or false membrane; but the deposit is generally absent even when false membranes are present. 2. *Functional Symptoms.* Sometimes there is no pain, &c., and the albuminuria is the only symptom of the action of the cantharides on the bladder; but generally there is frequent micturition, strong smelling urine, periodical pains, &c., &c.—*Revue Medicale*, June, p. 288.

M. *Bouillaud*, likewise, recently called the attention of the Academy to the frequent production of albuminuria by blisters. He stated that, for a long time past, he had detected albumen in the urine of persons in whom no suspicion of serious disease of the kidney could be entertained. Recent observations have confirmed this remark, and have likewise shown him that the action of cantharides is capable of inducing its appearance. In the cases of patients admitted for other causes than Bright's disease, large blisters were laid upon regions of the skin, wherein cupping-glasses had been applied. One of these was a man suffering from pleurisy with some effusion, whose urine tested had given no trace of albumen until after a blister had been applied, when it appeared in abundance—symptoms of strangury likewise existing. Blisters have since been applied upon eight or nine other patients, and in all of these next day albumen, which had not previously existed in it, was found in the urine (always most markedly so when the blisters had been applied to surfaces previously denuded of epidermis), tenesmus being likewise produced; and in other patients who were blistered without any such effect on the bladder resulting, albumen was not found.

M. *Martin-Solon* observed that we should not confound a transitory albuminuria of this sort with albuminuria properly so called. In the first, the urine continues healthy, some albumen only being mixed with it; while, in the other, it undergoes a complete change, a description of combination resulting.

At a subsequent sitting, M. *Vernois* detailed the observations he had made confirmatory of those of Morel-Lavallée. Blisters were applied upon 26 men and 9 women, and albumen was found in the urine of 16 of the former and 3 of the latter. Strangury existed in 15 of the men and 3 of the women. In 7 of the men this existed without any coincident albumen. In two cases, in which the blisters were applied over scarifications, the pain and albuminous deposit were proportionally greater. In two ecchymoses, and in one phlyctenæ, were found on the bladder after death.—*Bulletin de l'Academie*, Tom. xiv. p. 745.

In reference to this subject M. *Miquel* has addressed a letter to the *Gazette des Hôpitaux* (No. 82) upon the best mode of preventing the irritation of the bladder here referred to. He says that M. *Bretonneau* and himself, after long experience, have found this to consist in not leaving the blister on the part too long. Four hours often suffice to produce vesication, and the intervention of a piece of tissue paper in nowise impedes this.

He takes the opportunity of reporting upon the excellent effects M. Bretonneau has derived from the employment of *large flying blisters*, after local or general bleeding in rheumatism; these being applied to all the joints affected, so that three or four different ones have sometimes been blistered within the 24 hours. They are always taken off when the patient has complained of their irritation, for 15 or 20 minutes, which he does in from 4 to 8 hours. Vesication may only follow the second dressing, and, if the blister has not been kept on too long and the epidermis has not been destroyed, the pain is very tolerable, and the healing prompt. M. Bretonneau has observed that when the skin has been repeatedly blistered, it at last ceases to vesicate, although there is nothing anormal discoverable about it. Small pimples on a red ground are produced by the blister, and if this be retained on, the bladder will become irritated though the skin do not vesicate. M. Miquel has found that a piece of the omentum of the calf or pig, which, if washed, will serve several times, forms the best *dressing* for blisters, especially if the epidermis has become

torn. For the removal of the little pimples, furuncles, &c., which spring up around the margin of blistered surfaces, especially if these have been dressed with rancid substances, he finds the *sulphuret of potass* to form the best application (1 part to 20 of water).

In relation to this subject we may here notice a new formula for a very active Blistering Plaister devised by M. *Mialhe*. After noticing the effects which cantharides sometimes produce on the urinary organs, he goes on to say—

"Upon what does this phenomenon depend? Doubtless, it must be referred to the absorption of the active principle—cantharidine: and to prevent its production we should put a stop to such absorption as soon as the desired local effect has been produced. Among the means suggested for preventing the specific action of cantharides upon the bladder, none is so good as that indicated by M. Bretonneau—the interposition of a piece of tissue paper soaked in olive oil. Cantharidine being soluble in fatty bodies, the oil serves as a vehicle for its introduction into the economy, but this introduction ceases to be active as soon as the serous effusion has taken place, since oily are not miscible with aqueous fluids. Another means consists in the association of *camphor* with the epispastic, but it attains its object less frequently than the other plan, notwithstanding that some practitioners consider it as infallible. Nevertheless, we believe the association of camphor with vesicating plaisters useful for two reasons. First, the abuse which has been made of camphor* of late has placed beyond all doubt the fact that this substance is endued with anaphrodisiac properties; and secondly, that, inasmuch as it possesses the power of softening the resinous substances contained in blistering plaisters, these become more fluid, adhere better to the skin, and consequently act more promptly. Now, we have assured ourselves by experiment that, all things being equal, the dynamic action of cantharides is so much the less to be feared as the local action is rapidly produced; or, in other words, that the absorption of cantharidine is so much the more marked as the serous effusion has taken long to produce. Hence we conclude: 1, That we should give the preference to the most active epispastic; 2, That vesicants should only be kept in contact with the skin for the time strictly necessary to effect a detachment of the epidermis; and 3, That camphor forms an advantageous addition to blistering plaisters." The following is the formula for preparing an active one:—

Cantharides . . .	400 parts.
Lard	25
Veal Suet . . .	25
White Pitch . . .	50
Yellow Wax . . .	100
Sulphuric Ether . .	100
Camphor . . .	40

"Powder the cantharides without previously drying them. Pass them through a silk sieve, and suspend the pulverization as soon as 100 parts of a fine powder have been obtained, which are to be placed in a wide-mouthed flask with the ether. Place the rest of the cantharides in a tinned pan with the lard and suet, and a sufficient quantity of water to cover them completely. Boil this gently for an hour, continually stirring it the while, and then let it cool in the pan. Next separate the fatty matter at the surface from the settling at the bottom, which last is to be thrown away. The fatty matter collected is to be melted, without

---

* Camphor, under the auspices of that arch-quack M. Raspail, has obtained as much vogue among some classes of our neighbours as have Morrison's Pills, and other nostrums among ourselves.

water, and strained through a cloth into a tin water-bath, the pitch, wax and camphor added, and the whole heated until complete fusion takes place. Lastly, add the cantharides prepared with the ether, heat until the ether is entirely evaporated, that is to say, for about an hour, pour the plaister into a marble mortar, and stir it until entirely cold.

"The effect of this vesicant is very prompt, taking place in from two and a half to three hours, according to the susceptibility of the cutaneous tissue, the more or less elevation of temperature of the part, and the closeness of the adhesion of the plaister. It offers some analogy with the *Emp. Cantharid.* of the London Pharmacopœia, which is well known to be very preferable to that of the Paris Codex; but it is more active, as we have assured ourselves by repeated trials: and this because it contains a larger proportion of the fly; and because all the blistering principle contained in it exists in a state of perfect solution, by reason of the manipulation it has been subjected to."—*L'Union Medicale*, No. 22.

[Judging from the frequency with which the subject of strangury following the application of blisters is noticed in the French writings, we suppose it is a matter of common occurrence. In our own practice we seldom or never meet with it, which perhaps may arise, in some degree, from our always directing the early removal of the blister. Still, among the common people, it is no uncommon occurrence to leave a blister on from 12 to 24 hours, long after the epidermis has been raised or even ruptured by the fluid effused beneath it, and yet strangury is rarely met with.—*Rev.*]

On Abscess of the Breast. By M. Velpeau.

"Subcutaneous inflammation of the Breast" proceeds much as an ordinary phlegmon. When the abscess is formed between the mamma and the chest, the swelling is considerable, the breast raised up, but after an incision the cure usually takes place rapidly. But when the phlegmasia invades the substance of the breast itself, it is rare to find only a single abscess produced. We sometimes see 10, 20, 40 or 50 manifesting themselves in succession. An instant's reflection will show that this result is a natural consequence of the anatomical disposition of the inflamed tissue. The glandular parenchyma consists of different lobules, each of which constitutes a little organ having its own function, and which may become heated and irritated under the influence of lactation. Each lobule does not attain at the same time the same degree of irritation. One first inflames, then suppurates, and constitutes a first abscess: a neighbouring lobule then becomes affected and, in its turn, forms an abscess; and so it may go on with all of them until we have as many successive abscesses as there are lobules.

"This distinction of abscesses of the breast into at least three orders is of the highest importance; and if we do not adopt it, our ideas upon the subject will be but very vague, and devoid of all precision as respects prognosis and treatment. Parenchymatous abscesses may last four or six months, or a year even, according to the rapidity of their succession and their number. The subcutaneous abscess lasts only as long as an ordinary phlegmon; and the submammary abscess has not the long duration of the parenchymatous one.

"Each of these has again its special treatment. We may endeavour to procure the resolution of *subcutaneous abscess*, and that by ordinary means; and, if suppuration occurs, we open it promptly, in order to avoid the burrowing of the pus among the tissues. *Sub-mammary phlegmon* should be treated especially by general measures, and leeches around the nipple. Topical applications are of little use, as they are separated from the centre of inflammation by the whole

substance of the mammary gland. When an abscess is formed here, its prompt evacuation is desirable: but the perception of fluctuation is difficult, for the pus is surrounded by a large mass of tissues, and the thoracic parietes have not fixity enough to serve as a point of support. Nevertheless, you may recognise the existence of pus by the following characters: 1. An acute phlegmon rarely exists more than seven or eight days without suppuration taking place. 2. The breast is raised up like a sponge, and if we press upon it, it seems as if it were lying on a bladder full of fluid. 3. We find the breast surrounded by a kind of inflammatory œdema. Having recognised the pus, we should let it out promptly, or we expose ourselves to seeing it traverse the gland and form one of those abscesses I call *shirt-buttons*. These abscesses, moreover, have a mischievous influence upon the chest, and may lead to a purulent pleurisy. They may, too, penetrate into the cellular tissue for a distance, and give rise to a diffused phlegmon. The incision should be made into the most dependent part, the place of election being below and at the outer side of the nipple, but, in some cases, a projecting point of the abscess indicates the place at which the opening should be made. It is always advantageous to make the incision towards the circumference of the breast, because the gland itself is not touched, and its weight tends to expel the pus. The bistoury should be directed almost parallel with the thoracic parietes, so as to slide it in between these and the mamma. The danger of such incisions is not great, there being no large arteries to fear. *Parenchymatous phlegmon* requires an energetic and varied treatment. Bleeding, purging, and the so-called anti-lactal medicines. When pus forms, which is almost always the case, topical applications and incisions seldom prevent the successive implication of the lobules. Nevertheless, there is some advantage derived from the prompt opening the abscess, *if the patient agrees to it;* for you should recollect that, in practice, if you open one abscess and others form, she never fails attributing these to your proceedings. These details will, I think, suffice to show you how important it is to distinguish the different abscesses of the breast, and to explain to you the confusion which prevails in the minds of some surgeons as regards their treatment."—*Gazette des Hôpitaux*, No. 89.

[The above remarks, like all that falls from this excellent surgeon, are well worthy the attention of the practitioner. We have seen much needless suffering caused by the formidable operation of incising through the whole substance of the breast, to get at an abscess situated at its posterior part, and which might have been much more easily reached by the circumferential incision here recommended.—*Rev.*]

On the Nature of the Liquid secreted by the Mucous Membrane of the Intestines in Cholera. By M. Andral.

M. Andral recently read a note at the Academy of Medicine, giving an account of the researches he has been engaged in for the purpose of determining the nature of the peculiar white matter, resembling a decoction of half-cooked rice, which is found in the digestive organs of patients attacked with cholera, and which especially belongs to and characterizes that affection. From the facts detailed, he drew the following conclusions:

"1. The white matter which fills the intestines of cholera patients, is not, as it has often been stated to be, a portion of the blood itself; for neither albumen nor fiurine are found in it. 2. It is nothing else than mucus rapidly secreted in large quantities, and for this reason modified in its qualities. 3. The essential microscopic character of this matter is its containing a very considerable number of cells, with nuclei perfectly resembling, as far as regards their appearance, the cells

found in pus, although this matter in no other respect bears any resemblance to pus. 4. The examination of the blood of cholera patients shows that the albumen of the serum is maintained in its normal proportions. 5. The theory which refers the symptoms of the stage of cyanosis in cholera to the change which the blood has undergone by reason of a great and sudden loss of serum, cannot be admitted."—*Gazette Medicale*, No. 33.

Induction of Premature Labour for the Prevention of the Death of the Infant in Miscarriage at the Eighth Month.

Dr. Secondi proposed the following question for the consideration of the Medical and Surgical Section of the Eighth Italian Scientific Congress, held at Genoa, September, 1846: "*Whether, in the case of several successive deliveries of a dead fœtus in the course of the eighth month, it is not admissible in a consecutive pregnancy to induce premature delivery at the seventh month?*" In support of the proposition he narrated two cases observed by him; one of a lady who was delivered in eleven successive pregnancies of dead children at the eighth month, although the fœtus had continued alive after the seventh, and every means that could be suggested by art, and these of different kinds in the respective pregnancies, had been put into force, in order to avert the catastrophe; in the other case, the lady miscarried in this way five times. In both these cases the position of the patients was such as to admit of their adopting every hygienic precaution recommended, their health seemed quite good, and they were taken in labour without any indications of pre-existing or present affections, the fœtuses and their dependencies likewise not manifesting any signs of disease. Reasoning upon these facts, it seemed to him that these women were governed by different vital laws, either primarily so, or secondarily dependent upon the presence of the fœtus, by reason of which the nutritive relations between it and its parent ceased, and it was separated just like a fruit from the tree. As the provocation of premature labour does not compromise the life of the woman, and very little endangers that of the child, he answered the question in the affirmative.

Dr. *Arata* supported this conclusion, and related two cases in point. In the one the accident in question had occurred eleven, and in the other five times, in spite of the employment of every kind of therapeutical agency; and notwithstanding that the subject of the last case was of a favourable temperament and in good health, and the other, in consequence of a suspected syphilis, had been duly treated. He added that the fœtuses possessed a due development, with no indication of disease in them or their dependencies; and that if we induced premature labour on account of a narrowed pelvis, we should do so also in the case in question.

Professor *Centofanti*, although inclining to Dr. Secondi's opinion, would first wish to learn the cause which prevented the fœtus continuing to live after the eighth month, and whether it died really in the eighth month or before. The causes which lead a woman to abort in the eighth month must be either inherent in her own organisation, or in that of the fœtus or its dependencies. He believed that the causes inherent in the woman, if not organic, may be removed by a properly adapted method of cure; but that such is not the case with those dependent upon vices of the fœtus or its dependencies. Where, too, they depend upon fœtal disease, before we can determine whether we can or not prevent the bringing forth a dead fœtus, it would be requisite to examine whether these all were derived from the fœtus, or from the special condition of the woman reflected back upon the fœtus. Lastly, it is to be recollected that a fœtus may die at the fifth or sixth month, and continue to remain in the uterus until the eighth, providing the membranes have not been ruptured: so that the propriety of the operation is confined to those cases in which we are certain that its death takes place in the eighth month.

Professor *Vannoni*. We should look at this question in its true aspect; the death of the child. What ought to prevent us endeavouring to save an infant which would

otherwise die at the eighth month? And why is the Cæsarian section or symphysisotomy instituted but that the fœtus has a right to social life? He considered the question of the cause of the death of the child at the eighth month as one of the highest moment, and besides the recognised organic causes, and the maternal or fœtal diseases, &c., he would take into account other—*physical*—causes. It does not suffice for the explanation of these accidents to invoke a particular condition of the woman. There are other causes on the side of the fœtus. We should bear in mind that the fœtus, having reached a certain point, can live no longer, although the mother is healthy, and it itself, after expulsion, exhibits no mark of disease: and we must admit here an intrinsic cause which operates within the uterine cavity, by reason of which the fœtus has reached such condition of development at the eighth month, as to be already old and unable longer to retain life. When, therefore, the ordinary means are at fault, and a careful diagnosis has been made of the causes which could perchance give rise to this accident, premature labour should be induced. But are we certain, by this means, of preserving the child's life? To decide this we have need of experiments, and in inducing premature labour even in the case of our not succeeding in saving the fœtus, we have removed it from the cause of death to which it was exposed.

The Section eventually came to the following decision:—*That when, on four or five successive occasions, a woman has been delivered of a dead fœtus at the eighth month, it is justifiable in future pregnancies to induce premature labour.*—*Annali Universali*, Volume 121, pp. 172–5.

[This application of the induction of premature labour is new to us, and one of very doubtful expediency. The question of the cause of the death of the *fœtus in utero* is involved in too much obscurity to admit of positive opinions upon many points being pronounced; but it seems to us, when we bear in mind the great difficulty in preserving and rearing premature children, that one whose vital powers were so limited as not to admit of the retention of life *in utero*, would have its chances of so doing little if at all increased by inducing its more hasty expulsion. Admirable as is this resource in appropriate cases, and foremost as have the practitioners of this country shown themselves in employing it, the induction of premature labour is not an operation to be extended to additional emergencies without the gravest consideration and the strongest probabilities of success. Independently of the danger of too hasty a generalisation of it in a moral point of view, it is by no means so unattended with risk to the mother as our Italian brethren (prevented by priestly interference from employing it in appropriate cases) would seem to infer.

We may take this opportunity of expressing the great pleasure we have derived from the perusal of the proceedings of the Medical Section of the Congress. Several excellent and learned papers were read, the spirited discussions upon which well proved that the Italian physicians and surgeons were as earnest participators in the progress of medical science as their brethren of any other portion of Europe. This Section seems to have taken a prominence in the proceedings of the Congress not observed in the scientific periodical assemblages of France or England, which is probably due to the far more rapid diffusion and interchange of ideas which takes place by means of the organs of the medical press in these two latter countries than in Italy—where discussion of any kind is but a thing of yesterday.—*Rev.*]

Purulent Ophthalmia of New-born Infants.

M. *Chaissaignac*, surgeon to the Foundling Hospital at Paris, has recently addressed a letter to the *Académie des Sciences*, giving some account of the views he entertains concerning this affection, and of the success which has attended his treatment of it by irrigation. Since then, M. *Laborie* has attentively examined the subject in M. Chaissaignac's ward, and furnished the following account for the pages of *L'Union Medicale.*

When M. Chaissaignac received his appointment to the Foundling Hospital, he was struck with the great number of children who were suffering, in different degrees, from the consecutive effects of purulent ophthalmia. Attentively examining the disease for the purpose of endeavouring to prevent the continuance of such calamitous results, he discovered that this inflammation presents a characteristic which has hitherto escaped observers. With very few exceptions, a *pseudo-membrane* was found to be present on the conjunctiva, the removal of which was found much to facilitate the successful issue of the disease. This assimilates the disease to various other affections frequently met with in infancy, such as muguet, diphtheritis, croup, &c. This result, to which M. Chaissaignac's direct observations led him, appeared to us to be beyond all doubt; for we had already remarked during our residence at the *Clinique*, a remarkable and almost constant coincidence between the appearance of muguet and purulent ophthalmia. So constantly did these affections show themselves one after another, that M. Paul Dubois always predicted to us, when he had observed some cases of muguet in the wards, that we should soon see examples of purulent ophthalmia.

In his letter to the Academy, M. Chaissaignac reports the great success which has attended his mode of *treatment by irrigation*, and it will be as well to state what induced him to adopt this, and the mode in which he employs it. Being in charge of the *Hôpital de l'Ourcine*, he had a great number of women under his care who were suffering from *vaginitis*, attended with the most obstinate discharges, which every form of the ordinary treatment, perseveringly tried, failed to subdue. Believing this to arise from portions of the mucous membrane which the applications had not reached, constantly secreting a contagious liquid, it struck him that keeping these parts constantly washed might lead to more fortunate results. Since this has been put into force far greater success has attended the treatment by medicated injections, &c.

In purulent ophthalmia, also, the remaining portion of the mucous membrane is much rather affected by contagion than by continuity of surface, and *constant irrigation* seemed the most effectual means of preventing this through its prompt removal of the contagious matter. M. C. commenced his experiments very carefully, until he had ascertained that the means was positively harmless when not efficacious. The following is the manner in which the irrigation is employed. A reservoir is fixed against the wall at about three or four yards from the ground, having two cocks at its lower part, to which are adapted caoutchouc tubes, with apertures of about 2 *millimetres* diameter. By means of a moveable diaphragm the force of the stream may be regulated. The child is laid upon a table covered with waterproof cloth, with its head placed against the wall under the reservoir, and slightly inclined backwards, so that the rest of the body is not wetted. The two tubes are brought to a level with the head, and the operator directs the jets first upon the eyelids, then against their edges, and after some minutes, over the entire conjunctiva. The operation is continued for 10 or 15 minutes, or longer, and is repeated several times a day. Generally, the child manifests no sign of suffering as long as the eye-

lid is only watered, and sometimes not even when the interior of the eye is; but for the most part it then complains, and, if the operation is too prolonged, becomes very impatient under it, and cries, so that a suspension becomes necessary. During the operation the eye becomes a little red, and all the secretions and pus are removed from the edges of the eyelids. When the fluid pus has been thus removed also from the mucous membrane, the conjunctiva is found of a greyish aspect, which seems to result from a purulent deposit. But in spite of washing this does not disappear, and it is in fact the *false membrane* indicated by M. Chaissaignac, which, under the influence of the water, becomes more opaque. It may be raised and removed without much difficulty by means of a pair of fine forceps. Sometimes no erosion of the surface is left by its detachment, but at others there is all the appearance of a true wound left. Sometimes the membrane is very extensive, covering all the ocular conjunctiva as well as the cornea. At others it is only partial, being seen at several unconnected points. When the irrigations have been employed and this membrane removed, it is not reproduced upon the same part. Astringent collyria are used during the day, although, since he has employed irrigation, M. C. seldom has recourse to these.

M. Chaissaignac has employed the irrigation in other affections of the eye with advantage, such as obstinate *blepharitis*. Of 76 patients admitted during the last ten weeks, 40 were suffering from well-marked purulent ophthalmia, 20 from various forms of blepharitis, 7 from papular conjunctivitis, and 4 from old opacity. The duration of treatment has, for the purulent ophthalmia, occupied a mean period of 10 days, for the conjunctivitis 4 or 5 days, and for the blepharitis 3 weeks. In no one of the cases of purulent ophthalmia did the cornea become affected.

M. C. does not attribute to the irrigation any great special medicatory power, it being, in fact, chiefly a hygienic measure for the removal of the abundant discharge: and collyria are employed while resorting to it. But under its influence the false membrane becomes more easily visible and consistent, and consequently more easily detached; the removal of this, which acts upon the eye with all the injuriousness of a foreign body, expediting the cure, and rendering it more certain. The application of cold also, of such utility in other inflammations, doubtless has a beneficial effect in this.

In appreciating the success of this treatment, we must not, however, overlook that also derivable from other procedures; thus, we have not met with a single case at the Clinical Hospital, during two years, which did not yield to the following means, viz., a collyrium of nitrate of silver (32 parts Aq. dest., $\frac{1}{10}$ part Nit. Arg.;) a slightly emollient collyrium frequently during the day; great cleanliness of the parts to be secured by preventing the morbid secretions remaining in contact with the eye. In very rare cases a slight application by means of a pencil of nitrate of silver is made to the inner surface of the eyelids. M. Paul Dubois says he has never known an eye to be lost under this treatment.

We have already stated that M. Chaissaignac has treated several cases of blepharitis by irrigation with great success. Several of those we examined were examples of ciliary *blepharitis*, and the obstinacy of this disease is well known. A circumstance which contributes to the tediousness of cure is the manner in which the secretion from the Meibomian glands glues the eyelashes together, forming them into hard brushes which simple lotions can scarcely render supple. By means of irrigation, which produces no painful or disagreeable sensation, they are freed in some minutes from all concretions. They become supple and no longer stick together.

M. C. likewise believes that *staphyloma* may be advantageously treated by irrigation. He does not pretend to say it may be so cured, but, under the use of this means, he has observed a positive arrest in its progress, and sometimes even a slight dimunition of the projection. The douches in this case seem to exert a resolvent and tonic effect.—*L'Union Medicale*, Nos 108–109.

[When M. Chaissaignac's letter was read at the Academie, M. *Flourens* took

the opportunity of stating that he suspected the so-called pseudo-membranes of the mucous membranes were nothing but portions of detached epithelium. M. *Velpeau*, while believing this may sometimes be the case, considered that it was not so in the present disease. Diphtheric inflammation is not confined to the mucous membranes, for it may affect any region of the skin; and, on its removal, this structure be found sound beneath.

Although we certainly have never expressly examined the eye for the detection of the false membrane in question, we feel somewhat sceptical as to its usually forming a characteristic of this disease. It may, perhaps, do so sometimes in the impure atmosphere of the Paris Foundling Hospital, but it could scarcely have escaped detection in private practice were it a common occurrence. We have frequently seen the discharge assume an almost membranous tenacity. In respect to treatment, it seems to us that free syringing the eye with cold water would accomplish all the objects derivable from irrigation; but we would not like to employ this as any other than a subsidiary means. The hourly use of injections of sulphate of zinc or nitrate of silver generally rapidly subdue the disease, although the assistance of a leech may occasionally, but very rarely, be required also, if the inflammation, as denoted by the tumefaction of the lid, is excessive.—*Rev.*]

On the Treatment of Typhoid, or Entero-Mesenteric, Fever by the Black Sulphuret of Mercury. By M. Serres.

M. Serres has recently read some papers upon this subject before the *Academie des Sciences*, and as they have excited much attention in Paris, a notice of some of the principal points dwelt upon by him may prove acceptable to our readers.

He believes that the symptoms, progress, and anatomical lesions of this disease all show that it belongs to the exanthematous fevers, and this fact constitutes the basis of the proposed treatment. The histories of measles, scarlatina, erysipelas, but especially of variola and vaccinia, prove that the amount of fever is proportionate to the amount of eruption; if this is discrete the fever is slight; if it is confluent the fever is intense; "it becomes confluent also by the change which takes place in the composition of the blood, and the phenomena of re-action which are developed throughout the system." As long ago as 1812, the author, together with M. Petit, endeavoured to domonstrate a like dependence of typhoid fever upon the amount of entero-mesenteric changes which were developed; and 35 years additional opportunities of investigating the subject at La Pitié and the School of Anatomy, where bodies are brought from all the hospitals of Paris, have conferred upon the proposition all the certitude attainable in medicine.

"If, as now stated, every eruptive fever is compounded of two distinct elements: of the eruption, which is the dominating element, and of the fever which is the dominated element, the therapeutical course is traced out in this disease by this subordination of the phenomena. Reasoning indicates this, and medical experience has demonstrated it. In the remarkably faithful picture drawn by Sydenham of the progress and generation of symptoms in the small-pox (the passages are quoted, but they are or ought to be familiar to our readers), we recognise that of the typhoid fever which was furnished by M. Petit and ourselves. If, in fact, in the comparison of the two diseases, we form an abstraction of the eruption or fundamental portion of each, we find a perfect resemblance in the phenomena of the consecutive fever constituting them—the same infection of the blood—the same permanence in the

source of the infection—the same saturation of the system with a deleterious principle. The bases of their therapeutics should partake of and reflect this uniformity. But, for the bases of therapeutics to assume such conformity, they must be able to extend to the foundation of these two diseases. And here is the difficulty. With respect to small-pox the etiology has never been contested. All agree that, beyond the affection of the skin, there is a general affection, having its vehicle in the mass of the blood. It is not the same with the etiology which is here given of typhoid or entero-mesenteric fever. Eminent observers, and whose consummate experience might well serve as a guide in medicine, have entertained an opposite opinion. They have seen in the disease only an enteritis, or an inflammation of the intestine, different degrees of which might explain the general and local symptoms by which it reveals itself."

"The treatment I propose consists in the administration of the *black sulphuret of mercury* in the form of pills, and the inunction of the abdominal parietes by means of the mercurial ointment every morning. Four grains of the black sulphuret are formed into a pill with tragacanth and syrup; and from four to six of such are given every second day. The treatment may be continued for six or eight days, provided no stomatitis occurs. If the mucous membrane of the mouth becomes inflamed the frictions are to be suspended and the sulphuret diminished or discontinued, applying alum gargles or slices of lemon to the gums.

"Although every one recognises that the gravity of the disease is dependent upon the amount of the intestinal eruption, no one has hitherto tried to treat this topically. Purgatives in general fulfil the first indication of treating the general poisoning of the system; but it is the mercurial purgative which alone exerts a special topical action on the intestinal patches. We cannot give proofs of this direct action; but from the effect which mercurials exert on analogical diseases, we are enabled to make an *à posteriori* induction upon the subject. We know that the application of mercury procures the abortion of variolous pustules. Mercurial frictions dissipate an erysipelas springing from internal causes—as they do the rose-coloured lenticular patches which appear on the abdomen in typhoid. The diarrhœa and distension of the abdomen in typhoid are certainly due to the irritation which the intestinal eruption determines upon the mucous membrane of the intestines; and both these symptoms are relieved under the use of the black sulphuret (although ordinary purgatives fail to relieve them), proving that this exerts a topical action upon the intestinal eruption, preventing or arresting its development. But it likewise exerts a more generally beneficial effect upon the organism, seeming as if it reached the cause of the disease itself;—the fever becomes less, the pulse diminishes in number, and the delirium abates—and this in so decided a manner as to be obviously the result of the medicine. By this method we do not abridge the duration of the fever. It continues, as under other treatment, for 3 or 4 weeks; but generally, when seen early, it is conducted through its course without any accident arising."

"Although the lenticular and rose-coloured spots on the abdomen, which constitute so characteristic a symptom of typhoid fever, differ essentially from the variolous pustule, yet the septic nature of the two diseases, the concomitant changes in the state of the blood, led me to the study of the action of mercury on these petechiæ. In the year 1845, this petechial eruption was remarkably abundant in most of these patients; but, under the application of the mercurial ointment, they disappeared very rapidly—the accompanying meteorism simultaneously diminishing. This double result led me to conclude not only that the mercury operated beneficially upon the petechiæ, but also upon the intestinal eruption, which constituted the foundation of the fever. If this last conclusion were correct, it was reasonable to suppose that, could the mercury be brought into direct action on the intestine, its effect would be still more prompt and efficacious; and, after an attentive examination of several pharmaceutical preparations of this metal, the black sulphuret seemed the best adapted to fulfil the desired indication."

Some particulars of a few of the cases which have fallen under M. Serres' notice are furnished, and he draws the following conclusions:—1. The fever and

cephalalgia have been evidently influenced by the second or third day of the medicine. 2. The pulse has fallen below the mean, and even become remarkably slow. 3. No adynamic or ataxic accidents occurred; and, when adynamia appeared at the commencement of the disease, it was soon removed. 4. The quantity of ethiops employed to procure these results has not exceeded 50 grains, and several times but 30 have been administered. 5. Only a slight stomatitis, of which the patients hardly complained, was produced. 6. Convalescence was fairly established from the 8th to the 15th day, return to health always having been accomplished without relapse. 7. The patients left the hospital entirely cured in between 30 and 50 days; although they were encouraged to stay in as long as possible for the purpose of observing any relapse if such occurred.—*Gazette Medicale*, Nos. 33 and 34.

[That the consentaneous employment of the black sulphuret of mercury and mercurial inunctions have proved highly useful in typhoid fever, the statements of so intelligent an observer as M. Serres sufficiently prove; but that such effect has resulted from the ectrotic effect this substance has exerted upon the diseased follicles of the intestinal canal is the purest conjecture. Even in the analogical case of small-pox, so much dwelt upon in the memoir, mercury exerts no specific action in producing the abortion of the pustules, the same end being attainable less conveniently by the use of caustic and more conveniently by the application of blisters. Still, with any of these aids, the progress and result of the disease is capable of very slight modification, though for the prevention of disfigurement of the face, they become valuable subsidiary means. We are not told whether the sulphuret acts as an active purgative. If so, its utility may to some extent be explained, as many of the French practitioners have derived great benefit from this class of medicines, mercurial and otherwise, now the belief in the disease being essentially an enteritis has nearly passed away.—*Rev.*]

The Medical Profession in Paris.

According to M. Domangé's Medical Almanack, the number of practitioners in Paris 1st Jan. 1847, amounted to

	Doctors of Medicine . . .	1442
	Officiers de Santé	165
		——1617
To these may be added,	Pharmaciens	345
	Midwives	480
	Total	——2442

In estimating the amount of practice which falls to the lot of these 1617 qualified medical men (at Paris as elsewhere, charlatans and unlicensed persons abound likewise,) we find the official statements in 1846 give us a population (including, however, hospitals, colleges, and garrison) of 1,053,897 souls. In 1845, of 32,905 births, 5,678 occurred in the hospitals, that is, more than a sixth part did not fall into the hands of the practitioners and midwives, these last taking the lion's share of the remainder. In the same year, 25,156 deaths took place, of which 15,888 took place at their own houses, the rest dying in hospitals, &c. In 1845 there were 1598 practitioners, and dividing the deaths among these, we find about 10 must fall to the lot of each. In the hospitals, the proportion of deaths to patients is about one-eighth; but if, in the easier classes of society, we call it a fifteenth, each practitioner would then, upon an average, have an annual practice among 150 patients, or 12 per month. We may leave to others to calculate the mean number of visits these may require, and the product of such, at charges of 5, 3, 2 francs, or even less—to say nothing of bad debts. This, if patients were

equally divided; but there are some few of great repute who receive their £4000, others whom the hardness of the times leaves only £2000; and so on, from step to step, to those who, yet better provided for than the mass of their brethren, with great difficulty attain £200 at the end of the year. Lower, and much lower still, there are practitiouers who make as little as £60, £50, or £40 per annum; and how many are there who in the first year of their practice do not take £20.

And yet the evil is on the increase: the number of practitioners is constantly augmenting. In 1845 there were but 1430 doctors: 50 died and 62 left; but this vacuum of 112 was not only filled up, but replaced by 124. In 1845 there were 168 *officiers de santé*, 175 in 1847: so that the total addition of practitioners in only two years amounted to 19. In 1845 there were 450 midwives, in 1847 there are 480; while the accouchements at home in 1845 only amounted to 27,227. Take from these the gratuitous confinements provided by the charitable societies, and those conducted by the practitioner, and there certainly cannot remain a mean of 50 patients per annum for each midwife. The majority then cannot live by their profession. What then do they live by? That would be worth while inquiring into! —*L'Union Medicale*, No. 102.

[The writer goes on to recommend the limitation of the number of the practitioners to the wants of the population; forgetting apparently that every occupation and profession is in like manner overburdened and could put forth equal claims to repression, if these were not perfectly inadmissible in the present state of society, which is to be governed by moral and prudential restriction, and not by the exclusivism of a worn-out period. The medical profession has, however, the remedy in its own hands in a great measure, by means of which it may right itself, and yet benefit the public, namely, the increasing the period of study and the amount of requisite qualifications. Certainly the pecuniary position of the medical body in Paris, if the above representation be correct, is far worse than that of our own capital; since, for our own two million souls, we have but 2,413 qualified practitioners, while for their one million they have 1617. Then again, the midwives with us are a most insignificant class, employed only by the poorest of the population as a matter of necessity; while too many of our own practitioners add to their strictly professional receipts the profits of tradesmen by vending drugs, perfumery, and even quack medicines! For our parts we prefer the honourable poverty of our neighbours to this lucrative but degrading practice among ourselves.—*Rev.*]

Observations on the Materia Medica.

1. *On Alkaline Medicines. By M. Mialhe.*—M. Trousseau has recently recalled attention to the fact that, when these are taken in too great a quantity, they increase the fluidity of the blood and deprive it of colour, and lead to cachexia, pallor, and general infiltration, passive hæmorrhage, and an irreparable wasting—giving rise to far greater and more irremediable evils than the disease they are employed to combat, and causing just as much mischief as the abuse of iodine, mercury, or the preparations of iron. I propose here to indicate some of the circumstances under which alkalis may give rise to these ill consequences, and those of a contrary character, under which perseveringly used, they may re-establish the deranged equilibrium of the economy.

First, we may observe that the administration of alkalis in excess does not give rise to so much mischief as does a similar abuse of acids. In the state of health the three principal fluids of the body, the chyle, the lymph, and the blood, are alka-

and the amount of alkaline base they contain is incomparably greater than

unt of acids contained in other fluids. It is then in an alkaline medium

that the animal organic mutations are operated, while in plants it is always in a neuter or acid medium that the phenomena of nutrition take place. The alkalis fulfil a far more important function of the economy than that of mere exciters and fluidifies, presiding as they do over the decomposition and assimilation of the hydrocarbonaceous substances of the amylaceous or cellular families.

But however the blood and other liquids may be physiologically a little more or a little less alkaline, it does not follow that they may be inconsiderately administered. They stand at the head of the agents which exert on the serum the most marked fluidifying action. All the alkalis affect our economy in an identical manner. They produce on the organ of taste an impression *sui generis* designated as *alkaline* or *urinary*. M. Clevereul showed this to be always due to the same substance, ammonia, which is set free by the decomposing effect exerted by the alkaline base on the hydrochlorate of ammonia contained in the buccal fluids. Experience has shown that the same chemical fact prevails in respect to the bicarbonates and carbonates, and all the other fluids of the living body. Hence, wherever we introduce any alkaline substance into the economy, a certain quantity of ammonia is set free. This explains why the ingestion of a certain quantity of bi-carb. soda dissipates the symptoms of drunkenness—the ammonia disengaged restores to the albuminons elements of the blood that fluidity which the coagulating action of alcohol had partially deprived them of.

Under what circumstances is the employment of alkalis efficacious or dangerous? Clinical observation shows us that the daily taking of a drachm or a drachm and a half of bicarbonate of soda, or its equivalent of any other alkali, so far from being generally injurious is frequently advantageous. Many persons can take far larger doses with impunity, while much smaller ones have, in some cases, induced serious accidents. All substances which produce an acid predominance in the blood allow of a large quantity of alkalis being taken. Thus the inactive inhabitants of towns, in whom there is hardly any acid secretion from the skin, especially in Winter, will bear large doses of alkalis. It is the same with those who live upon an almost exclusively meat diet, inasmuch as the albuminised elements containing sulphur and phosphorus, these two elements produce, by their interstitial combustion, phosphoric and sulphuric acids in marked proportions. This explains why the urine of the carnivora is normally acid, that of the herbivora always alkaline. On the other hand, whatever favours the predominance of alkalis in the vital humours forbids their employment. Thus the laborious inhabitants of the country, in consequence of their abundant acid sweats, can ill bear the ingestion of alkalis. So with persons who adopt an exclusively vegetable regimen, the blood is normally rich in carbonate of potass, by means of the transformation which the salts of potass in combination with the organic acids have undergone. Lastly, there are certain pathological conditions which lead us to vary the amount of alkalis given. Who does not know that in gout, gravel, and especially diabetes, immense quantities of these may be given? and who is not aware that certain Summer and Autumn putrid affections will not tolerate them?

Clinical observation has long shown that the various alkalis may replace each other in practice. This is the case with carbonite of lime and of magnesia; as also with the compounds of soda and potass. It has been erroneously stated that soda is more favourable to the animal constitution than potass. Analysis of the animal liquids shows that the potass compounds are equally prevalent with the soda, and as regards the herbivora, far more so. But, although we can in general by the aid of any alkaline preparation induce an identical medical result, we believe it is better to give the preference (as mere antacids) to those alkaline compounds which present the advantage of having always an uniform chemical composition, and of producing little or no therapeutical effect; and in these respects the hydrated calcined magnesia and the bicarbonate of soda seem to hold the first rank.—*L'Union Medicale*, Nos. 1 and 4.

2. *Alum Gargles.*—We have shown elsewhere that the true *astringents* belong to the class of *coagulants*, that is to say, the class of chemical agents capable of

entering into chemical combinations with the albuminous elements of the blood, and forming with them an insoluble compound. In applying this principle to *alum*, we showed how this substance, penetrating into our tissues, is first decomposed by the alkalis of the blood, so as to form an insoluble sub-salt, which is deposited in the organic tissues, filling their network, and, so to speak, tanning them. We pointed out how a new portion of alum, being no longer modified by the alkalis already saturated, then acts, by fluidifying the albumen, in stimulating exhalation; and how, lastly, this *alum-albuminous fluid* taken into the circulation, again becomes solid when it finds itself in presence of all the alkalis contained in the mass of the blood—and in this way we explained the agency of large doses of alum in arresting hæmorrhage. Thus, in a small quantity it is a very precious local astringent; in a larger quantity it becomes an energetic local fluidifyer; and, after absorption, a geueral hæmostatic of undoubted efficacy.

We omitted to state why we thought this substance should not be combined with *mel rosæ*, which it so commonly is. It always contains a marked quantity of the proto-sulphate of iron by reaction on which the tannin of the mel rosæ produces a greenish precipitate. This, besides being disagreeable in appearance, leads the patient to the belief that the properties of the medicine have become deteriorated. Preferable combinations are the following:—

1st. *Astringent Gargarism.*

Alum	½ a part.
Distilled water . . .	150 parts.
Syrup of mulberries }	of each 25 parts.
——— Poppies, }	

For aphthous affections, mercurial stomatitis, and generally in all the diseases of the throat in which astringents are indicated.

2. *Detersive Gargarism.*

Alum	20 parts.
Distilled water . . .	100 parts.
Syrup of mulberries }	of each 30 parts.
——— Poppies }	

In hoarseness, in aphonia, and in those affections of the pharynx characterised by great dryness, and in which it is desired to excite the excretion of the mucosites. It is in this same dose—that is its fluidifying dose—that alum should be given for the prevention and cure of pharyngeal diphtheritis.

Most practitioners, I may remark, administer alum in too small a proportion for the removal of acute hoarseness, and especially in the aphonia of singers. Theory and practice alike show us that these cases require strong gargles.—*Mialhe, l' Union Medicale*, No 34.

3. *Astringent Collyria.*—Many practitioners are in the habit of prescribing *mucilages* of gum, psyllium, and especially of quince, with the different medicinal agents which constitute the base of their astringent collyria. It is a bad practice, since all true astringents necessarily belong to the class of bodies which coagulate the serum of the blood, and all substances which coagulate albumen also coagulate gum and the liquids which contain it; whence it results that the addition of mucilage to a salt of alum, zinc, copper, lead, silver, &c., necessarily gives rise to more or less of an entirely insoluble precipitate, which can nowise advantageously act upon the mucous membrane of the eye. Such a combination is worthy of the period which gave rise to it. It belongs to that epoch in therapeutics in which practitioners were persuaded that all agents capable

modifying the living economy were endowed with absolute curative properties having nothing in common with the action they exercised on our organs, an action which they considered as generally hurtful and never useful. A fundamental error which time and experience have happilyd one justice to!—*Mialhe, l'Union Medicale*, No. 40.

4. *Sedative Cataplasm in Puerperal Arthritis.*—The following is the means of treatment adopted by M. Trousseau in that special form of articular phlegmasia which is frequently observed during the puerperal state at the Hôpital Necker—a means whence he declares he has derived the greatest advantage. It consists in the application of a cataplasm formed as follows:—As much bread as is required is boiled in camphorated brandy, and upon the cataplasm so formed a layer of camphor, consisting of about two or three drachms, is powdered over the poultice, and the whole moistened with a solution of the like quantity of the extract of belladonna. It is to remain on for five or six days, and then to be renewed. Generally, on the very first night, the pain lessens, and after some days completely disappear. The resolution of the swelling is hastened, though much less rapidly.—*Bulletin de Therapeutique*, Feb.

5. *Disguise of the Bitterness of Medicines by means of Coffee.*—Medical men are perhaps scarcely sufficiently alive to the desirableness of masking the nauseousness of the abominable compounds they are forced to meddle with. It is not always desirable to do so, medicines of the anti-spasmodic and anti-hysteric class owing a portion of their efficacy to their nastiness; while, again, it certainly is questionable whether the bitter taste of many medicines can be removed without impairing the value of the principle upon which this depends. However this may be, the statement of a M. *De Vouves*, a medical student at Paris, that the *bitter taste of Quinine may be completely masked by Coffee*, has excited considerable attention. M. *Dorvault*, Pharmacien, in a communication to the Union Medicale, furnishes the following formula, as, after repeated trials, being found to be the best for securing this object. Take of ground fresh-roasted coffee 10 parts, boiling water 100 parts. Treat it by displacement, strain and add Sulphate of Quinine 1 part, Sugar 15 parts—these two last having been previously well mixed together. The mixture must be well shaken when administered. For children milk may be added. He sum sup his paper with these conclusions. 1. A solution of coffee annihilates completely, instantaneously, within wide limits, the bitterness of Quinine. 2. The disappearance of this taste is due in part to the transformation of the dissolved portion of the salt into a sort of tannate, and in part to other principles of the coffee. 3. Of all tanniferous substances coffee is most apt for this effect. 4. The therapeutical action of the medicine does not seem to be diminished.—*L'Union Medicale*, No. 32.

In another number of the same Journal, a M. *Combes*, a student in pharmacy, communicates the successful issue of a long series of trials upon the power *coffee possesses in masking the bitter taste of sulphate of magnesia*—a taste far more nauseous to most persons than is that of quinine. The following is his formula for an ordinary dose of about an ounce of the salt.

Sulphate of Magnesia . . .	30 parts.
Ground Coffee	10 "
Water	700 or 800 "

Boil them briskly together for two minutes in an untinned vessel. Remove from the fire, and having allowed the mixture to infuse for a few minutes, strain it. Sugar it and drink it hot or cold, according to taste. To ensure the effect, the coffee must be boiled with the salt as directed above; adding the latter to it afterwards or to an infusion does not suffice. If the quantity of the sulphate be much increased, and it is yet desired not to add more coffee than the above,

that will suffice if, while the fluid is boiling, a grain or two of tannin be added.—*L'Union Medicale*, No. 95.

The Bulletin de Therapeutique signalises another use of *coffee* in *disguising the taste of Purgatives for Children;* MM. Guersant and Blache frequently employing it for this purpose. A weak decoction of coffee is made, to which some milk and sugar are added, care having been taken while boiling the coffee, to put in a *few follicles of senna.* If it is given to the children with a little bread, they will generally take it with avidity. This medicine generally acts freely upon children, and thus administered does not induce the violent griping it sometimes does in the adult. (As a matter of taste, we think the senna tea and prunes of our grandmothers is a more delicious preparation.)

6. *The Employment of Tonics in Acute Disease.*—M. Legroux, in a recent lecture at the Hôpital Beaujon, adverted to his successful treatment of some examples of acute disease by means of Tonics. He observed that "in order properly to avail ourselves of the indications furnished by diseases, we should take into account their *foundation* (*fond*) and *form*, their *mode* and their *nature.* The *form* is the disease itself, which offers its general indications; the *foundation* is the idiosyncracy of the individual, which may modify and completely change the indication. The *mode* indicates the description of morbid process, but nowise its etiological nature, which in its turn furnishes new indications.

"It is especially in relation to *tonics* that the indications and contra-indications springing up between the relations or opposition which exist between the *foundation* and the *form* should be studied. And first, it is important to distinguish *true* from *apparent adynamia.* Prostration of strength with a good coloration of the skin, and sometimes with cyanic congestion of the face, occurring in a well-fed subject, having a small sub-inflammable pulse, but with a notable and persistent elevation of temperature, do not represent *true* adynamia. But if the patient is enfeebled by age, bad regimen, prior diseases, spontaneous or provoked deperditions; if he yields under an attempted antiphlogistic treatment; if the skin is cold; if the pulse in spite of its frequency is small and 'subinflammable;' if there are cold sweats, and on the assuming the erect posture a tendency to syncope, then the adynamia is real. In that case, whether the disease be a phlegmasia internal or external, an erysipelas, a pneumonia, or what not, we must abandon the form for the fundamental; and broths, wine, or quinine sometimes in such cases perform wonders. The tonic medication becomes antiphlogistic. It is especially indicated in persons habituated to the immoderate use of spirituous drinks, and whose idiosyncracy ill accommodates itself to active antiphlogistic measures.

The action of tonics under these circumstances may be easily understood if it is remembered that the capillary circulation in inflammations requires a certain degree of activity to effect the resolution of engorgements; and that a stimulant of a different character to that which has induced the congestion, and capable even itself of inducing the like in healthy tissues, will cause the disappearance of this. At the surface of the body we frequently find ourselves obliged to animate by topical applications languid inflammations; while the internal therapeutics opposed to the majority of the chronic inflammations of the lymphatic system are composed of stimulating medicinal substances.

One word as to rheumatism in particular. Suppose we have to do with a patient enfeebled by the duration of the disease, or by great deperditions. He may be pale, anæmic, wasted, tortured by cruel sufferings, his limbs rendered stiff by prolonged congestions, admitting of no movements, and his digestive organs being the seat of more or less acute pains, the stomach seeming to revolt at the slightest aliment. In these cases, again, the foundation is to be attended to before the form. We must restore the constitutional powers by ferruginous

preparations and by substantial food, which will be perfectly well tolerated by stomachs that reject all insipid or non-azotized substances.

After adverting to the use of tonics in typhoid fever, Dr. Legroux concludes:—

"It is one of the most serious errors to '*dichotomize*' medical substances according to their properties; for their action is almost always complex. Stimulant and tonic at a slight or moderate dose, they may become great debilitants, or fatally hyposthenisant in larger doses. *Wine* is certainly the most powerful hyposthenisant we have; but in large quantities it debilitates and kills. In therapeutical doses *arsenic* is a tonic; but kills by hyposthenisation in a larger one. *Quinine* renders us high services as a tonic, but it is a hyposthenisant in large doses. The *tonic* medication, then, cannot be regarded as simple in its effects. If it raises the fallen strength of a patient, so, on the other hand, may it moderate the febrile element, which is not incompatible with true adynamia."—*L'Union Medicale*, No. 86.

M. Cazenave on Papular Diseases of the Skin.

The skin is a compound of very distinct organs, of the apparatus of different functions, some being entirely independent of each other, and others more or less connected in their physiological actions. Hence we might naturally be led to expect that each of these organs or apparatus would have its special lesion, giving rise to a special train of symptoms. The determination of *such elementary lesion* is by no means a matter of mere curiosity. The knowledge of the *seat* may lead, through the conditions of physiological pathology, and especially by the sympathies it discloses, to the demonstration of its *cause*, the appreciation of its *nature*, and the choice of its *treatment*.

If there is a form of disease of the skin in which the conditions of seat are in harmony with pathological observation, in which we can render an exact account of the phenomena observed by the anatomical composition of the affected tissue, it is beyond all doubt the *papular form*. Two very distinct characteristics distinguish eruptions of this class: the presence of small, full, solid, projecting elevations; and a more or less severe, but constant pruritus. The elevation is nothing else than the anormal development of the papillæ itself; a development which is easily appreciable from that permanent condition of "goose-skin" observed at the inner portion of the limbs of some persons, to the large papulæ of prurigo. If we bear in mind the nervous and vascular conformation of the papillæ, we easily explain the various phenomena. Of these the *pruritus* is the dominant one, and it is a purely *nervous symptom*. In most cases the papillæ preserve the colour of the skin, and the vascular net-work takes no other part in the disease than as a slight congestion consequent on the hypertrophy of the papilla. In some forms indeed, as *prurigo vulvæ*, &c., the minutest examination fails to detect any enlargement whatever. Under other circumstances, the papillæ put on all the characters of a true *inflammation*; and in others the nervous and vascular elements may be found conjoined, as in *lichen agrius*. When the papular disease becomes chronic, and the papules become hard, dry, and parchment-like—sometimes preserving a remarkable size, and no less remarkable thickness—the *epidermic* element is especially involved.

In examining into the intimate nature of these diseases, we remark their great affinity to other *nervous* diseases, with some of which they sometimes alternate. It is usually in women and sensitive individuals, in persons of a fine delicate skin, and at the periods of life when the nervous system is in highest activity, that these diseases are most frequent; and it is rare not to find them preceded or accompanied by other nervous disturbances, such as gastralgia, neuralgia, or uterine disorders.

Moreover, the accessions of pruritus sometimes assume a periodicity only met with in the neuroses.

The papular eruptions present themselves under two distinct forms. In the one the papilla is often notably developed, but without any true inflammation; the nervous element and the epidermis of the papilla being alone interested. This is *prurigo*, an affection characterized by more or less intense pruritus, and by papulæ of different size, without change of colour of the skin, isolated, distinct, and accidentally tipped with little black crusts of coagulated blood; and when the disease has existed for a long time, by great thickening and condensation of the altered structure of the skin. In the other there may be less swelling than in the former, but there are also redness, and new products of inflammation, the whole elements of the papillæ being now affected. This is *lichen*, a disease often characterized by intense pruritus, accompanied by solid, full papulæ, generally very small, agglomerated, and sometimes confluent. They are generally red, and may go on to ulceration, the secretion of a sero-purulent fluid, scales, and even crusts, and later to a thickening of the skin, which takes on a peculiar yellowish tinge. In point of treatment the distinction of the two forms is not so important as might be supposed. All depends upon the morbid predominance of this or that element. If the exaggeration of the sensibility is very predominant, anti-spasmodics are given, hygienic precautions adhered to, and especially a non-stimulant regimen ordered. M. Cazenave has tried a great quantity of substances, but has found most benefit from the use of *Extract of Aconite*, (1 to 2 grs. per diem,) which, together with a few baths, has frequently enabled him to triumph over even general papular eruptions. *Topical applications* are seldom required, save when the epidermis has become hypertrophied. In the *acute stage* of papular eruption, which is represented by the varieties of *lichen*, the application of topics, and especially of ointments, is generally hurtful. M. Cazenave has seen, under the use of the simplest ointments, simple lichen become violently inflamed and go on to ulcers. In the *chronic* state, however, constituted by *prurigo*, ointments are usually well borne, and advantageously modify the disease in most cases, although they do not procure its cessation. When there is distressing *pruritus* topical applications are essential, those of a narcotic character being often useful in irritable subjects. In other cases tar-water, alkaline lotions (subcarb. potass. 8 to 12 parts to 500 of water), or sulphuretted lotions (sulphuret of potass. 4 p. water 300), are most so.

In chronic *eczema*, and especially in *pruritus without eruption*, M. Cazenave has found the formula recommended by Biett (Cyanuret of Potass. gr. x., Emulsion of Bitter Almonds, ℥ vj.), frequently very useful. In spite of all means, the itching often remains with provoking obstinacy. It may be allayed sometimes by camphor (Camphor 30 p., Lettuce water 500) when it is very intense. But of all topics *mercurial* lotions usually best counteract this. In the dreadful suffering arising from *pruritus vulvæ*, a little of the following may be employed. Bichlor. Hydr. gr. iv., Spt. Menthe ℥ ss., Aq. Dest. ℥ vj.—*L'Union Medicale*, No. 98.

Treatment of Obstinate Hiccough by Prolonged Compression of the Epigastrium.

Dr. Boyer relates three cases of prolonged and alarming hiccough, which, having resisted all the usual means employed for its relief, were relieved by the application of pressure, a practice first suggested by Bordeu and since revived by M. Rostan. A large pad is laid on the epigastrium, and bound forcibly on by means of a towel or bandage. It generally causes instant relief, but if discontinued too soon the hiccough returns. It is usually necessary to wear it for twenty-four hours, before it can be safely removed.—*Revue Medico-Chirurgicale*, July.

Account of a Physical Sign of Pneumonia of the Apex of the Lung. By Wm. Boling, M.D.

The experience of Dr. Boling is confirmatory of the opinion that, when pneumonia commences at the apex of the lung, it is especially fatal; and his object in the present communication is to indicate a physical sign which may lead to its earlier diagnosis. "This is a fine mucous or crepitant ronchus, *seemingly seated in the larynx*, loud enough to be heard distinctly at the distance of two or three feet from the patient, and so *persistent*, that it is not removable, or but momentarily, by any effort to expectorate which the patient may make, while at the same time there are present none of the signs of bronchitis or laryngitis." Though seeming to the by-stander to arise from mucus in the larynx, the indifference manifested by the patient proves this is not the case; and on applying the stethoscope just above or below the clavicles it will be found to proceed from the apex of the inflamed lung. "It would seem that the sound there produced in the pulmonary vesicles must be conveyed by the larger bronchial ramifications, numerous and superficial at this point, to the larynx, where, in consequence of the thinness of the tube, or rather the thinness of its covering, and its proximity to the surface, the deceptive impression of its production in this organ, from the presence of a small quantity of viscid mucus, is created. It is the indifference of the patient to the presence of the sound, but still more especially, its *persistence*, which constitutes its peculiar and distinctive feature, and upon which its value as an evidence of pneumonia commencing in the apex of the lung depends."—*American Journal Med. Sciences*, July, 1847.

New Modes of Effecting Union of Wounds.

M. *Amussat* has of late procured the union of large wounds by the first intention by means of the following suture. He passes several very fine steel sewing needles through the cutaneous edges of the wound, and having twisted a waxen thread around them, breaks off their extremities by means of a forceps, and leaves them to fall out of themselves, which they do in a few days, just as ligatures of vessels are allowed to do.—*Gazette des Hopitaux*, No. 69.

M. *Baudens* employs the following means for bringing together the edges of wounds. Speaking of that resulting from an amputation for example, he directs a circular bandage to be placed above the stump, and two strong pins fixed into this, one before and one behind, in such a manner as to leave their heads and points exposed. A double point of support is thus got, around which strong cotton threads are passed; these are crossed over each other towards the face of the stump in such a manner as to draw the integuments together with any desired force, after the manner of a uniting bandage, terminating with a figure of eight, just as in the operation for hare-lip.—*Comptes Rendus*, T. 24, p. 1018.

Chronic Cutaneous Eruptions.

M. Cazenave recommends the following formula as of excellent service in chronic dartrous eruptions, as impetigo, eczema, lupus, and all diseases of the skin allied to the lymphatic and scrofulous constitutions.

Crystallized Chloride of Lime . . . . 15 parts.
Distilled Water 500

A tablespoonful three times daily in some bitter infusion.

ADDRESS TO THE READER.

The Readers of the Medico-Chirurgical Review will perceive, by the Prospectus which accompanies the present Number, that it has been determined to incorporate with it, in the future publication, The British and Foreign Medical Review, hitherto conducted by Dr. Forbes. By this arrangement the Proprietors have the gratification of being able to state, that the services of the most valued Contributors to both of these Reviews will be combined—and that there is every reason to believe that the result will be the production of a work second to none in Europe for the soundness, depth, and variety of its Medical and Chirurgical investigations.

London, *September* 30, 1847.

BIBLIOGRAPHICAL RECORD.

1. The Retrospect of Medicine; being a Half-yearly Journal, containing a Retrospective View of every Discovery and Practical Improvement in the Medical Science. Edited by *W. Braithwaite*. Part 15. Jan.—June, 1847. 8vo, pp. 483. London.

2. The Half-yearly Abstract of the Medical Sciences; being a Practical and Analytical Digest of the Contents of the Principal British and Continental Medical Works published in the preceding Six Months. Edited by *W. H. Ranking*, M.D. Vol. V. Jan.—June, 1847. 8vo, pp. 424. London.

3. The Medical Examiner and Record of Medical Science. Edited by *Robert M. Huston*, M.D. No. 29—30, May and June, 1847. Philadelphia.

4. An Experimental Inquiry into the Functions of the Great Sympathetic Nerve. By *C. Radclyffe Hall*, M.D. Part I. 8vo, pp. 126. Plates. London, 1847.

5. A Guide to the Use of the Buxton Waters. By *William Henry Robertson*, M.D. Fourth Edition, revised. Foolscap 8vo, pp. 32. London, 1847.

6. A Copy of Reports on Sir *William Burnett's* Disinfecting Fluid. Ordered by the House of Commons to be printed, 20th July, 1847. Pp. 14.

7. Cholera, Dysentery, and Fever, pathologically and Practically considered; or the Nature, Causes, Connexion, and Treatment of these Diseases in all their forms. By *Charles Searle*, M.D. 8vo, pp. 140. London, 1847.

8. A Treatise on Diet and Regimen. By *William H. Robertson*, M.D. Fourth Edition, Part III., 8vo. London, 1847.

9. Twenty-seventh Annual Report of the Directors of the Dundee Royal Asylum for Lunatics, submitted, in Terms of their Charter, to a General Meeting of the Directors, 21st June, 1847. With the Report of the Medical Officers. 8vo, pp. 62. Dundee, 1847.

10. Seventeenth Annual Report of the Belfast District Asylum for the Insane Poor of the Counties of Antrim and Down, and of the Town of Carrickfergus. For the Year ending 31st March, 1847. Drawn up by the Resident Physician. 8vo, pp. 43. Belfast.

11. A Letter to Benjamin Rotch, Esq., Chairman of the Committee of Visitors; on the Plan and Government of the Additional Lunatic Asylum for the County of Middlesex, about to be erected at Colney Hatch. By *John Conolly*, M.D. 8vo, pp. 27. London, 1847.

12. The Consciousness of Right and Wrong a just Test of the Plea of partial Insanity in Criminal Cases. Illustrated by the Case of William Stalker, indicted at the Cumberland Lent Assizes, 1847, for the wilful Murder of his Wife. By *C. Lockhart Robinson*, M.D. 8vo, pp. 18. Edinburgh, 1847.

13. Proceedings of the National Medical Conventions, held in New-York, May, 1846, and in Philadelphia, May, 1847. 8vo, pp. 175. Philadelphia, 1847.

14. The American Journal of the Medical Sciences. Edited by *Isaac Hays*, M.D. 8vo, pp. 284. July, 1847. Philadelphia.

15. The Human Brain; its Structure, Physiology, and Diseases, with a Description of the Typical Forms of Brain in the Animal Kingdom. By *Samuel Solly*. Second Edition. 8vo, pp. 684. London, 1847.

16. On the Causes and Treatment of Abortion and Sterility; being the Result of an extended Practical Inquiry into the Physiological and Morbid Conditions of the Uterus, with reference especially to Leucorrhœal Affections, and the Diseases of Menstruation. By *James Whitehead*, F.R.C.S. 8vo, pp. 498. London, 1847.

17. The Microscopic Anatomy of the Human Body in Health and Disease. Illustrated with numerous Drawings in Colour. By *Arthur Hill Hassall*. Parts 10 and 11. London, 1847.

18. An Account of a Simple Means of Moderating the Effects of Fire upon the Human Body. By Mr. *F. A. Bulley*, F.R.C.S., Surgeon to the Royal Berkshire Hospital, Reading. [Reprinted from the Medical Times.]

This paper gives an account of the benefit its author has derived from the employment of Treacle diluted with three parts of water, at a temp. of 98°, as an application to Burns of various degrees of intensity. Lint soaked in the mixture is to be kept constantly applied to the part, renewing morning and evening, and moistening at intervals.

19. On the Use of Nitrate of Silver in the Cure of Erysipelas. By *John Higginbottom*, F.R.C.S.E. Nottingham. (Read before the Provincial Medical and Surgical Association.)

[The Profession is much indebted to Mr. Higginbottom for having many years since called its attention to the valuable properties of the Nitrate of Silver, and especially to its power in arresting the progress of Erysipelas. In the present communication he recommends the application at a far earlier period of the disease than he formerly deemed advisable, such mode of using it, with attention to the digestive organs, often rapidly cutting short its progress. He employs the following solution:—Arg. Nitr. four scruples, Nitric Acid six drops, Distilled Water four drachms, previously well washing the part, first with soap and water, and then with pure water. So used, it is especially beneficial in erysipelas, threatening to spread over the scalp, to which it may be freely applied without inducing vesication.]

20. The Preservation of Infants in Delivery. Being an Exposition of the Chief Cause of Mortality of Still-born Children. By *Richard King*, M.D., M.R.C.S. 8vo, pp, 60. London, 1847.

21. The Chemistry of Vegetable and Animal Physiology. By Dr. *G. J. Mulder*. Translated from the Dutch, by Dr. *P. F. H. Fromberg*. With an Introduction and Notes, by *J. F. W. Johnstone*, F.R.S.L. & E. Part III. 8vo, pp. 267. Eight Coloured Lithographs. Edinburgh, 1847.

22. Contributions to the Pathology and Treatment of the Scorbutus, which is at present prevalent in various parts of Scotland. By *Charles Ritchie*, M.D. (From the Monthly Journal of Medical Science.)

[Dr. Ritchie has here drawn up a very excellent account of this formidable disease as it has prevailed in Scotland—in common with entire Europe—during the late Spring. For reasons stated in our Periscope, we cannot, however, agree with him in referring its production exclusively to defective diet, exposure to severe weather, and the like; and we are among those whom he speaks of as "turning away from what is known and tangible, to seek the causes of the epidemic in the unknown and impalpable obscurities of an aerial constitution."]

23. Anecdota Sydenhamiana: Medical Notes and Observations. By *Thomas Sydenham*: hitherto unpublished. Second Edition, pp. 80. Oxford, 1847.

[We are glad to observe, that these "Notes, &c.," of a cotemporary and acquaintance of Sydenham himself, have reached a second edition. Our readers are probably aware that the profession is indebted to Dr. Greenhill, of Oxford, for the possession of this little work. As a matter of course, all the members of the Sydenham Society should have a copy of it.]

24. On the Duties of Physicians, resulting from the Physician. By the late Rev. *Thomas Gisborne*, M.A. Pp. 56. Oxford, 1847.

[A reprint of a portion of the author's well-known and useful work, "Enquiry into the Duties of Men in the higher and middle classes of Society in Great Britain, resulting from their respective Stations, Professions and Employments." 6th Edit., 1811. We all stand in need of being reminded of our duties towards those who commit their health, and often their happiness too, to our keeping. To those beginning the practice of the medical profession, much of the advice in these pages may be truly useful.]

25. A few Remarks on the Expectant Treatment of Diseases. By ΑΚΕΣΤΗΣ. Pp. 16.

[These remarks are intended as a defence of "legitimate medicine," and in answer to certain recent heresies in a cotemporary Journal, on the subject of what has been called the "natural method" of treating diseases. We cannot congratulate the author on the success of his attempt. Much of what he says is altogether beside his argument, and the tendency of some of his observations is altogether very objectionable.]

26. Unhealthiness of London, and the Necessity of Remedial Measures. By *Hector Gavin*, M.D., F.R.C.S.E. Pp. 70. London, 1847.

[Contains much useful information respecting the state of health in the Metropolis, the fearful amount of disease that might be prevented, and the simple and efficient means of attaining this most desirable object. The lecture is exceedingly well adapted for a popular audience.]

27. Consumption of the Lungs and Asthma, arrested and cured, in the Majority of Cases, by Inhalation and other Rational Means. By *Daniel Carr*, M.D. 12mo, pp. 200. London, 1847.

[A work that is altogether discreditable to the writer, whose address is Birmingham, if he be a regularly-educated member of the profession. What shall we say of an M.D., who appends "a series of questions on Consumption and Asthma, which will enable patients, who are desirous of consulting a physician, to state their symptoms clearly, either personally or by letter?" Need we say more respecting the style and purport of the book?]

28. Theorie des Neuro-viscerites ou Fievres Primitives. Par *Ant. Hugon*. 8vo, pp. 111. Paris, 1847.

The author is a decided anti-Broussaist, and an energetic advocate of the essentiality of fevers. His views are generally sound and practical, and we have derived much pleasure from the perusal.

29. Gazette Medicale, July to September.
In exchange.

30. L'Union Medicale.
In exchange.

31. Dublin Medical Review.
In exchange.

32. Edinburgh Medical and Surgical Journal for July.
In exchange.

33. British and Foreign Medical Review for July.
In exchange.

34. Edinburgh Monthly Journal of Medical Science, for July, August, and September.
In exchange.

CONTENTS

OF THE

MEDICO-CHIRURGICAL REVIEW,

OCTOBER 1, 1847.

Reviews.

I.

The Human Brain; its Structure, Physiology, and Diseases. By Samuel Solly, F.R.S., &c. 285
1. Comparative Anatomy of the Encephalon 287
2. True Mode of Studying the Brain 289
3. Relations of the Cerebellum and Spinal Cord . . . 290
4. Anatomical Conclusions 293
5. Seat of Sensation and Volition 295
6. On Anæmic Coma 297
7. Principles of treating Insanity 299
8. Pathology of Epilepsy 300

II.

Ricerche ed Esperienze sul Sangue Umano. Del Dottore Gio Polli . 303
1. Researches and Experiments upon the Formation of the Buffy Coat of the Blood, &c. 303
2. On a New Criterion for the Regulation of Bloodletting . 310
3. On the Condition of the Fibrine of the Blood in Inflammation . 314
4. On the Effects of the Abstraction of Blood upon the Human Organism 319
5. Modifications of the Blood by Bleeding 321
6. Practical Applications 323

III.

The Physiological Anatomy and Physiology of Man. Part the Third. By Robert Bentley Todd, M.D. F.R.S., &c. and William Bowman, F.R.S. 326
1. On Sensation and Perception 327
2. Structure of the Cornea, Iris, &c. 328
3. Structure of the Retina, of the Cochlea, &c. . . . 330

IV.

I. The American Journal of Insanity . . . . . }
II. Fourth Annual Report of the Managers of the State Lunatic Asylum of New York . . . . . } 333
III. Remarques sur quelques Etablissemens d'Alienés de la Belgique, de la Hollande, et de l'Angleterre. Par M. Brierre de Boismont . }
1. Shakespeare's Delineations of Insanity 333
2. Medical Treatment of Insanity at the New York State Lunatic Asylum 339
3. Frequency of Pulse of the Insane—Size and Shape of the Head 340
4. Weight of the Insane—Hereditary Predisposition . . 341
5. Suicidal Form of Insanity—Homicidal Insanity . . 342
6. Plea of Insanity 344
7. On Establishments for the Insane in Belgium, Holland, and England 346
8. Objections to the Non-Restraint System. 349

V.

I. On Pulmonary Consumption, and on Bronchial and Laryngeal Disease, &c. By Sir Charles Scudamore, M.D., F.R.S., &c. . . .
II. Cold and Consumption. By Henry C. Deshon, M.D., &c. . 351
1. Treatment with the Inhalation of Iodine 353
2. Great Increase of the Animal Heat in Phthisis 355
3. Increase of the Carbonic Acid during Respiration . . . 357
4. Effects of Inhaling the Vapour of Water 359

VI.

A System of Surgery. By J. M. Chelius 360
1. Elementary Proceedings of Surgical Operations . . . 361
2. Treatment after Operations 363
3. General Treatment of Lithotomy Patients 364
4. Comparison of Lithotomy and Lithotrity 366
5. Radical Cure of Hydrocele 369
6. Question of Removing a Scirrhous Breast 370
7. Application of Blisters 371
8. Use of the Saw in Amputation 372
9. Results of Amputation—Circumcision in Phymosis . . . 373
10. Reduction of Paraphymosis 374
11. Production and Treatment of Curvatures 375

VII.

On the Causes and Treatment of Abortion and Sterility. By James Whitehead, F.R.C.S., &c. 377
1. Physical Properties of the Menstrual Fluid 378
2. Spurious Menstruation 380
3. Conditions influencing Menstruation 382
4. Dysmenorrhœa—Menorrhagia 384
5. Last Menstrual Crisis 386
6. Uterine Diseases liable to occur at the Crisis 387
7. Signs of Pregnancy 390
8. Menstruation during Pregnancy 393
9. Abortion; its Statistics and Causes 395
10. Leucorrhœa—Endo-uteritis 397
11. Gonorrhœal Inflammation of the Uterus 398
12. Sterility—Illustrative Cases 400

VIII.

I. Researches on the Chemistry of the Food. By Justus Liebig .
II. The Chemistry of Vegetable and Animal Physiology. By Dr. G. J. Mulder 405
1. Insufficiency of Ultimate Analysis 407
2. Composition of the Juices of Flesh 409
3. Rationale of Cooking Processes 410
4. Chemico-Histology 413

IX.

Further Report of the Commissioners in Lunacy to the Lord Chancellor . 415
1. Improved Condition of the Insane 417
2. Imprisonment of a Lunatic for Debt—Lincoln Asylum . . 419
3. Treatment of Insanity in the English Asylums . . 421

X.

Théorie Expérimentale de la Formation des Os. Par P. Flourens . . 42
1. Powers of the Periosteum in forming Bone . . . 42
2. Powers of the Endosteum 42

XI.

Report on the Climate and principal Diseases of the African Station. By Alexander Bryson, M.D., &c. 43

1. Topography of Sierra Leone 432
2. Yellow Fever on board the "Bann," &c. in 1823 . . 434
3. Yellow Fever on board the "Eden" in 1829 . . . 436
4. Yellow Fever on board the "Sybille" in 1829 . . 439
5. Medical History of Fernando Po 440
6. Yellow Fever on board several ships in 1838 . . . 442
7. Epidemic Diarrhœa a Precursor of Yellow Fever . . 445
8. Yellow Fever on board the "Eclair" . . . 446
9. Dr. Bryson's Views respecting the Nature of Yellow Fever . 449
10. Remarks on the Treatment of Yellow Fever . . 451

XII.

A Treatise on the Physical Cause of the Death of Christ, and its relation to the Principles and Practice of Christianity. By WILLIAM STROUD, M.D. . 453
1. Influence of Strong Emotions on the Heart, &c. . . 455
2. Instances of Sanguineous Perspiration 457
3. Rupture of the Heart from Intense Emotion . . 458
4. Separation of Effused Blood into Clot and Serum . . 461
5. Effusion of Blood into the Pericardium . . . 463
6. Punishment of Crucifixion Described 465
7. Crucifixion a Lingering Mode of Death . . . 466
8. Suddenness of our Saviour's Dissolution . . . 468
9. Physical Cause of our Saviour's Death Explained . . 471

XIII.

Memoir sur la Rougeole des Adultes. Par le Docteur MICHEL LEVY . 477
1. Form and Progress of the Eruption . . . 477
2. Concomitants of the Disease 478
3. Complications of the Disease 479
4. Consecutive Maladies 480
5. Antagonism—Mortality 481
6. Treatment 482

XIV.

I. Copy of Reports and Communications on M. Ledoyen's Disinfecting Fluid
II. Copy of Reports on Sir William Burnett's Disinfecting Fluid } 483
1. Solutions of Nitrate of Lead and Chloride of Zinc . . 485
2. Their Antibromic and Antiseptic Properties . . . 487
3. Have they any Power over Infectious Miasms . . . 489
4. Results of Experiments in Dublin 492

XV.

Traité Theoretique et Pratique d'Auscultation Obstetricale. Par J. H. A. DEPAUL, D.M., &c. 495
1. The Uterine Souffle or Blowing Sound . . . 497
2. The Cardiac Pulsation of the Fœtus 500

XVI.

On the System of the Great Sympathetic Nerve. By C. RADCLYFFE HALL, M.D. 506

XVII.

Observations on the Treatment of Lateral Curvature of the Spine, &c. &c. By E. F. LONSDALE, F.R.C.S.E. &c. 510

XVIII.

Cholera, Dysentery, and Fever, pathologically and practically considered. By CHARLES SEARLE, M.D. &c. 515

XIX.

Remarks on Vivisection, and on Certain Allegations as to its Utility in the Study and Application of Physiology. By GEORGE MACILWAIN, F.R.C.S., &c. 518

XX.

Report on the Archetype and Homologies of the Vertebrate Skeleton. By Professor Owen, F.R.S., &c. 520

XXI.

The Microscopic Anatomy of the Human Body in Health and Disease. By Arthur Hill Hassall 522

XXII.

The Preservation of Infants in Delivery. By Richard King, M.D., M.R.C.S. 522

Periscope.

SELECTIONS FROM THE FOREIGN PERIODICALS.

1. On the Frequency of the Pulse and Respiration in the Aged. By C. W. Pennock, M.D. 523
2. On Scorbutus as observed in the Salpétrière in 1847, and on the Composition of the Blood in this Disease. By Dr. Fauvel 525
    1. Andral on the Blood in Scorbutus 527
    2. Analogy between Scorbutus and Typhus 529
3. Influence of Electricity in the Production of Diseases. By M. Pallas . 531
4. On the Diagnostic Signs furnished by Inspection of the Vulva and the Entrance of the Vagina. By M. Huguier 531
5. On Stricture of the Intestine in Hernia. By Louis Chapel . . . 533
6. Researches on the Normal and Anormal Capacity of the Cavities of the Heart. By Dr. Beau 534
7. Eschars over the Sacrum 537
8. Painful Crepitation of the Tendons. By M. Velpeau . . . 537
9. M. Royer-Collard on the Statistics of Insanity 538
10. M. Civiale, on the Statistics of Lithotrity and Lithotomy . . . 539
11. On the Induction of Cystitis and Albuminuria by Blisters . . . 540
12. On Abscess of the Breast. By M. Velpeau 543
13. Andral on the Intestinal Secretions in Cholera 544
14. Induction of Premature Labour in Miscarriages 545
15. Purulent Ophthalmia of New-born Infants 547
16. Serres on the Treatment of Typhoid Fever by the Black Sulphuret of Mercury 549
17. The Medical Profession in Paris 551
18. Observations on the Materia Medica 552
    1. Mialhe on Alkaline Medicines 552
    2. Alum Gargles 554
    3. Astringent Collyria 554
    4. Sedative Cataplasm in Puerperal Arthritis 555
    5. Disguise of the Bitterness of Medicines by Means of Coffee . 555
    6. The Employment of Tonics in Acute Disease . . . 556
19. Cazenave on Papular Diseases of the Skin 557
20. Prolonged Compression in Hiccough 558
21. Account of a Physical Sign of Pneumonia of the Apex of the Lung. By Wm. Boling, M.D. 559
22. New Modes of Effecting Union of Wounds 559
23. Chronic Cutaneous Eruptions 559
Address to the Reader 560
Bibliographical Record 561

INDEX.

Abdomen, wounds of, Guthrie on . 88
Abdomen, wounds of, sutures in . 91
Abdomen, effects of blows on the . 95
Abdomen, treatment of injuries of . 97
Abortion, habitual, Assafœtida in . 266
Abortion, habitual, induction of premature labour in . . . 545
Abortion and Sterility, Whitehead on 377
Abortion, statistics and causes of . 395
Accidental Productions 40
Acid, Hydrocyanic, Nunneley's experiments on 116
Acid, Hydrocyanic, *modus operandi* of 118
Acid, Hydrocyanic, treatment of poisoning by 120
Africa, Bryson's Report on Diseases of 431, 449
Aged, frequency of pulse in . . 523
Albuminuria in Pregnancy . . 277
Albuminuria induced by blisters . 540
Alkalis, Mialhe on 552
Alum, Mialhe on the action of . 554
Amputation, employment of the Saw in 372
Amputation, Results of . . . 373
Amygdalæ, extirpation of . . 51
Anæmia, cerebral 297
Analysis, insufficiency of ultimate . 407
Aneurism, liability of various arteries to 45
Aneurism, predisposing and exciting causes of 45
Aneurism, Bellingham on Compression in 46
Animals, Experiments on . . 518
Anus, Artificial 95
Aorta, Inflammation of the . . 43
Apoplexy, Grisolle on . . . 34
Apothecaries, Guy Patin on the . 175
Army-Surgeons, Douglas on the Claims of 245
Arsenic, Poisoning by 36
Arsenic, employment of, in diseases of the skin 247, 267
Arteries, deposits in the coats of . 44
Arteritis, Crisp on 43
Arteritis, Chronic diffused . . 257
Arthritis, Puerperal 555
Asylums, Lunatic, Conolly on . . 128
Asylums, Lunatic, improved state of 416
Auscultation, Depaul on Obstetrical . 495
Barber-Surgeons, Guy Patin on . 177
Bellingham on Compression in Aneurism 46
Bischoff on the Ovum 5
Bladder, Wounds of the . . . 96
Bladder, Inflammation of, from blisters 540
Blisters, Rules for the application of . 371
Blisters, Induction of Strangury by . 540
Blisters, Mialhe on the composition of 542
Blisters, effect of on the Blood . . 325
Blood, Polli's Researches on the . 303
Blood, formation of the buffy coat of . 303
Blood, density of the . . 306, 321
Blood, causes of the coagulation of the 308
Blood, Plethoric and Anæmic . 309
Blood, period of coagulation of the . 310
Blood, condition of, in inflammation . 314
Blood, coagulation within the body . 461
Blood, effects produced on the, by bleeding 319
Bloodletting, Guy Patin on . . 172
Bloodletting from the Jugular Vein . 252
Bloodletting, Tanchou on . . . 279
Bloodletting, Regulation of . . 310
Bloodletting, effects of, on the mass of the blood 319
Bloodletting, indications and contra-indications of 320, 323
Bloodletting, formation of dropsy after 325
Bloodletting, habitual . . . 325
Bloodletting, effect of excessive . 325
Blood-vessels, Crisp on the diseases of 42
Bone, Owen on the Structure of . 156
Bone, Owen's description of a . . 158
Bone, Flourens on the formation of . 426
Bowman's Physiological Anatomy . 326
Brain, Solly on the Human . . 285
Brain, mode of study of the . . 289
Brain, Physiology of the . . 293
Brain, Ramollissement of the . . 299
Brain, Nature of Congestion of the . 301
Breast, question of removing for cancer 370
Breast, Velpeau on Abscess of the . 543
Brookes on Inhalation of Ether . 246
Bryson, Report on Diseases of African Station 431
Burnett's Disinfecting Fluid . . 483
Burns, treatment of 256
Callus, formation of 430
Cancer, Question of operating for . 370
Cardialgia, Seymour on . . . 98
Catamenia, Nature of the . . . 379
Catamenia, age at appearance of . 382
Cells, Mulder on animal and vegetable 414
Cerebellum, Relation of, to the Spinal Cord 291
Chancre, Marchal on . . . 57
Chelius' System of Surgery . . 361
Chemistry, Animal, Liebig on the Study of 405
Chemists, Guy Patin on . . . 173
Children, bleeding from the jugular vein 252
Children, Trousseau on the dentition of 275
Children, Still Born 522
Cholera, Andral on the Intestinal Secretions in 544

holera, treatment of . . . 517
hrist, Physical Death of . 453, 468
iliary Muscle, Todd and Bowman on 330
Circumcision, Jewish mode of performance of 373
Cochlea, Stricture of the . . 331
Cod-Liver-oil, substitute for . . 282
Collyria, Mialhe on astringent . . 554
Coma, Anæmic, Solly on . . 297
Conception, Time and Seat of . . 8
Congelation, Shrimpton on . . 52
Conolly on Lunatic Asylums . . 128
Cookery, Liebig on the Rationale of 410
Copland on the Yellow Fever . . 183
Cornea, Structure of the . . 329
Corpus Callosum, formation of the . 293
Cranium, Owen on the homologies of 521
Crisp on the Blood-vessels . . 42
Crosse on Inversio Uteri . . 124
Croup, Alum emetics in . . 282
Crucifixion, nature and effects of the punishment of . . . 465
Curvatures, Chelius on . . . 375
Cysts, formation of . . . 38
Decidua, formation of the . . 12
Delivery, state of the Vulva after . 532
Dentition, Trousseau on . . . 275
Depaul, Auscultation, Obstetricale . 495
Deshon on Cold and Consumption . 357
Development, Owen on the laws of . 154
Disease in relation to Sex . . 250
Disinfecting Fluids, nature of . 483
Duncan on Medical Statistics . 250
Dysentery, Harty on . . . 69
Dysentery, Simple . . . 73
Dysentery combined with Fevers . 75
Dysentery, the, of ancient London . 83
Dysentery, infectiousness of . . 85
Electricity, influence of, in the production of disease . . . 531
Embryology, importance of the study 2
Encephalon, comparative anatomy of 287
Epydidymitis, Treatment of . 60
Epilepsy, Moreau on treatment of . 256
Epilepsy, Solly on Pathology of . 300
Epilepsy, Solly on treatment of . 302
Epilepsy Complicated with Insanity 424
Ether, Inhalation of . . . 246
Exostosis, Roux on . . . 262
Eye, foreign bodies, removal of, from 274
Eye, Sichel on preservation of . . 278
Eye, Irrigation in diseases of the . 548
Femur, Fracture of the neck of . 137
Fever, Typhoid, Grisolle on . 25
Fever, Typhoid, Serres on Æthiops mineral in 549
Fever, Typhus, Grisolle on . . 28
Fever, Eruptive, Grisolle on . . 30
Fever, Eruptive, Serres on . . 549
Fever, Intermittent, Grisolle on . 31
Fever, Malignant Remittent . . 193
Fever, the Yellow, Copland on . 183
Fever, Yellow, analogy to other fevers 190
Fever, the Yellow, remittence of . 199
Fever, the Yellow, question of a second attack 201
Fever, the Yellow, etiology of . 205
Fever, Yellow, epidemic visitation of 208
Fever, Yellow, influence of Sanitary Improvements on the . . 213
Fever, Yellow, at Boa Vista . . 219
Fever, Yellow, McWilliam on the . 218
Fever, Yellow, Quarantine and Preventive measures against . 233
Fever, Yellow, of Africa . 435, 449
Fever, Yellow, Diarrhœa a precursor of 445
Fever, Yellow, treatment of . . 451
Fever, the Eclair . . . 217, 447
Fibrine, conditions of, in the blood in inflammation 314
Fishes, Owen on the Skeleton of . 166
Flesh, Liebig on the juices of . . 407
Flourens on the formation of Bone . 426
Fœtus, Auscultation of the . . 501
Frost-bites, treatment of . . 52
Ganglia, Hall on Functions of the . 509
Gangrene, Dry, Crisp on . . 42
Gleet, treatment of . . . 55
Gonorrhœa, treatment of, by injections 53
Gout, Seymour on . . . 112
Gout, Skin disease in subjects of . 105
Gray's Supplement to Pharmacopæia 283
Gream on diet of children . . 251
Green on Mental Dynamics . . 235
Grisolle, Pathologie Interne . . 22
Guthrie on Wounds of the Abdomen 88
Hæmorrhages, Grisolle on . . 33
Hæmorrhages, Treatment by bleeding 325
Hall on the Sympathetic Nerve . 506
Harty on Dysentery . . . 69
Hassall's Microscopic Anatomy . 522
Heart, Influence of strong Emotions on the 455
Heart, Rupture of the . . . 459
Heart, Beau on the capacity of the cavities of the 534
Heart, Size of Cavities during arterial murmurs 535
Heart, Effects of Age on the size of the 536
Heart, Polypi of the 40
Heart, Abscess of the . . . 122
Hernia Humoralis, treatment of . 60
Hernia, Ventral 90
Hernia, Stricture of the intestine in . 53
Hip-joint, chronic rheumatism of . 14
Humerus, fracture of, near the shoulder 14
Hunt on Diseases of the Skin . 24
Hunterian Oration, Green's . . 2
Hydrocele, radical cure of . . 3
Hypochondriasis, Seymour on . . 1
Impregnation, Bischoff on . .
Infection, Sedillot on purulent . .

Inflammation, condition of the fibrine of the blood in . . 314, 317
Insanity, Seymour on opium in . 109
Insanity, Puerperal . . 111
Insanity, Prevention of . . 136
Insanity, Study of . . . 136
Insanity, Exercise and Recreations in 130
Insane, Employments of the . . 131
Insane, Superintendence of . . 132
Insanity, Religious Service in . 134
Insanity, Establishments for . . 135
Insanity in Reference to Phrenology . 296
Insanity, dependence of, on disease of the Brain 299
Insanity, Shakespeare's delineations of 333
Insanity, Brigham on Medical Treatment of 339
Insanity, State of the Pulse in . 340
Insanity, Shape of the Head in . 341
Insanity, Hereditary Predisposition to . . . 341, 538
Insanity, Education of the predisposed to 342
Insanity, Homicidal . . . 342
Insanity, Plea of . . . 344
Insanity, Establishment for at Gheel 346
Insanity, Brierre on the English Asylums for . . . 347
Insanity, Brierre on non-restraint treatment of . . . 349
Insanity, improved condition of the establishments for . . 417
Insanity not a protection from creditors 418
Insanity, Treatment of, at English Asylums 421
Insanity, combination with Epilepsy 424
Insanity, combination with General Paralysis 424
Insanity, moral treatment of . 425
Insanity, Statistics of Royer-Collard on the 538
Intention, Union by the First . 50
Intercostal Neuralgia and Neuritis . 259
Intestine, Wounds of the . . 92
Iodine, Inhalation of . . . 352
Iris, Structure and Connections of the 320
Iris, Hall on the motions of the . 508
Itch, Devergie on the . . 269
Jaw, Lower, the, dislocation of . 150
Jaw, Lower, Chronic Rheumatism of 151
Jugular vein, Bleeding from the . 252
King on Still-born Children . . 522
Kreatine and Kreatinine, Liebig on . 407
Labour, Premature, Induction of . 545
Lead, Anæsthesia from . . 282
Ledoyen's Disinfecting Fluid . 483
Leucorrhœa in relation to Abortion . 397
Levy on Measles in the Adult . 477
Liebig's Researches on the Chemistry of Food 405
Lithotomy, Treatment before and after 364
Lithotomy compared to Lithotrity 366, 539
Lithotomy, Statistics of . 368, 539
Lithotrity, Civiale on . . 539
Liver, Wounds of the . . 95
Lonsdale on Curvature of Spine . 570
Lunacy, Report of Commissioners of 415
Lunatics, improved condition of . 417
Lunatics, imprisonment of, for debt . 418
Macilwain on Vivisection . . 518
McWilliam's Report on Yellow Fever 218
Malaria, Nature of . . . 516
Malgaigne on Duties of a Surgeon . 254
Mania, Treatment of . . 421
Mania, Sutherland on the use of anodynes in . . . 422
Measles of Adult, Levy on the . 477
Meat, mode of boiling . . 410
Meat, effects of salting . . 412
Medicines disguised by Coffee . 555
Melancholia, Treatment of . 112, 423
Melancholia, connection with Phthisis 113
Menorrhagia, Whitehead on . 385
Menses, Properties and Source of . 379
Menstruation, Spurious . . 380
Menstruation, age of . . . 382
Menstruation, influence of factory labour on 383
Menstruation, period of terminating . 386
Menstruation, diseases succeeding . 387
Menstruation during pregnancy . 393
Microscope, importance of, in Morbid Anatomy . . . 393
Mouth, the, in man and animals . 249
Mulder, Chemistry of Vegetable and Animal Physiology . . 405
Myopia, Sichel on . . . 278.
Natural History, Green on . . 236
Nerve, Sympathetic, Swan on the . 237
Nerve, Sympathetic, Hall on the . 506
Nervous System, Bell's Researches on the 249
Nervous System, Solly on the . 294
Neuralgia, Intercostal . . 259
Novellis on Scorbutus . . 61
Operations, Surgical, Chelius . 361
Operations, Surgical, After-treatment of 362
Ophthalmia, Purulent of Infants . 547
Ovarian Cysts, teeth in . . 38
Ovarian Cysts, serous . . 39
Ovariotomy, Southam on . . 121
Ovum, Structure of the . . 3
Ovum, Bischoff's Researches on . 5
Ovum, Development of . . 9
Owen's Lectures on Comparative Anatomy 153
Owen on the Vertebrate Skeleton . 520
Paralysis, General, of the Insane . 424
Paraphymosis, Reduction of . 374
Patin, Guy, Lettres de . . 169
Perception, Nature of . . 327

Pericardium, Effusion of Blood into . 463
Periosteum, Agency in the formation of Bone 427
Pharmacopœia, Gray's Supplement to, 283
Phrenology illustrated by Insanity . 296
Phthisis in relation to Sex . . 250
Phthisis, Scudamore on Inhalation for, 351
Phthisis, Increase of Animal Heat . 355
Phthisis, Evolution of Carbonic Acid, 356
Phymosis, Circumcision in . . 373
Placenta, Structure of the . . 13
Placenta, Hunter . . . 17
Placenta, Vessels of the . . 18
Pneumonia of the Aged . . 281
Poisons, Action of, Grisolle on . 35
Poisons, limitation of the sale of . 115
Polli, Researches on the Blood . 303
Pompholyx in gouty habits . . 105
Pregnancy, Use of Assafœtida in . 266
Pregnancy, Albuminuria in . . 277
Pregnancy, Auscultation in . . 495
Pregnancy, Signs of . . 391, 532
Pregnancy, Menstruation during . 393
Psoriasis in Gouty Habits . . 105
Puberty, Female, influence of factory labour on . . . 383
Pulmonary Consump., Scudamore on 351
Pulse, frequency of in the aged . 523
Pupil, Action of the . . . 507
Pyrosis, Seymour on . . . 98
Radius, Fracture of lower end of the, 143
Ramollissement, microscopic examination of 298
Receuil de Memoires de Medecine . 49
Respiration, frequency of, in the aged, 523
Rete Mirabile, The . . . 288
Retina, Structure of the . . 330
Sacrum, Eschar over the . . 537
Sciatica, Seymour on . . . 106
Scorbutus, Novellis on . . 61
Scorbutus, Chronic or Apyretic . 63
Scorbutus, Pathology of . . 63
Scorbutus, Acute . . . 64
Scorbutus, Complication . . 65
Scorbutus, Treatment . . 67
Scorbutus, State of the Blood in . 525
Scorbutus, Andral on Fibrine in . 528
Scorbutus, analogy with Typhoid . 529
Scudamore on Pul. Consumption . 351
Searle on Cholera . . . 516
Sensation, Nature of . . . 540
Sex, influence of diseases according to, 250
Seymour, Thoughts on various diseases 97
Shakespeare's Delineations of Insanity 333
Shaw on Bell's Researches . . 249
Shoulder, Congenital Dislocation . 149
Sierra Leone, Topog. and Diseases of 432
Skeleton, Owen on the development of 156
Skeleton, homological and teleological structure . . . 158
Skin, Arsenic in diseases of the . 247
Skin, Treatment of Squamous Diseases of the 267
Small-pox, Ectrotic Treatment . 256
Smith on Fractures near Joints . 137
Solly on the Human Brain . . 285
Soup, Liebig's formula for . . 411
South's Household Surgery . . 240
South's Translation of Chelius . 360
Southam on Ovariotomy . . 121
Spinal Cord, Structure of . . 290
Spinal Cord, relation to Brain . 291
Spine, Lonsdale on Curvature of . 510
Spleen, Wounds of the . . 96
Stallard on Abscess of the Heart . 123
Statistics, Medical, Duncan on . 250
Sterility, Whitehead on Cause and Treatment of . . . 401
Stomach, Wounds of . . . 95
Stomach, Pain of the . . . 98
Stomach, Ulceration of the . . 99
Strangury from Blisters . . 540
Stroud on Physical Death of Christ . 453
Surgeon, Malgaigne on Qualifications, 254
Swan on the Sympathetic Nerve . 237
Sweat, Bloody, Stroud on . . 457
Syphilis, Marchal on treatment of . 57
Tænia, frequency in Algeria . .
Tarsus, Dislocation of the . . 148
Tendons, Painful Crepitation of . 537
Todd's Physiological Anatomy . 326
Tommasini, Biographical Sketch of . 271
Tongue, Papillæ of the . . 328
Tonics in Acute Disease . . 556
Tonsils, Extirpation of the . . 51
Uterus, Glands of the . . 11
Uterus, formation of the decidua of the 12
Uterus, Crosse on Inversion of the . 124
Uterus, Blood-vessels of the . . 280
Uterus, diseases of, after menstruation 387
Uterus, state of in Pregnancy . 391
Uterus, Inflammation of . 397, 404
Uterus, Gonorrhœal Inflammation of, . 398
Uterus, Prolapsus of . . . 400
Uterus, Auscultation of the . . 499
Vagina, Mucus of the . . 378
Vertebra, Owen's Theory of the . 160
Vertebrata, Skeletons of . . 154
Vivisection, Macilwain on . . 518
Vomiting, Seymour on treatment of . 100
Vulva, inspection of . . . 531
Weber on Uterine Glands . . 11
Weber on Placenta . . . 20
Whitehead on Abortion and Sterility . 377
Wounds, union of . . . 50